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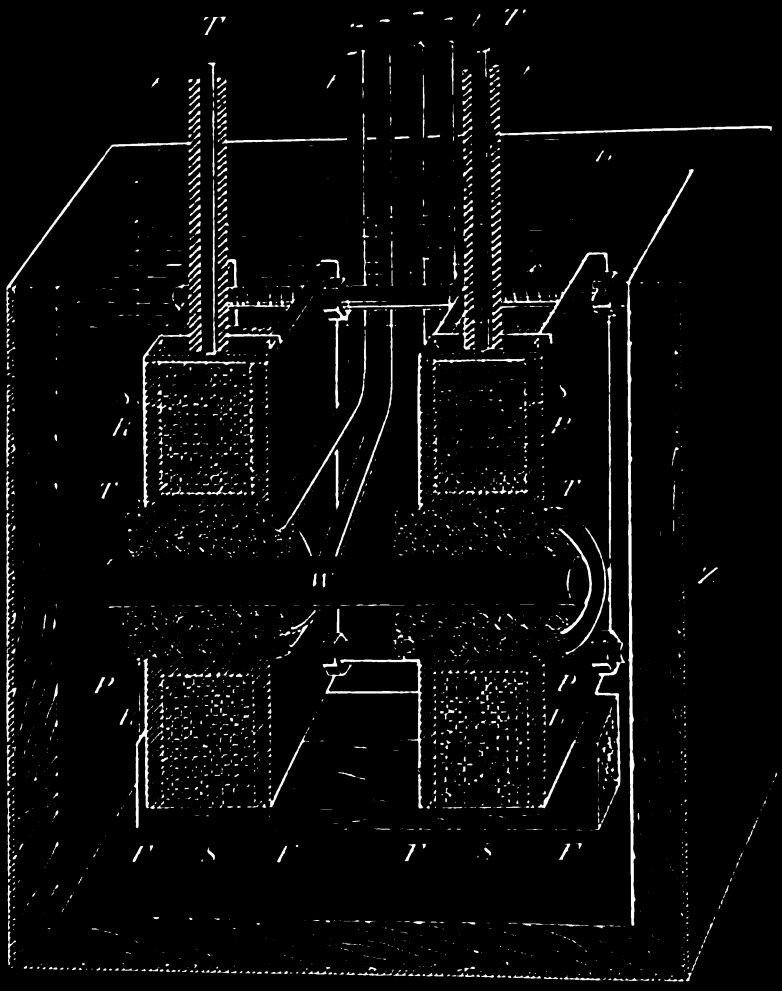

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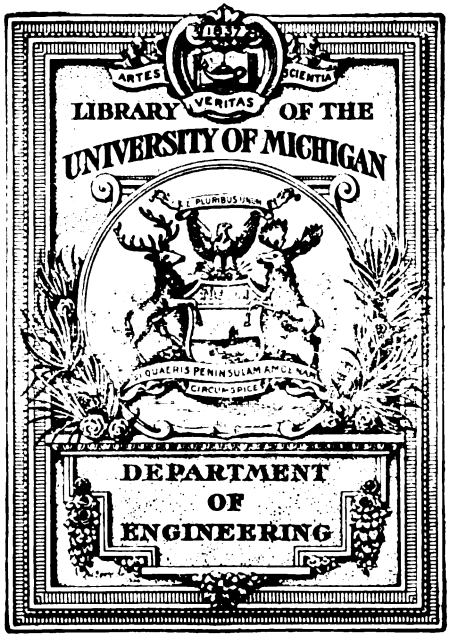

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The Two Hundred and Twenty-eighth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, S.W., on Thursday evening, January 14th, 1892—Professor WILLIAM CROOKES, F.R.S., late President, in the Chair.

The CHAIRMAN: Gentlemen,—In the face of the national calamity which has occurred this day, and the terrible grief into which it has thrown the Royal Family and the whole country, it has been considered by the Council that it would be proper and right for us to adjourn this meeting. The Council have passed the following resolution:—

> "That the Council of the Institution of Electrical Engineers "request the President to convey to their Royal "Highnesses the Prince and Princess of Wales the "expression of the great sorrow with which the mem- "bers of the Institution have learnt of the death "of His Royal Highness the Duke of Clarence and "Avondale, and of the profound sympathy of all "the members with their Royal Highnesses in their "heavy affliction."

Gentlemen, I adjourn this meeting to the 28th of this month.

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The Two Hundred and Twenty-ninth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 28th, 1892 — Professor WILLIAM CROOKES, F.R.S., late President, in the Chair.

The minutes of the Annual General Meeting of December 10th, 1891, and of the Ordinary General Meeting of January 14th, 1892, were read and confirmed.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

H. T. Barnett.	E. T. Mercer.
Hardman A. Earle.	John D. Miller.
R. E. Fletcher.	L. Pyke.
R. C. Jackson.	G. W. Selby.

O. V. Thomas.

From the class of Students to that of Associates—

H. M. Ackery.	A. P. Donnison.
H. W. Bagot.	L. W. de Grave.
J. H. Bouwer.	L. M. Green.

The CHAIRMAN: I announce with regret the death of one of our Honorary Members, whose name is known all over the civilised world—Sir George B. Airy. Sir George Airy has been a member of this Institution for many years, and his loss will be felt widely in all branches of physical science, especially in astronomy, the science in which he principally earned his laurels.

Donations to the Library were announced as having been received from the Astronomer Royal; Captain E. Ironside Bax; Mr. Auguste Bonel; the Director-General of Telegraphs in India; Mr. Félix Lucas; Mr. Henry Wilde; Mr. Latimer Clark and Professor D. E. Hughes, Past-Presidents; Dr. Werner von

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Siemens, Foreign Member; and Mr. John Aylmer and Mr. S. H. C. Hutchinson, Members; to whom the thanks of the meeting were duly accorded.

The CHAIRMAN: I have now a very agreeable duty to perform before I vacate this chair, and that is, to present the prizes to those who have won them. "The Institution Premium," value £10, has been given to Mr. G. C. Lamb, Mr. E. W. Smith, and Mr. W. Woods, for two papers on "Secondary Cells," by Professor Ayrton and themselves. The books and the apparatus are generally selected by the happy candidates. Mr. Lamb has chosen books and a slide rule; Mr. Smith a galvanometer; and Mr. Woods books. I congratulate you, Mr. Lamb, upon the way in which your paper has been appreciated, and I hope you will convey the appreciation of the Institution to your colleagues. The "Paris Electrical Exhibition Premium," £5, has been awarded to Mr. James Swinburne for his paper on "Transformer "Distribution." Mr. Swinburne, in the name of the Institution, I have very great pleasure in presenting you with these books which you have chosen, and I hope we shall before long have other papers, equally valuable, from your pen. The "Fahie "Premium" has been awarded to Mr. P. V. Luke for his paper on "The Early History of the Telegraph in India." This premium will be sent out to him in India. We have not yet heard from him what form he would like it to take.

The last duty which now devolves upon me is to vacate this chair in favour of the new President, my successor, Professor Ayrton. I will not detain you with more words at present, because time is getting on, and we are all anxious to hear Professor Ayrton's Address.

Mr. LATIMER CLARK: It is now, I am sure, a matter of great pleasure to us all to formally present a vote of thanks to our late President on his retirement from the chair. His fame throughout the world is so great that it would be perfectly unnecessary and superfluous for me to dwell upon it; and seeing that we have so much business in hand this evening, I at once proceed to the point, and beg to propose a hearty vote of thanks to Professor

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Crookes for the energetic and able manner in which he has fulfilled the arduous duties of the chair and upheld the dignity of the Institution.

Mr. A. RECKENZAUN: I have much pleasure in seconding the vote of thanks just proposed by Mr. Latimer Clark. It would be difficult to add anything to the praise already bestowed upon Mr. Crookes, and we all know that Mr. Crookes has exercised his Chairmanship over our Society in the most excellent manner possible.

Mr. CROOKES: I feel deeply grateful for the manner in which you have received the vote of thanks, and for the way in which the proposer and seconder have spoken about my past year's work. It is with mixed feelings of regret and pleasure that I leave this chair. Regret, because I feel I have not acted up to the standard that a President of this Institution ought to place before himself. I have been all my life living so much in the rarefied, high and dry atmosphere of pure research, that I have sometimes felt out of touch when listening to some of the papers which have been read here. The work of the President is not entirely confined to taking the chair at these meetings. Much more of the work is connected with committee meetings and general work which must devolve upon the ostensible head of a large Institution like this. The pleasure that I feel in rising is connected with the feeling that I am giving up the chair to so excellent, so typical a President as my successor will be. I must not sit down without alluding to the vacancy you have noticed on my right hand. Mr. Webb, I am sorry to say, is seriously ill with the prevailing epidemic, and we are afraid he will not be able to come back to the Institution for some weeks. I regret this, both for his own sake and for the sake of my successor, for I can assure you Mr. Webb's heart and soul are in the management and the carrying on of this Institution, and I do not think it is easy for a President to feel altogether comfortable unless at the commencement of his office he has Mr. Webb at his elbow to prompt him and suggest what is to be done. I have felt the greatest thankfulness for the way Mr. Webb has smoothed down

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the duties of this office during the evening meetings and at the various meetings of the Council and committees. I am happy to say the Council have this evening passed a unanimous vote of sympathy with him, hoping that he may rapidly recover, and that he will not be too anxious to get back to his duties, for we all feel he ought to take a complete rest for some time. Gentlemen, again I thank you for the way in which you have responded to this vote.

The President then delivered his Inaugural Address.

ELECTROTECHNICS.

By Professor W. E. AYRTON, F.R.S.

I beg to thank you for the great honour you have done me in electing me your President for this year—a year which the need for a new complete index of this Society's Journal marks out as closing the second decade of its life; a year which sees the second thousand added to our roll of members; and a year which the Electrical Exhibition at the Crystal Palace distinguishes as inaugurating the second decade of electric lighting in Great Britain.

It has gradually become the custom for your incoming President to select as the subject of his Address some investigation that has been engaging his attention. Following this custom, I purpose to-night to discuss an experiment in which for the last 19 years I have taken some part—an experiment which of all others has been the one I have had most at heart—and that is, how best to train the young electrical engineer.

To some it may appear that I am treading on well-worn ground; but as the problem is one that is as yet by no means solved, and as it involves the preparation of the machine that is daily used alike by the dynamo constructor, the cable manufacturer, the central station engineer, and the lamp maker—viz., the human machine—the problem of fashioning this tool, so that it may possess sharpness, an even temper, moral strength, and a mental grain capable of taking a high polish, is one that in truth deeply concerns every Member, every Associate, every Student of this Society.

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It is only 15 years ago since I wrote from Japan to my old and valued master Dr. Hirst, then the Principal of the Royal Naval College, Greenwich, asking whether he thought that the time had come for starting in this country a course of applied physics somewhat on the lines of that given at the Imperial College of Engineering in Japan. He replied that England was not yet ripe for such an innovation—an opinion which appeared to be borne out by the fact that after the authorities at University College, London, had in 1878 actually advertised for applications for a new chair of "Technology," they decided that it would be premature to take the responsibility of creating such a Professorship.

But matters were advancing more rapidly than was imagined by collegiate bodies; for in that same year this most valuable report on technical education which I hold in my hand was issued by a committee of the Livery Companies of London, based on the opinions expressed by Sir W. (now Lord) Armstrong, Mr. G. C. T. Bartley, Colonel (now General) Donelly, Captain (now Sir Douglas) Galton, Professor Huxley, and Mr. (now Sir H. Truman) Wood. And although it is 12 years since this book was published, I can recommend it to your notice, for it supplies most interesting reading even at the present day.

Under the guidance of the three joint honorary secretaries, Mr. John Watney, Mr. Sawyer, and Mr. (now Sir Owen) Roberts, the City and Guilds of London Institute for the Advancement of Technical Education started with a name that was very long, but in a way that was very modest, to develop a "Trades School" in accordance with this report. They borrowed some rooms, but for use in the evening only, from the Middle Class Schools in Cowper Street, Finsbury, and decided to erect ultimately a chemical laboratory in that neighbourhood.

But neither the building of a physical nor even of a mechanical laboratory formed any part of the scheme for this "Local Trades School." For at that time the teaching of the practical applications of physics to industry hardly existed, and certainly not its application to any electrical industry other than telegraphy. To make a start, however, in such teaching was most desirable,

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and, therefore Dr. Wormell, the enlightened head master of the Cowper Street Schools, consented to give up the use of some rooms not merely during the evening, but also during the day, to enable Dr. Armstrong and myself to carry out our plan of fitting up students' laboratories with a small amount of apparatus kept permanently ready in position.

For the devotion of these rooms to the carrying out of this new experiment we must always feel grateful to Dr. Wormell, for it was necessarily accompanied by a reduction in the size of his school, and consequently by a pecuniary loss to himself.

The first laboratory course of the City and Guilds Institute was then advertised, and on January 9th, 1880, three students presented themselves—a little boy, a grey-haired lame man, and a middle-aged workman with emphatic, but hazy, notions about the electric fluid.

In order to further utilise these rooms the Institute sanctioned laboratory teaching during the day, and one of the cellars of the Cowper Street Schools was borrowed in order to fit up a gas engine, coned shafting, and a transmission dynamometer, obtained out of the funds of the Institute; an A Gramme dynamo, lent by Mr. Sennett, then one of the students; and two arc light dynamos for transmission of power experiments, lent by the Anglo-American Brush Corporation, whose cordial interest in the work of the City and Guilds Institute has been marked throughout. And, as these dynamos were used, not for electric lighting, but as laboratory instruments for educational purposes, England can claim to have been one of the first in the field of teaching electrotechnics.

Rapidly grew these electrotechnical classes; soon the temporary laboratories in Cowper Street were overcrowded, especially as applied mathematics and mechanics, under Professor Perry, were added to the subjects taught; the £3,000 which had been set aside for the building of this "Local Trades School" grew into £35,000, thanks to the combined donations of the Drapers' Company and of the Institute, and in 1881 was laid the foundation stone of the present Finsbury College.

During the many years that Professor Perry and I were linked

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together, the work of either was the work of both; but now I wish to take this opportunity of acknowledging my personal debt of gratitude for the fund of suggestion which he put forth regarding the teaching of science through its practical applications—the key-note of true technical education. The value of these suggestions you will fully appreciate, for they form the basis of those characteristic and attractive lectures familiar to so many of you who have been his pupils.

As we have seen, then, the present Finsbury College grew out of the "Local Trades School," and formed no part of the original scheme of the Institute. And it was because London was really in want of practical laboratory teaching about dynamos, motors, electric lamps, and engines, and because that want was supplied in a form suitable to the comprehension and to the pockets of workmen in the basement and cellars of the Cowper Street Schools, and last, but by no means least, because one of the executive committee of the Institute, Mr. Robins, strenuously exerted himself to further technical education in Finsbury, that the various electrical, physical, and mechanical laboratories now in Leonard Street, Finsbury, came into existence.

But the establishment of a Central Technical Institution "for "training technical teachers, and providing instruction for "advanced students in applied art and science," had been recommended in all the reports sent in to the committee of the Livery Companies by the six authorities to whom I have referred. So that in the same year that the foundation stone of the Finsbury College was laid by the late Duke of Albany, that of the Central Technical Institution was laid by the Prince of Wales.

And, if you will allow me to say so, the success of the latter institution has been no less marked than that of the former, for, in spite of the rather stiff entrance examination, the number of students who attend all four of the departments at the Central Institution is more than threefold what it was five years ago. In fact, in the mechanical and electrical engineering departments there are already about as many students under instruction as class room and laboratory accommodation will admit. Hence this year will see a considerable increase in the amount of

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apparatus and machinery, as well as in the space devoted to dynamos and motors, in Exhibition Road.

While, on the one hand, the rapid growth of the work of the Guilds Institute is no little due to the fact that the latter end of this century has ushered in the electric age of the world; the electrical industry of our country, on the other hand, is no little indebted to the aid so generously given by our City Companies to the teaching of electrotechnics. For the students who during the last 11 years have, for an almost nominal fee, worked in the electrical laboratories at Cowper Street, at the Finsbury College, and at the Central Institution, number several thousands, and nearly every electrical Works, every place giving electrotechnical instruction throughout this country, employs some of them.

The success which these students have thus achieved, through their own ability and exertions, is, I think, in no small measure due to the Institute having so wisely left the teaching it gave untrammelled by any outside examining body, so that it was possible for this teaching to be directed solely to the professional needs of the students, and to be modified from time to time as it seemed necessary.

My hearty thanks are indeed due to the Japanese Government and the City and Guilds Institute, my masters during the last 19 years, for having left my colleagues and myself unfettered liberty to carry on this experiment of finding out better and better ways of teaching the applications of science to industry.

And there need be no fear that with this freedom the teaching will become stereotyped, and gradually cease to deal with the living science of the factory, for, being bound by no code, we are able to vary our methods, our experiments, and our apparatus according to the continually changing conditions of the profession. In order that the Guilds Institute should fulfil its aim, it is absolutely necessary that its teaching should keep pace with industrial progress. Now, even if it were possible for outside examiners, with fixed scholastic notions, to aid in securing this result, would not their efforts be superfluous? for are there not

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you, the employers of labour, to ultimately decide whether the human tool we fashion is, or is not, adapted to your requirements?

. . . . . .

Leaving now the consideration of the direct work of the City and Guilds Institute, including their extended system of technological examinations, at which last year 7,322 candidates were examined in 53 different subjects at 245 different places in Great Britain and the Colonies, the indirect results that have proceeded from the initiative of this Institute are even greater. For while 12 years ago education in applied science in this country was a tender little infant, requiring much watching and support, combined with constant encouragement, to-day Technical Education—with a capital T and a capital E, bear in mind—is a stalwart athlete, the strong man on the political platform, exercising the minds of County Councillors, and actually regarded as of more importance than the vested interests of the publican.

Until quite recently it was the technical education of the young engineer that had to be considered, but now the problem has become a far wider one, for the education of the British workman is being vigorously pushed forward, and I think that it has become incumbent on you—the representatives of the electrical profession—to express your decided opinion as to what this education of the electrical artisan ought to be.

The technical education snowball set in motion 12 years ago by the City Companies has been rolling—nay, bounding forward—so swiftly during the last year or two, that probably some of you have hardly followed it in its rapid growth both in size and speed. £30,000 has been spent on the Polytechnic in the Borough Road, the Charity Commissioners have already endowed this school with an income of £2,500 a year, and it is hoped that before the building is opened, this income will have been doubled. £50,000 has been already promised for the Battersea Polytechnic, the Charity Commissioners having also undertaken to provide this technical school with an income of £2,500 a year as soon as the subscription reaches £60,000; and for the establishment of a polytechnic in the City £50,000 has been set aside out of the funds of the Charity Commissioners, as well as a yearly grant of

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£5,350. Finally, not to speak of polytechnics in North, South, East, and West London, Mr. Quintin Hogg has himself spent £100,000 on the Regent Street Polytechnic; while the Drapers' Company have alone given £55,000 to the technical department of the People's Palace at Stepney, and endowed it with an income of £7,000 a year. And, most recently of all, the Goldsmiths' Company have put on one side nearly a quarter of a million sterling for the land, the buildings, and for an endowment of £5,000 a year in perpetuity, for their Technical and Recreative Institute recently opened at New Cross.

The following table gives an idea of the sort of sums that are being spent on polytechnic education in London, but it does not profess to give the entire amounts that have been devoted to capital expenditure and yearly maintenance, even for the six polytechnics named in the table:—

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CAPITAL EXPENDITURE.		YEARLY ENDOWMENTS.	
POLYTECHNIC, BOROUGH ROAD.			
Already spent	£30,000	Charity Commissioners alone .. .. (Endowment expected to be doubled before opening.)	£2,500
BATTERSEA POLYTECHNIC.			
Already subscribed	£50,000	Charity Commissioners alone	£2,500
CITY POLYTECHNIC.			
Charity Commissioners alone to spend ..	£50,000	Charity Commissioners alone	£5,350
REGENT STREET POLYTECHNIC.			
Spent by Mr. Quintin Hogg	£100,000	Charity Commissioners alone	£3,500
„ Charity Commissioners ..	11,750		
PEOPLE'S PALACE, MILE END.			
Given by Drapers' Company alone ..	£55,000	Drapers' Company alone	£7,000
„ Charity Commissioners alone ..	6,750	Charity Commissioners alone	3,500
TECHNICAL AND RECREATIVE INSTITUTE, NEW CROSS.			
Given by Goldsmiths' Company .. .. (Representing a total expenditure of nearly £250,000.)	£70,000	Goldsmiths' Company	£5,000
Other Contributions to Polytechnics in London by Charity Commissioners	£6,000	Yearly Endowments of Charity Commissioners to other Technical Institutions in London	£3,200
TOTALS FROM THE ABOVE SOURCES ALONE:			
	£379,500		£32,500

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Large as are these sums, they are, however, even small compared with the amount raised by Mr. Goschen's beer and spirit tax, which it has been decided shall be used for the public benefit, and not for the benefit of the publican. The counties and county boroughs of England now receive nearly three-quarters of a million sterling per annum, of which the whole may be devoted to technical education. The majority of the counties and county boroughs propose to utilise this magnificent opportunity and devote to technical education the entire sum allocated to them, while the remainder use at least a part for this purpose. Middlesex and London, however, stand alone, and employ their whole yearly grant of £163,000 for the relief of the rates, on the plea that they consider that the City Companies are well able to look after the technical education of London.*

Besides this spirit duty, 106 towns are levying rates in aid of technical education under the Technical Institution Acts of 1889 and 1891, the number of these towns having increased by 20 in the last seven months, showing how rapidly is this desire for technical education spreading throughout Great Britain.

In addition to the sums contributed for technical education by the City Companies, collegiate bodies, and private persons who have the practical education of the nation at heart, the following represent, as far as I have been able to ascertain, the amounts that it has been already decided shall be *actually* spent, *yearly*, on technical education in England alone, exclusive of Scotland, Ireland, and Wales:—

Received from the Customs and Excise Duties ..	£500,000
„ „ „ Rates	18,046
Given by the Charity Commissioners	20,550
	£538,596

The yearly amounts that will be actually raised under the Technical Instruction Acts will be far larger than the £18,046 stated above, for this represents only the sum of the amounts raised in the very few towns which have already made returns.

* Since this Address was delivered the County Council of Middlesex has decided to assist technical education.

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Hence the total sum to be spent in England alone on so-called technical education amounts to certainly over £600,000 *per annum.*

As the teaching of electrical technology has been started, in some form or other, in nearly every important town in Great Britain, there is no occasion for me to advocate, as I did in this room ten years ago, that a student of electrical engineering should have an education in applied science; but what I desire to most strongly urge on you to-night is, that it is your bounden duty to see that some portion of the vast sum that is about to be spent on the education of the people is used to give such a training to your workmen as shall really benefit your industry. For otherwise there is a great fear that most of the money devoted to electrical teaching will either be frittered away on the natural loadstone, rubbed-amber order of instruction so dear to the hearts of the schoolmen, or on semi-popular lectures describing in a bewildering, sketchy fashion the whole vast field of electrical engineering.

The workmen you employ are of two classes. In the one class is the man who is all day long, say, stamping out iron discs for armature cores, and the boy who, say, feeds the screw-making machine with its proper meals of brass rod. For such work no technical education is necessary: the workers are mere adjuncts to the machines, to be dispensed with as the machines become more and more perfect. Hence, unless the machine-minder has the ambition and the ability to rise to some less mechanical occupation, his activity, if any be left him after a hard day's work, had probably better be spent in effort of a lighter and more recreative character than would alone be necessary to make him a higher class of artisan.

For him the polytechnic variety course of instruction is an inestimable blessing, for he can do a little type-writing, learn violin-playing and modelling in clay, attend an ambulance class, recite a poem, and devote the remainder of his leisure to the piano, botany, sanitary science, reading books and learning how to keep them.

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His general interests will be roused, the human side of his nature developed, and during the evening at any rate he may forget that he is the slave of the Gramme ring or the slave of the electric lamp.

No wonder, then, that within two months of the opening of the Goldsmiths' Institute at New Cross 4,000 members were enrolled.

But your workmen of the other class must, or at any rate ought to, think. Take, for example, the man engaged in wiring houses, whose work is continually changing, and offering small problems to be solved. Here common sense—or uncommon sense, if you prefer it—is of great value, and the work, to be good, *must* be done by a man with a knowledge of principles, and not by a mere machine-minder.

Many joints,—bad joints,—wires laid in cement under mosaic, which cannot be replaced except at vast expense, even although the insulation has rotted away,—parqueterie floors nailed to insulated wire,—switch-boards screwed on to damp walls,—lamp-holders that only make contact when the lamps are twisted askew, —high-class insulated mains terminating in snake-like coils of flexible wire rubbing against metal in shop windows, under shop fronts,—heavy oriental metal lamps hanging from lightly insulated cord—all this would be avoided, if the workmen had been taught to use their brains as well as their hands.

Now, do you think that the teaching necessary for this purpose is likely to be given at the ordinary English polytechnic school? In the case of the Goldsmiths' Institute the electro-technical department has been put under the charge of Messrs. Dykes and Thornton, two diploma students of the Central Institution; and the fact that these men are, in addition, both employed in Messrs. Siemens's works at Charlton leads one to hope that their teaching, at any rate, will breathe the spirit of the factory. And, therefore, if ample funds be forthcoming for keeping the apparatus at New Cross always up to date, so that the meters, the models, the dynamos—not merely now at the start, but three years hence, six years hence—are truly representative of the industry, there will be a fair prospect that the electrical department of the

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Goldsmiths' Institute, although but a fraction of the whole undertaking, may really benefit the electrical workmen in the East End of London.

But my colleagues and I view with considerable apprehension the way, in which the present wide demand for teachers in technical schools is being supplied. Several of our own students, for example, tempted by the comparatively high remuneration that is offered, have become teachers in technical schools immediately on leaving the Central Institution. In many respects they are undoubtedly well qualified; but if they had first spent some time in works before attempting to teach technical subjects, they would have better understood the wants of the persons whom they have undertaken to instruct.

No greater mistake can be made than to think that a student who has distinguished himself at a technical college can dispense with the training of the factory, unless it be the opposite mistake of imagining that the factory training is equivalent to, or even something better than, that given at a modern school of engineering.

It is the province of the manufacturer to turn out apparatus and machinery as cheaply, quickly, and as well made as is possible; it is the province of the technical teacher to prepare the human tool for subsequent grinding and polishing in the works.

And this necessity for the teacher having himself passed through the shops has especial weight when we are dealing with the technical instruction of workmen, for in such a case there are three requirements absolutely necessary—first, knowing how to teach; second, possessing a fair knowledge of scientific principles; and thirdly—and this is perhaps the most important of all—knowing exactly what it is that the particular workman ought to learn in order to help him in his particular trade.

Schoolmasters may have the first two requisites, and so may do valuable work in connection with the variety teaching at a polytechnic; but they are not in touch with the workshop, and therefore, no matter what may be their scholastic attainments, no matter what the extent of their experience in training the

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young, they are not the persons to give the real technical education to workmen.

In addition, then, to the polytechnics, we must have special schools for special industries, where workmen are taught the application of science to their special trades; and everything taught in such a school must be taught as bearing on the particular industry which the school is intended to benefit. A teacher of physics, for instance, must remember that he is not training physicists, but workmen whose use of physical principles will be bounded by their application to their special trade. For the great danger of such teachers is that, carried away with enthusiasm for their own subject, they will not subordinate it properly to the end in view—viz., helping the workman to know what will be useful to him in his work.

Indeed, as Professor Huxley pointed out in his original report to the Livery Companies' committee, "success in any form of "practical life is not an affair of mere knowledge. Even in the "learned professions, knowledge *per se* is of less consequence than "people are apt to suppose. . . . A system of technical educa- "tion may be so arranged as to help the scholar to use his "intelligence, to acquire a fair store of elementary knowledge "which shall be thorough as far as it goes, and to learn to "employ his hands, while leaving him fresh, vigorous, and con- "tent; and such a system will render an invaluable service to "all those who come under its influence.

"But if, on the other hand, education tends to the encourage- "ment of bookishness, if it sets the goal of youthful ambition, "not in knowing, but in being able to pass an examination, "especially if it fosters the delusion that brain work is, in itself, "a nobler or more respectable kind of occupation than handiwork, "and leads to the sacrifice of health and strength in the pursuit "of mere learning, then such a system may do incalculable harm, "and lead to the rapid ruin of the industries it is intended to "serve."

And I venture to think that not merely at technical schools for workmen, but at technical colleges for engineers, it should be ever remembered that the main object of the training is not the

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cultivation of mental gymnastics, but to enable the student to acquire knowledge and habits which shall be professionally useful to him in after life.

"Useful learning usefully taught," would be no bad motto for technical institutions, seeing that those who favour the compulsory teaching of Greek are apparently willing to accept the converse as the motto for the University. For example, Mr. Butcher, in his address delivered at the end of last session at University College, Bangor, said, "We may claim it as a "distinction that in the seats of academic learning, little or "nothing useful is taught;" and in an article in last month's *Fortnightly Review*, congratulating Cambridge on its recent victory over the *barbarian*, Mr. Bury says quite candidly, "Greek is useless; but its uselessness is the very strongest "reason for its being a compulsory subject in the University "course." And he adds, in italics, "*For the true function of a "University is the teaching of useless learning.*"

A few of the County Councils have realised that the real teaching of the application of science to a special industry, which is what the British workman is so much in need of, cannot be given, as well as a host of other subjects, out of limited funds. For example, Bedfordshire has decided to spend its grant of £4,343 mainly on agriculture, market gardening, the straw trade, domestic economy, and industries for women; Cambridgeshire and Cheshire devote themselves largely to the teaching of agricultural pursuits.

But other places aim at issuing vast comprehensive programmes and turning out yearly a mighty array of students, knowing, it may be, the something of everything, but who certainly will not know the everything of something. For example, the Holland division of Lincolnshire has decided, out of only £2,000 a year, to make grants for dairy schools, University extension and art schools, agricultural science, domestic economy, mechanics, commercial subjects, and ambulance teaching; while Bootle, with a yearly expenditure of only the same amount, maintains classes in five commercial subjects, in 16 science and art subjects, in cookery, wood-working tools, as well as four courses of University extension lectures.

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Because a certain building in Regent Street famed for its ghost and its diving bell was years ago named "The Polytechnic," the majority of the new technical institutions which are being established in London at such vast cost are also called "Polytechnics," and will, I fear, give only an English polytechnic course. Now such recreative education, although admirable for those who seek relief from work in the use of their minds, is not generally sufficient for those of your workmen who use their minds in their daily occupation.

It ought, then, to be thoroughly recognised that there is an entirely new problem to be solved, and that the solution of this problem, in so far as it has been worked out at the Finsbury College and at other places giving practical teaching in the evening, must, in the language of the mathematician, be regarded simply as "the singular solution," and not the general solution, of the problem of technically educating the British workman.

Let us gratefully accept the English polytechnics, for they will undoubtedly confer benefit on our country, and all credit be to those who have so generously established them. But do not let us be misled by the similarity between their generic name and that of the German "polytechnicum" into fancying that the recreative courses of the one are equivalent to the serious education given by the other.

Like Oliver Twist, let us ask for more; for, on behalf of the large number of minds already employed in the electrical industry, and on behalf of the still larger number that will in the future be so employed, it is our duty to secure that ample provision be made in this country for the practical teaching of electrotechnics on a scale comparable with that afforded in the technical high schools of Germany and the institutes of technology of the United States.

On the screen you see projected a photograph of the façade of the Technical High School at Charlottenburg (Berlin), which appears extensive and grand; and yet, as you will see from the next photograph, which gives the ground plan, it was only a small portion of the whole building that you were looking at on the first photograph. In fact the first photograph only shows the portion

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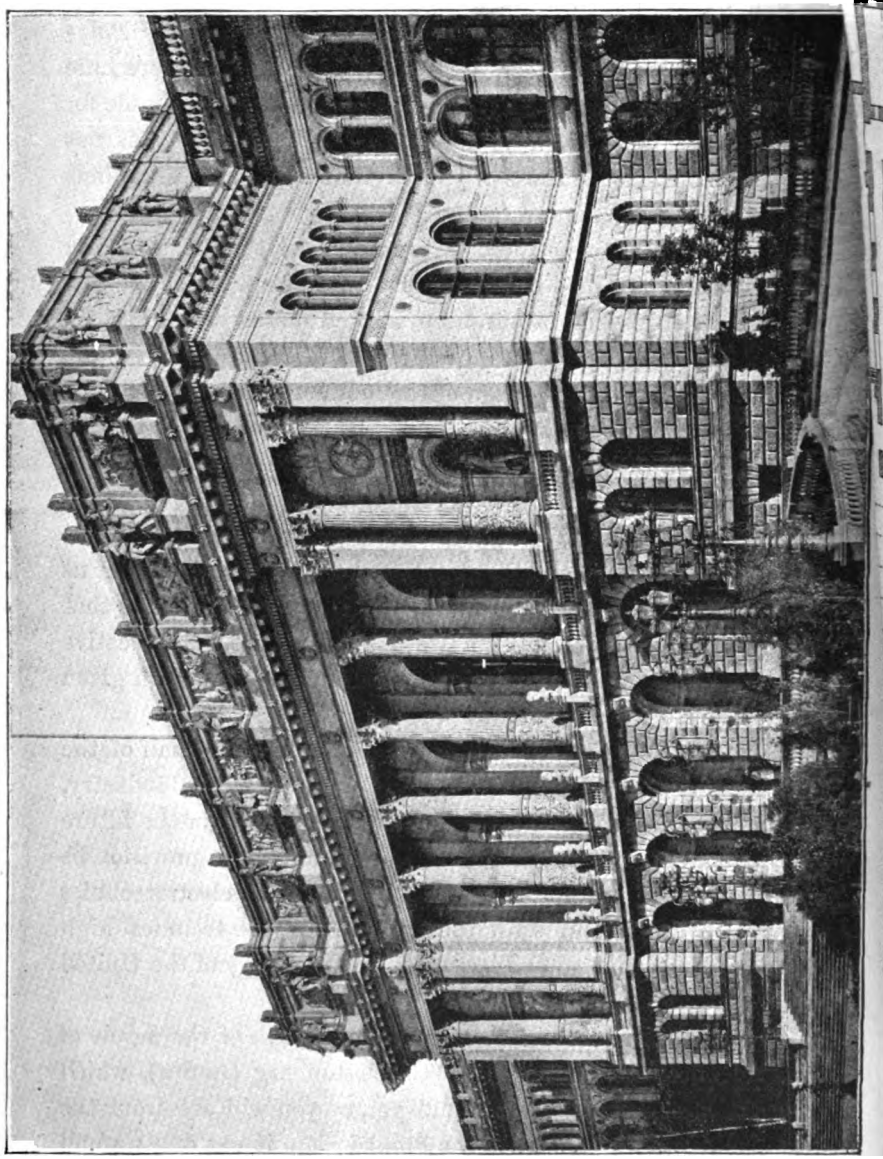

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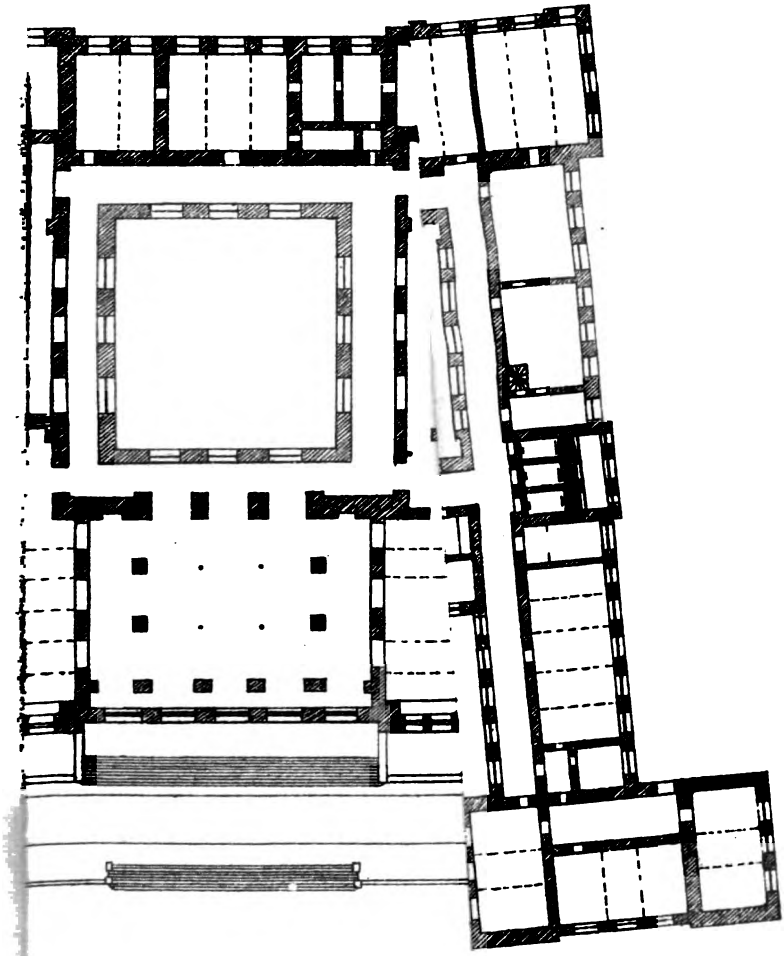

HIGH SCHURG.

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— SCALE OF FEET. —

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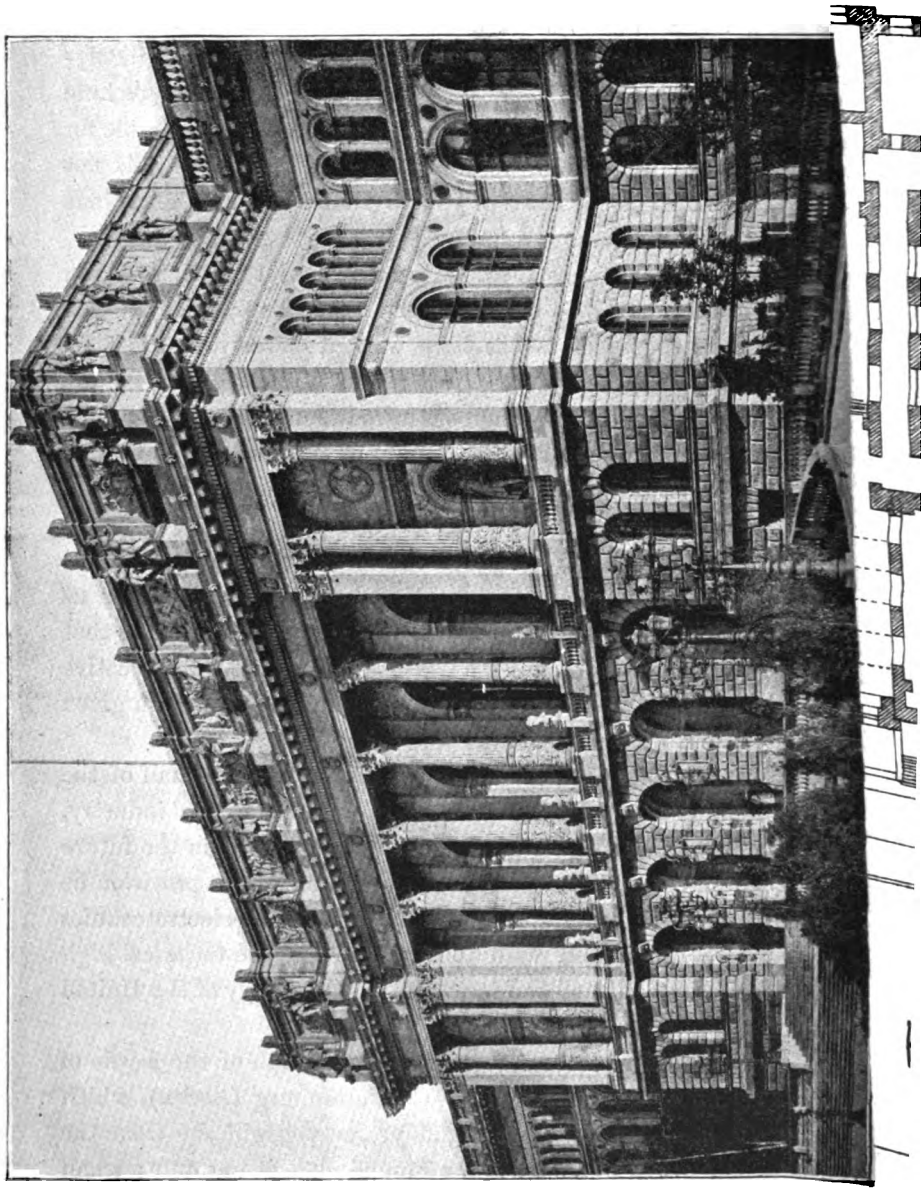

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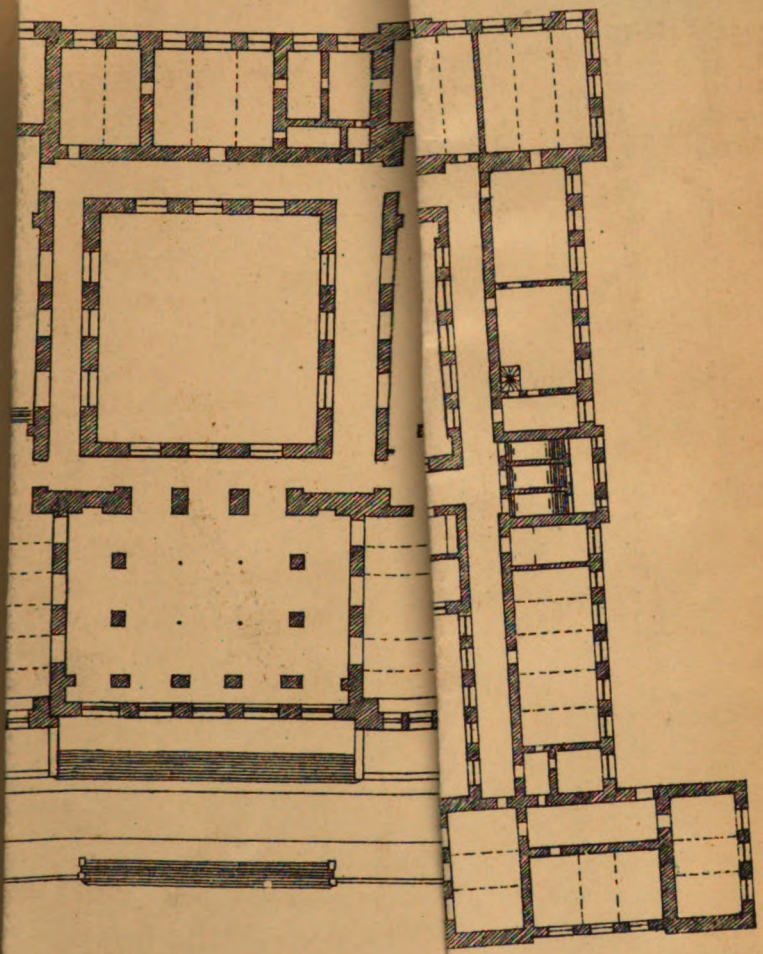

HIGH SCHOOL.

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— SCALE OF FEET. —

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of the building that is contained between the lines *FF*, *FF* in the second.

This is but one of the many technical high schools in different towns of Germany, and yet *it covers an area more than five times as large as that occupied by the Central Technical Institution in Exhibition Road, London, cost four times as much to erect, and has more than four times as much spent on its yearly maintenance.*

The next photograph shows a building devoted wholly to the training of electrical engineers, being that of the Electrotechnical Institution Montefiore, at Liége, which Professor Gerard kindly took me over this last summer, and which has since been opened. When I tell you that there are rooms for small direct-current dynamos, separate rooms for large direct-current dynamos, separate rooms for alternators, and that every three students have a separate little laboratory, with the necessary measuring instruments, all to themselves, your educational mouths will water, as mine did.

We now cross the Atlantic to the Massachusetts Institute of Technology, Boston, which, as you see, consists of several distinct buildings, the centre one being that which contains the electrical laboratories. The dynamo room now seen on the screen has many small and large dynamos in it, and yet there is ample room to walk about, for this dynamo room occupies a space many times as large as that devoted to dynamos at the Central Technical Institution of London.

Professor Cross was so good as to mention in a letter that was shown me some two years ago, that several of the devices that had been worked out for the electrical laboratories of the City and Guilds Institute had been reproduced at Massachusetts; but there is one device that Professor Cross has succeeded in working out, and which I should be most glad to see copied by the City and Guilds Institute, and that is, having one assistant for every five students working in the physical laboratories.

Franklin Hall, presided over by Professor Nichols, is devoted solely to the department of pure and applied physics at the Cornell University, Ithaca. You see how large this four-storied

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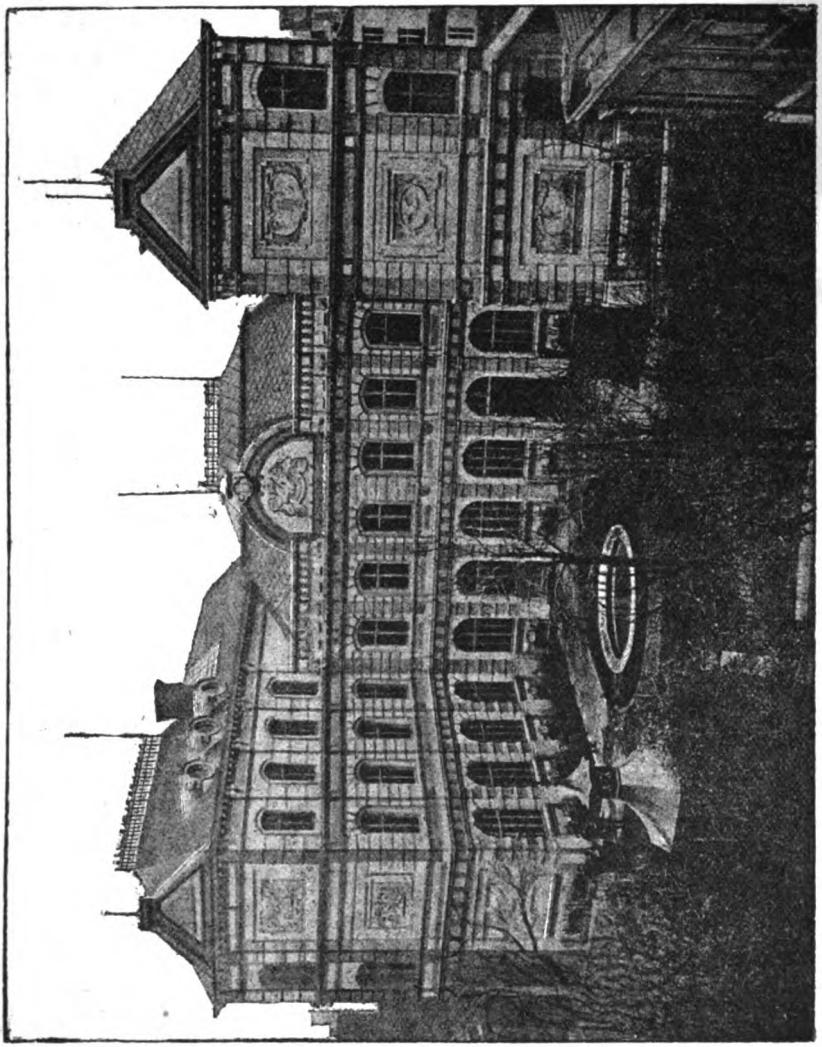

THE ELECTROTECHNICAL INSTITUTION MONTEFIORE, AT LIÉGE.

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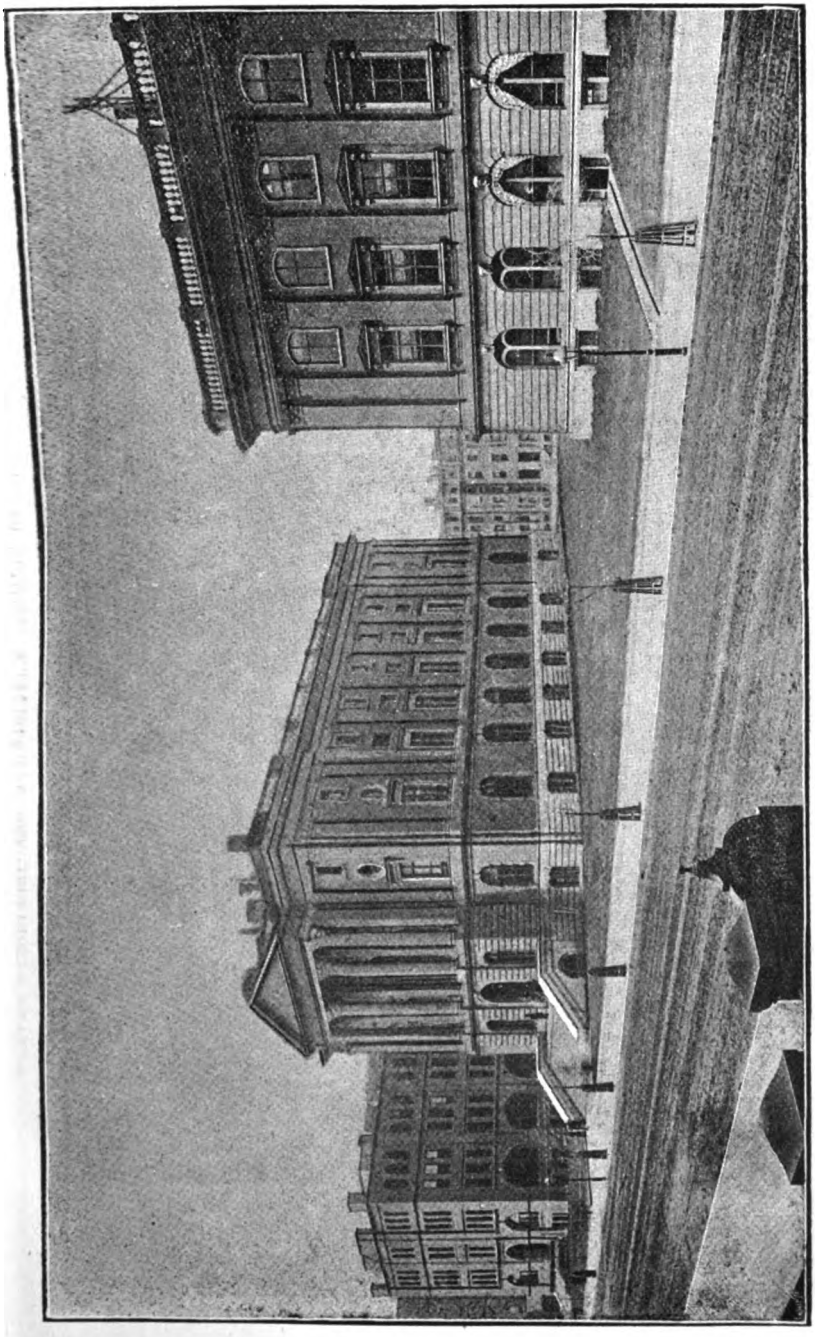

THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY, BOSTON

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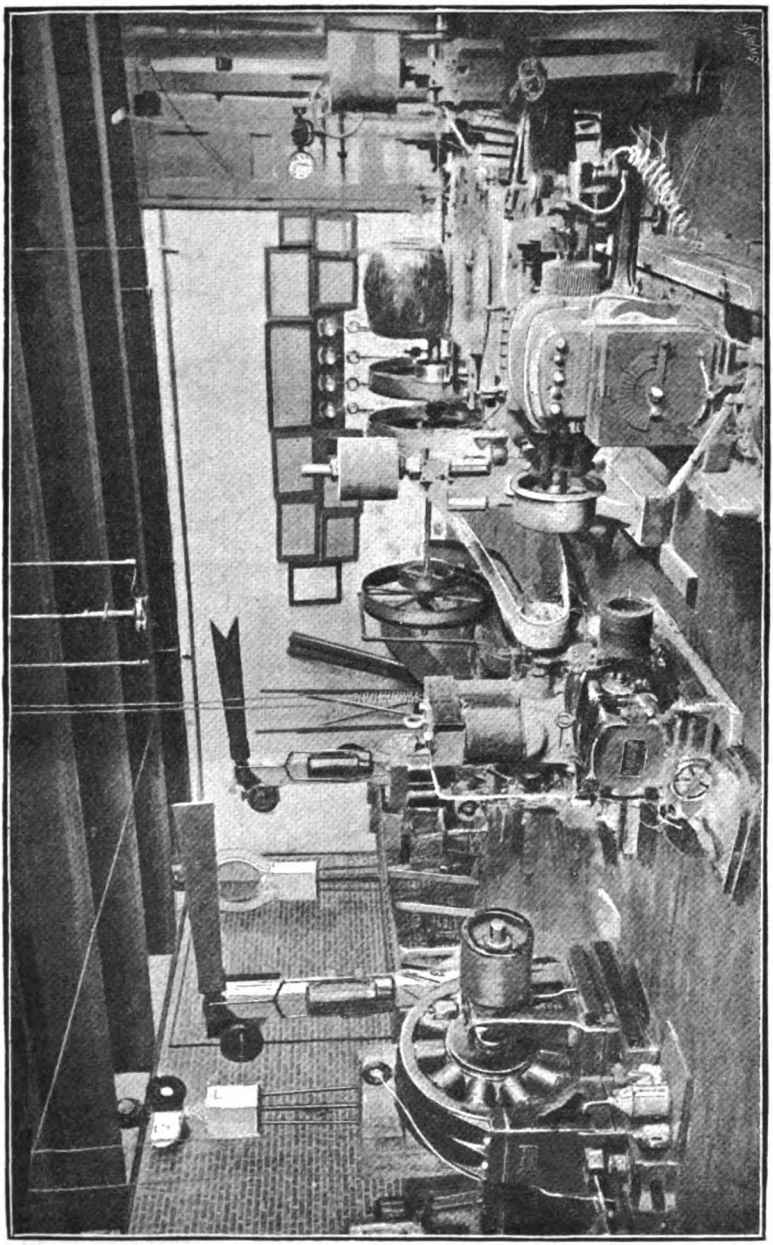

DYNAMO LABORATORY, THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY, BOSTON.

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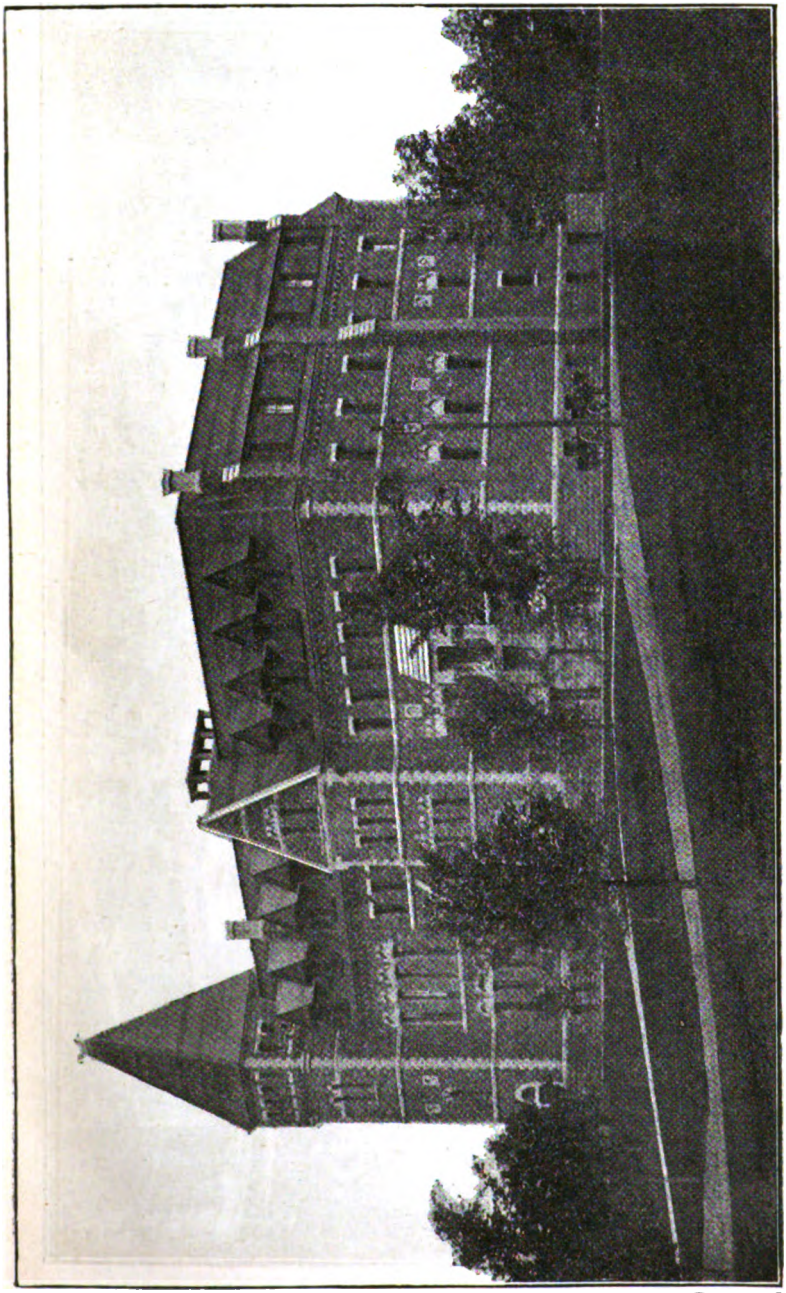

FRANKLIN HALL: DEVOTED TO THE PHYSICAL DEPARTMENT, CORNELL UNIVERSITY, ITHACA.

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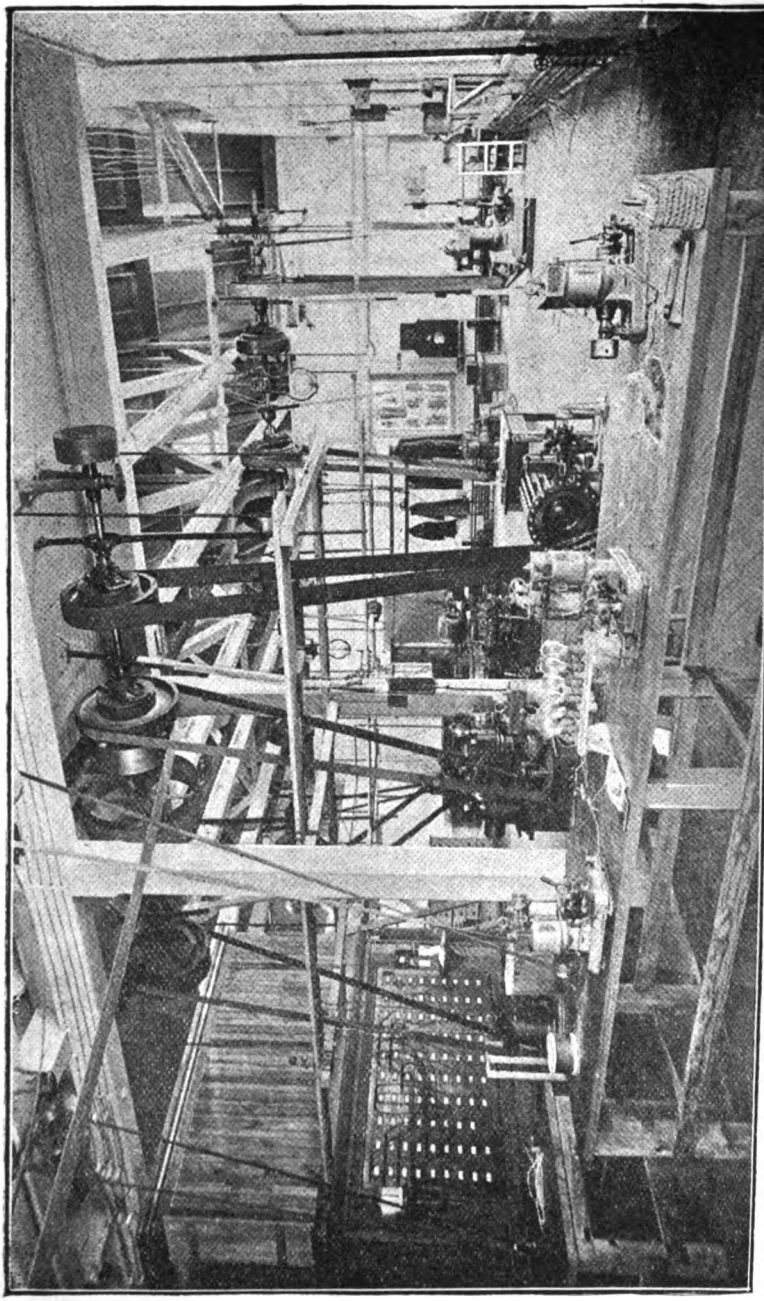

ELECTRICAL LABORATORY, CORNELL UNIVERSITY, ITHACA.

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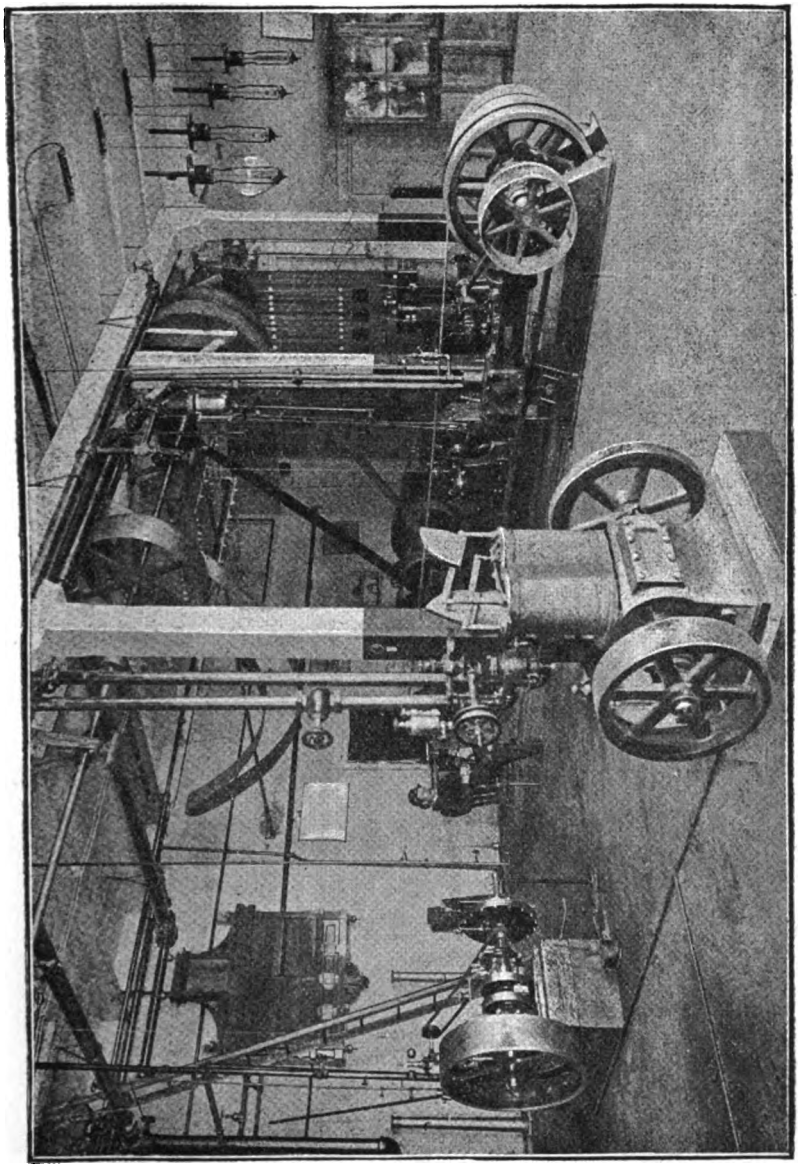

DYNAMO ROOM, CORNELL UNIVERSITY, ITHACA.

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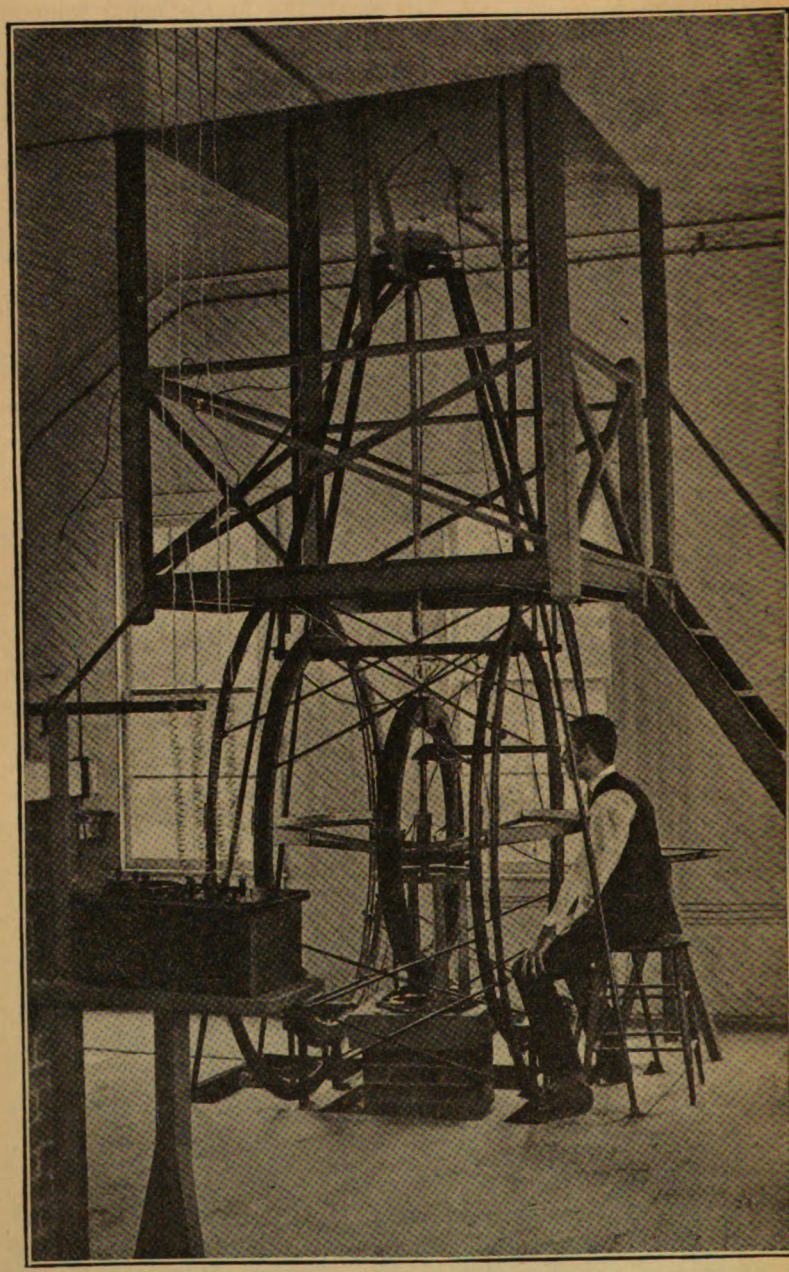

PRINCIPAL TANGENT GALVANOMETER, CORNELL UNIVERSITY, ITHACA.

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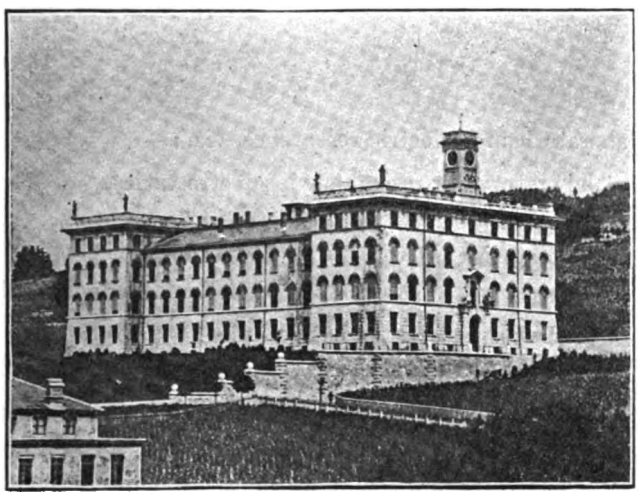
building must be, for look how small the four-wheeled waggon standing in front of it appears.

The next three photographs show some of the provisions made for teaching electrotechnics in Franklin Hall; the electrical laboratory, under Assistant-Professor Moler; the dynamo room, under Professor Ryan, whose analyses of alternate-current curves are well known to you all; and the principal tangent galvanometer.

The most extensive physical laboratories in the world are probably those under Professor Weber at Zurich. When asked by the Swiss Federal Government how much he would require for the building seen in the next illustration, and for its equipment, Professor Weber considered, as he has told me, not what was the



PHYSICAL DEPARTMENT, THE POLYTECHNIC, ZURICH.

smallest sum that would suffice for a makeshift laboratory, but what ought to be spent on the equipment of an ideal set of physical laboratories. He, therefore, asked the Government for two million francs, and they, feeling that, if it were worth while to employ a man like Weber, it was certainly worth while providing him with the physical tools he needed to do his work in the most efficient way, granted this sum. And the result is that up to the present time about £100,000 has been already spent on these new physical laboratories, and on the apparatus contained in them.

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The building seen above contains 52 separate laboratories, of which three are kept artificially at such a constant temperature that the variation does not exceed 0·3° C. throughout the year. These three rooms are employed respectively for very accurate experiments on diffusion, on magnetism, and on electrical phenomena. Among the remaining 49 laboratories there are, so Professor Weber tells me—

5 laboratories for experiments on heat,
5 „ „ „ „ light,
2 „ „ „ „ radiation;

3 laboratories for exact electrical measurement, viz. :—

1 laboratory for exact measurement of electric resistance,
1 „ „ „ „ „ „ current,
1 „ „ „ „ „ „ P.D. and capacity;

5 laboratories for electric researches, viz. :—

1 laboratory for researches in electrostatics,
1 „ „ „ „ varying currents,
2 laboratories „ „ „ induction,
1 laboratory „ „ „ E.M.F. and analogous subjects;

15 laboratories for electrotechnics, viz. :—

3 laboratories for experiments on the electric light,
1 laboratory, with a floor space of 2,153 square feet, containing 12 dynamos,
2 laboratories for accumulator measurements,
2 „ „ alternate-current measurements,
2 „ „ experiments on submarine cables,
1 laboratory „ calibrating ammeters,
1 „ „ „ voltmeters,
1 „ „ „ wattmeters,
1 „ „ testing the magnetic qualities of iron;
&c., &c.;

the preceding only giving an account of the uses of 38 out of the 59 rooms contained in the building seen above.

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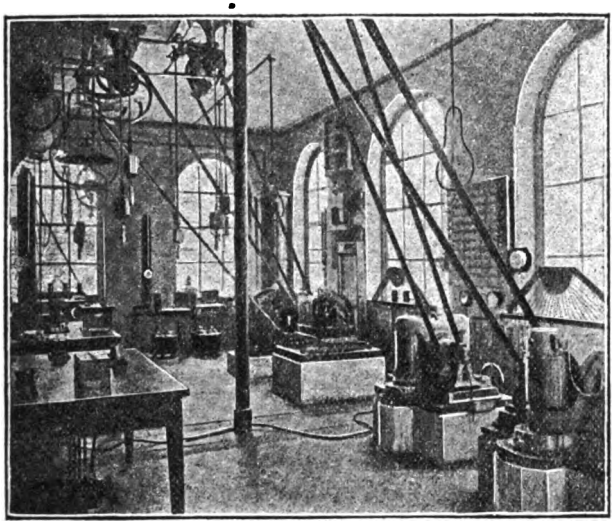
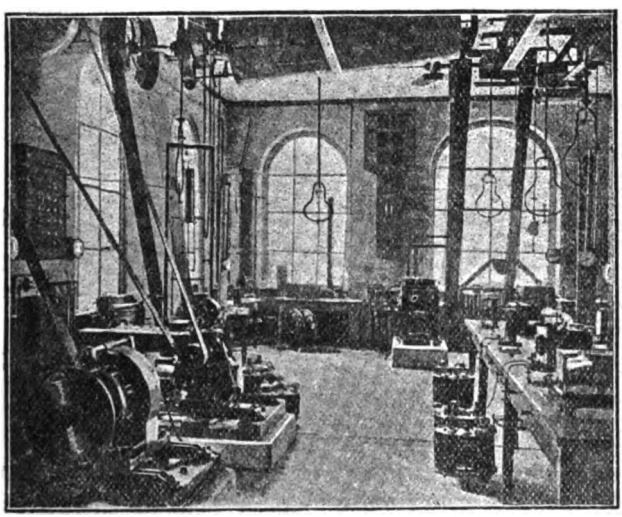
The following show various views of the dynamo room :—



DYNAMO ROOM, THE POLYTECHNIC, ZURICH.



DYNAMO ROOM, THE POLYTECHNIC, ZURICH

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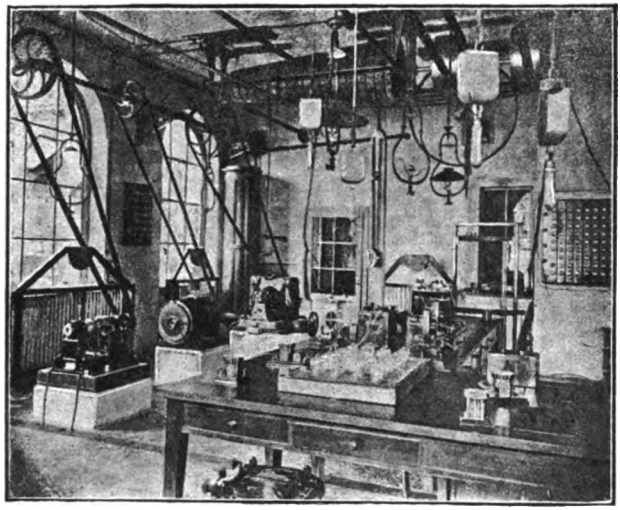

DYNAMO ROOM, THE POLYTECHNIC, ZURICH.

I might show you photographs of many other institutions used for the training of electrical engineering students. In fact, my choice of magnificent Continental and American laboratories has been so great that I have hardly known which to select as specimens.

But there is one thing I cannot show you—and it must remain for the exercise of your influence as representatives of the electrical profession to make that possible—the British electrotechnical laboratories for education and research which are truly worthy of London, the capital of the world.

The training of such students as those at the Central Institution must, of course, differ essentially from that of the electrical artisan, not because we, or the students, expect that on entering a factory at the conclusion of their college course they will start, as a rule, much above the bottom of the ladder, but because they hope in time to be able to mount higher.

They are, therefore, taught, not merely to construct meters and motors, use dynamos and engines, build a chimney and lay a street main, but, as they are not to spend all their lives wiring houses or watching a central station voltmeter, they are well

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practised in calculating and designing, and they further obtain sufficient acquaintance with the methods of attacking new problems not to be daunted when they meet with them in after life.

But so strong is becoming our belief in the value of science to the manufacturer, so anti-classical are some of us growing, that there is great risk that the literary side of the education of an electrical engineer will soon be wholly neglected. Now, important as it no doubt is for him to be quite at home with electrical apparatus and machinery, it is no less important, if he is to take advantage quickly of the progress made abroad, that he should be able to read a German or a French newspaper. I do not merely mean that with a grammar and dictionary, and plenty of leisure, he should be able to translate the newspaper, sentence by sentence, like a schoolboy preparing to-morrow's lesson, but that he should have the power to glance down the columns, gather the gist of the articles, and quickly see whether there be anything new that especially concerns him.

How many electricians are there in this country who can, for example, take up the *Zeitschrift für Instrumentenkunde* or the *Electrotechnische Zeitschrift* and look through their pages as they do those of the *Electrician*, the *Electrical Review*, or of the *Electrical Engineer* during breakfast on Friday morning? There are, I know, a few—I wish I were one of them.

And yet examples are not wanting of the scientific isolation that is caused by not possessing that familiarity with foreign languages which is such a characteristic of diplomatists and hotel waiters. Take, for instance, the fact that, whereas manganin was manufactured on a commercial scale in Germany, and German resistance coils have for the last three years been constructed of this material with a temperature coefficient of nearly zero, the very existence of this alloy was unknown to many English electrical instrument makers a few weeks ago; and even now many of them are still unacquainted with the composition of manganin, and its peculiar properties, as well as with the results of the extensive and striking experiments that have been carried out at the Reichsanstalt at Charlottenburg on the temperature coefficient

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and specific resistance of all sorts of manganin-copper-zinc-nickel-iron alloys.

This Physikalisch-Technische Reichsanstalt, I may mention, is an establishment totally distinct from the Technical High School in Charlottenburg, some photographs of which I showed you this evening. The Reichsanstalt is not an institution with students, but a vast series of Imperial laboratories, presided over by Professor von Helmholtz, solely used for carrying out researches in pure and technical physics. The investigations are conducted under the direction of Dr. Loewenherz, aided by 46 assistants.

We have no establishment in Great Britain at all comparable with this Reichsanstalt. The original work turned out there in electrotechnics alone is considerable. Here are some of the published accounts of researches immediately bearing on your profession which Dr. S. Lindeck has been so kind as to send me: "Hardening Steel Magnets," "Standard Resistance Coils for "Large Currents," "Tests of Commercial Ammeters and Volt-"meters," "Mercury Standard of Resistance," "Photometric "Investigations," "Compensation Apparatus for Use in P.D. "Measurements," "Alloys for Resistance Coils," and so on.

Surely it is part of the technical education of the electrical engineer to be taught how to read such pamphlets as these with comparative ease.

A working knowledge of French and German can be obtained without the necessity of learning to express oneself fluently in epigrammatic French, or to imitate with facility the word-building of a native German; and with such a working knowledge the average technical student may rest content. But as regards his own language he should aim at something higher, and therefore the electrical engineering students of our country should be, I would urge, practised in writing—yes, and also in speaking—vigorous English.

Only the other day, Professor Nichols, of the Cornell University, was deploring with me the rarity of finding a student of electrotechnics who could write a decent report. The experimental methods employed in the student's investigation might have been good, the mathematical analysis suitable, and the

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calculations exact; but the description of the apparatus and of the results obtained would be scattered pell-mell over the paper, as if the writer were quite ignorant of the fact that the style in which a dish is served up is nearly as important as the goodness of its ingredients.

Why do you suppose that Huxley's portrait has nearly as much prominence given it in the photographer's window as that of a duke or a ballet dancer? Quite as much because he knows how to express himself in terse and forcible English as on account of his wide scientific knowledge; because even when writing about dry bones the flow of his language clothes them with rounded forms.

But, you will ask, how are we to find the time for all this linguistic and literary polish? has the electrical student of to-day so many spare hours that fresh subjects of study must be sought for to fill up his leisure moments?

At present much time has to be wasted at technical and other colleges teaching students 16 years or older elementary mathematics and science, which ought to have been mastered before that age. When the education of childhood is improved, when the higher education of women is properly carried out, there will be no need for male experts to trouble about general training, for then children will spend less time at school and learn more: boys and girls will as a matter of course acquire the foundation of modern languages and general education; and students at a college will be able to devote their whole time to the special training—scientific, manual, linguistic, and literary—which pertains to the particular profession which their special tastes will generally have led them to select before the age of 16.

And just as methods of teaching applied science have been developed during the past few years, so I look forward to the growth of new methods of teaching what may be called applied literature. For it seems to me that there is a want of breadth in the view that because the study of Greek verse would be unprofitable for a student of electrotechnics, and because he has neither the taste nor the time to enter into the intricacies of

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etymology and grammar, therefore the study of modern languages and literature, even as directly applicable to his profession, should form no part of his regular training.

As well might it be thought (and I am sorry to say this view is not yet quite exploded) that because a student has neither the taste nor the time for the study of abstract mathematics, therefore he should be debarred from all work in a physical laboratory. Well, if it be generally accepted that although a young electrical engineer has no chance of becoming a Cayley or a Maxwell, still he ought to be taught such portions of mathematics and physics as will be directly useful to him in his profession, why should the certainty that he will neither become a Jebb nor a Dickens lead us to tolerate an inability on his part to speak fluently and write tersely his own language, surpassed only by his entire ignorance of every other?

Habits of scientific thought are highly necessary for electrical students; to be masters of their own language, and to know something of one or two others, are, I venture to think, no less so; but the main result to be achieved, the main object to be aimed at, with every system of education, is moral thoroughness.

For until every workman, foreman, engineer, and manufacturer feels regret and pain at seeing work inefficiently performed, our national system of education will be incomplete.

All the labour now expended in watching work in progress, and in testing it when completed to see that it has not been scamped, is so much withdrawn from the real business of production. Every rise, therefore, in the standard of thoroughness of a community means the saving of waste labour. But far greater than this will be the actual increase in the productive power when each gives his best endeavours to his share of the world's work. And greatest of all will be the gain in the nation's happiness, since he who works with his whole soul knows no drudgery.

The lesson to be taught is no new one—it was set many centuries ago; and hundreds of thousands a year will be well spent if the County Councils can succeed in bringing home to the hearts of us all this—"Whatsoever thy hand findeth to do, do it "with thy might."

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Dr. SILVANUS THOMPSON: I have been most unexpectedly requested to be your mouthpiece in moving a vote of thanks to Professor Ayrton for the Address with which he has favoured us. I would rather that the task, pleasant as it is, had fallen to some other lips than mine, because I am a sort of colleague of Professor Ayrton—his successor, indeed, in one of his most successful experiments; and, treading in his footsteps to the best of my ability, it hardly becomes me to praise him for what he has done. Professor Ayrton has set before us in his own inimitable way that which he has been attempting to do in aid of the industry that more than any other depends upon the progress of science—the industry with which we are all concerned—an industry which is growing faster than any other—an industry the growth of which can only be measured by the growth of the science that underlies it. Now, if he has taken us abroad—to the Continent, to the shores of America—if he has shown us what our rivals in commerce are doing on their part to try and wrest from us the supremacy in the electrical world, it is our own fault if we do not thank him most heartily for the lesson. He has the happy art, which is not the possession alone of a Huxley, to clothe the dry bones of science with the rounded forms of language. Amongst the many good things which fell from his lips, there is one for which I, thanking him personally, and in thanking him on your behalf, will venture to reiterate, for it most thoroughly expresses the feeling we have—we who are endeavouring to shape the tool for your use. It is this: that, whatever the means at our disposal, or whatever the means that are lacking, for shaping the human tool for use in your industry, it is you who are our masters, it is you to whom we have to look to know whether we teach the future engineers what they ought to be taught, and whether they are taught in the right way. No matter what the curriculum, however carefully it may be brought up to date, if we fail, it is because we have not come up to what you want. Tell us what you want, and we will do our best to fulfil your needs. Professor Ayrton has set this forth so well, I must not dwell upon it. In tendering him our hearty thanks, I ask him, in your name, to allow his Address to be printed in the usual form, and published amongst the Proceedings of this Institution.

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Mr. ALEXANDER SIEMENS: Having to speak after two eminent Professors, I hope you will allow me to be as brief as possible. The words which Dr. Thompson has said—that it is the employer's tool which is to be shaped in these institutions,—that they ought to say what they want—should also be borne in mind by all our members. I do not think it would be at this time right to enter upon the subject very fully, but I want to put quite shortly, in a few words, radically different ideas from what Professor Ayrton has put before us; and that is: he has been alluding in a disparaging way to the study of ancient languages, but if you reflect a little you will find that they are, to put it very strongly, essential for an electrical student. What do we employers want? We want a young man who has got a knowledge of his subject, who knows thoroughly the theoretical principles which are underlying our profession; we want a young man who is able to follow the literature of foreign countries, as Professor Ayrton so correctly said; we want a young man who can put properly before us what he has seen and heard and read. But we want also a young man who can distinguish chaff from corn; and I believe that this result can best be achieved by training the mind for strictly logical consideration of all subjects. To do this for a young boy—with whom you ought, of course, to begin—a thorough knowledge of Latin and Greek is very useful, because he is made to consider very well what he has to say. He is taught not to rush rapidly into a form of a word, but to consider well what he is putting down. That is a very useful habit to acquire. The same study will enable him to deal with quite a number of things, in the way Professor Ayrton indicated that he can take up newspapers and look at them without a dictionary, see what they amount to, and, if necessary, study an article if he finds it worth looking at. He must, however, be thoroughly grounded in the principles of physics and mathematics, so that he can follow the reasoning of technical and scientific papers that he sees, and that he can judge for himself whether any novelty which he himself would like to introduce is really worth pursuing or not. If the general principles were thoroughly taught, and very little attention were given to details, and the time which,

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as Professor Ayrton said, was so precious to students were more devoted to general principles, and not too much to the making of galvanometers and such things, then the employers would be much better pleased. I hope you will now allow me to second the motion of Professor Thompson that Professor Ayrton be requested to have his Address printed and published in the Transactions of the Institution.

Sir DAVID SALOMONS: I have the permission of the President to say a few words. No man is more interested in Technical Education, or has been, in many ways, more connected with the movement in recent years, than myself. I can therefore fully appreciate the able Address of our President. He has, however, not mentioned the name of a gentleman who has done so much for the cause of Technical Education, and for this omission I attach no blame to Professor Ayrton, for this gentleman is so modest and retiring in nature, that few know how much valuable work he has done. He is one of us, and his name is known to most of us—Henry Cunynghame. He was appointed assistant secretary of the Charity Commission in order to assist the matter of Technical Education. Being an intimate friend of mine, I am able to speak from personal knowledge of his good work, never resting day or night in the cause. Nearly, if not all, the funds now placed at the disposal of Technical Education in London, as shown you by the President, have been obtained through the efforts of Mr. Cunynghame, by looking up all misapplied funds. This is a great feat to accomplish, and we shonld feel proud to possess such a man as one of ourselves. These remarks must of themselves be their own apology for having intruded upon your time.

Mr. CROOKES: It has been proposed and seconded that a hearty vote of thanks be given to your President for the Address he has favoured us with to-night, coupled with the request that he will allow it to be published in the *Journal*.

The motion was carried by acclamation.

The PRESIDENT: I have to make a special announcement, which some of you will anticipate, namely, that we are to have the pleasure of receiving a most interesting lecture from Mr.

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Tesla next Wednesday evening, February 3rd; and partly because we anticipate a very large attendance, and partly because Mr. Tesla will require special facilities for his experiments, his lecture will, by the kindness of the Managers, be delivered at the Royal Institution.

The following candidates were declared by the scrutineers to be duly elected:—

Foreign Member:

Edwin Wilbur Rice, jun.

Members:

Francisco K. Cassels.
Frederick Sargent.

Associates:

Darwin Bates.
William Bates.
Stewart Nicholas de Saumarez Brock.
William Brown.
Fourd Ely.
Arthur Yates Gahagan.
Herbert Furnell Laughton.
Henry Nicoll.
Harold King Smith.

Student:

Harry Butler.

The meeting then adjourned.

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ABSTRACTS.

ALCOCK'S PHASE-INDICATOR FOR SYNCHRONISING.

(*Lumière Electrique, Vol.* 41, *p.* 609.)

A horizontal disc is attached to a vertical rod hung on knife edges, and connected to the new machine requiring to be joined up. Close under this disc is fixed a second one in connection with the one already running. When the two are in step, the two discs vary in potential concurrently and the hanging one is repelled, the amount of repulsion being shown on a scale. When the repulsion is the same as when both discs are attached to one machine, a mercury contact is made joining the new machine up with the one already on.

THE GULCHER AND PINTSCH THERMOPILE.

(*Lumière Electrique, Vol.* 41, *p.* 478.)

Twelve or more blocks of copper are arranged radially round a centre, forming a tube, through which the flame of a Bunsen burner passes. To the back of each of these pieces of copper (which are insulated from each other) are connected the ends of plates of antimony-zinc alloy and nickel; the former being cast on to the copper, and the latter brazed. The outer end of the nickel is connected to the outer end of the antimony of the next couple, and so on round the ring; the series being—Cold Sb-Ni, hot Ni-Cu, hot Cu-Sb, cold Sb-Ni, &c. Large copper plates are attached to the cold junctions to facilitate radiation. Several of the rings thus built up are placed one over the other, separated by asbestos, which is the insulating material used throughout; the flame of the burner passing through the tube thus formed. The whole is in an outer case, through which a draught of air is induced by joining it to the outlet of the central tube into the chimney.

THE WATER-POWER STATION AT DOVER, U.S.A.

(*Elektrotechnische Zeitschrift, Vol.* 12, *No.* 27, *p.* 361.)

The generating station is situated on the banks of the Salmon, some seven miles from the town of Dover, and also supplies current to Rochester and East Rochester, which are at a similar distance, and to three other towns somewhat nearer, besides running 12 15-H.P. cars on a tramway of about seven miles. The motive power is obtained from a 500-H.P. turbine, and in case of a breakdown or shortness of water two 250-H.P. engines have been erected as a stand-by. There are nine 50-lighter Thomson-Houston arc light machines, one 140-unit alternator for glow lamp circuits, and three machines—210 H.P. total—for the tramway, all worked direct except the alternator, which is belt-driven

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Dr. DENSLER—THE SESSACH-GELTERKINDEN STREET RAILWAY.

(*Electrotechnische Zeitschrift*, *Vol.* 12, *p.* 504.)

The line is 3¼ kilometres long, with one intermediate station, and runs principally on the public streets and roads. The sharpest gradient is about 1 in 70. The two sharpest curves are 60 metres, and one of these, being on a gradient of 1 in 85, is the part of the line which requires more power than any other. The power is obtained from a Jonval turbine giving about 40 H.P. at 98 to 100 revs. The dynamo is a series one and runs at 600 revs., giving 700 volts and 50 amperes. The current is taken off by two pairs of copper gauze brushes, and it is found that there is much less injurious action on neighbouring telephone wires than when plate brushes are used. The load being very variable, and the cost of power negligible, the velocity of the dynamo is regulated by a brake. The dynamos are carefully protected from lightning, the guards consisting of carbon points set opposite one another and momentarily snatched apart by an electro-magnet to break the arc when the machine current follows a discharge. This apparatus has so far always worked most satisfactorily. The current is taken by the cars by an overhead arm, from a hard-drawn copper rod ¼ in. diam., which runs the whole length of the line, and is joined every 100 yards to a second insulated conductor carried on the same posts. Both conductors are on oil insulators. The rolling stock consists of one locomotive, four carriages, and four goods waggons. The locomotive has two drum armature four-pole 25-H.P. motors with radial carbon brushes, and will run in either direction. The axle of the driving wheels runs loose through the base of the motor, and is connected to the spindle of the latter by gearing. The motor is kept approximately vertically over the axle by a strong spring, which, however, gives enough to prevent jars at starting. The lighting has to be done by oil, as the E.M.F. is very variable.

P. BACHMETJEW—SOME PHENOMENA IN PERMANENT MAGNETISM.

(*Exner's Repertorium*, 27, *p.* 147, 1891; *Beiblätter*, *No.* 6, 1891.)

The experiments were on straight and also on ring magnets, and the changes of magnetism were measured by a coil round the magnet joined to a ballistic galvanometer. The magnets were excited by a current round another winding. From the results it appears that the effects are not the same in the two forms.

The normal magnetism, M_n, of a straight electro-magnet is the mean of the two induction currents, C and C_1, obtained by making the current in one direction and then reversing it; so that $M_n = K \times \frac{1}{2}(C + C_1)$, and the permanent magnetism $M_p = K \times \frac{1}{2}(C_1 - C)$, where K is a constant. The normal magnetism for a ring magnet, however, is expressed by the formula, $M_n = K \times \frac{1}{2}(C + 2C_1)$. Its permanent magnetism when it vanishes seems to cause only one-half the effect that it should.

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H. L. CALLENDER—THE CONSTRUCTION OF PLATINUM THERMOMETERS.

(*Phil. Mag., Vol. 32, No. 194, p. 105.*)

Contrary to what is generally supposed, the author finds that, when properly constructed, this form of thermometer is very trustworthy, and not liable to the changes of zero which occur in the Siemens form, and which he attributes to the fact that the wire is in that pattern wound on a clay cylinder, which attacks the metal at high temperatures. The author winds a fine platinum wire, preferably non-inductively, on a thin sheet of mica, fuses or welds the ends on to stout leads, and encloses the whole in a tube. For temperatures below 700° C. the tube may be hard glass, and the leads silver or copper. For rough work below 1,000° C. a wrought-iron tube and iron leads may be used, but for accurate tests platinum leads and a glazed porcelain or silica tube are necessary. The leads are insulated by passing through holes in a series of mica wads fitting in the tube. To allow for the resistance of the leads (which, if thick, are cumbersome and costly, and, if thin, are liable to introduce errors, as their temperature is unknown) the author runs a pair of exactly similar wires from the bridge to the tube, and down to the bottom of the latter, where they are joined together. These wires vary in resistance, therefore, exactly as the leads to the coil do, and by introducing them into the "R" of the bridge (using equal ratios) the lead resistance is accurately compensated. The formula generally used is that of Siemens—$R = \alpha T^{\frac{1}{2}} + \beta T + \gamma$; but the author has found Benoit's simpler formula, viz., $R \div R_0 = 1 + \alpha T + \beta T^2$, to be much preferable. The following advantages of this form of thermometer may be pointed out:—The range is from absolute zero to above 1,500° C; whereas a mercury thermometer is only available between − 40° and + 400°. A mercury thermometer will alter its zero 10° or 20° if kept at 350° for a week, besides being subject to erratic variations depending on its past history, so that it is impossible to obtain results, even with the best instruments, accurate to 0·1° in 200°. A properly made platinum one, when once annealed, which is done in a few minutes, will not vary its zero 0·01° when used at any temperatures up to 500° C. And the author has used some up to 1,300° C., and not found an alteration of more than 0·1°. There is no correction to be made for the amount of the stem not immersed, which may in a mercury thermometer lead to considerable errors. And, lastly, the scale may be made very open. The author uses a slide wire as part of the "R" arm of his bridge for the final adjustment, and readings of 1 cm., or even more, per degree can easily be arranged for.

C. FROMME—MAGNETIC INVESTIGATIONS.

(*Wiedemann's Annalen, Vol. 44, No. 9, p. 138.*)

The author in his former papers (cf. previous abstracts) investigated the abnormal phenomena in permanent magnetism depending on the manner in which the current in the magnetising solenoid was broken—whether quickly or slowly. In the present paper he inquires how far these effects are due to the presence or absence of induced currents in the iron, or oscillatory currents at break in the solenoid. He first applied a strong magnetising current, and then slowly reduced

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it, repeating this till the permanent magnetism (P.M.) was constant. He then applied a somewhat smaller force, also slowly reduced to zero, and noted the change in P.M. The whole was then repeated, but the magnetising spiral was in the case of the small current short-circuited before the battery circuit was broken, thus giving a quick break, but without inducing currents in the iron core, as the magnetising current remained always in the same direction, when the short-circuit was applied, till it died out. Enclosing the core (of iron wires in paraffin) in a brass tube made no difference. In every case the change in P.M. was different with quick and slow break, and the author concludes that quickness of break is the primary cause of the difference. Somewhat different results were obtained with solid iron and steel cores, but amalgamating iron wires and filling the tube containing them with mercury had no effect. The author considers that all the phenomena can be explained on the theory of molecular magnets, capable of rotation; the probability of the truth of which theory is therefore strengthened.

W. SHRADER—THE TERMINAL RESISTANCE OF A SMALL CONDUCTOR WITH VERY LARGE ENDS.

(*Wiedemann's Annalen, Vol.* 44, *No.* 10, *p.* 222.)

When, as in mercury resistance standards, the resistance consists of a small tube of mercury terminating in receptacles of mercury of very large section compared to a, the cross section of the tube, a correction, $n\,a$, has to be added to the length to allow for the spreading out of current when it leaves the tube. Maxwell has calculated that the value of n lies between 0·785 and 0·849. Rayleigh (*Proc. Lond. Math. Soc.*, 1875-76) showed that n was less than 0·8242, and believed that it was very near to that figure. The author, by actual experiments, makes $n = 0{\cdot}805$. The only previous experimental determinations are, he believes, those of Mascart, who found $n = 0{\cdot}794$, and Kohlrausch, who made it 0·789.

The author's method was to immerse in a mercury bath a horizontal glass tube about 0·002 inch internal diameter, and having vertical ends of large diameter. The tube and bath were filled with mercury until the level was the same inside and out. The resistance between amalgamated platinum plates in the two vertical ends was then taken, being that of the capillary tube plus the two end contacts, &c. The tube was then broken in the middle, under the mercury, and the resistance again taken with ends at a considerable distance apart. This gave the former resistance plus the "spreading-out resistance,"* where the current leaves each end of the broken tube and spreads out into the (practically infinite) mass of mercury all round. The resistance increased as the tubes were drawn further and further apart, until their ends were about 1 cm. from each other, but after that it remained practically constant. The mean value of n from a large number of these experiments was 0·804. Another series of similar determinations was then made with $KHSO_4$, which was chosen as having a very low temperature coefficient. Its resistance was measured by alternating currents and telephone, and the mean of the results was $n = 0{\cdot}805$. The tubes used were 6 and 8 mm. diameter—say 0·24 in. and 0·32 in.

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* Ausbreitungswiderstand.

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F. GÉRALDY—AN ATTEMPT AT A SIMPLE THEORY OF MOTORS WITH ROTATING FIELD.

(*Lumière Electrique*, *Vol.* 40, *No.* 27, *p* 1.)

The simple machine with rotating field consists, as is well known, of two magnetic fields at right angles to each other, of which one is excited at any instant by a current $C = k \sin \alpha$, where k is a constant, and the other by a current $C = k \cos \alpha$. In the machine, Fig. 1, when the current in A and B is $k \sin \alpha$, that in C

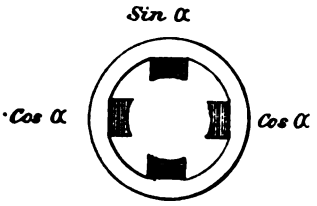

FIG. 1.

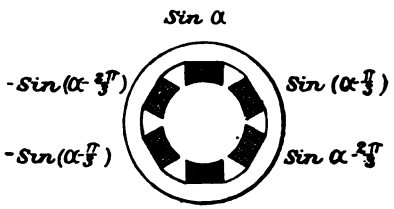

FIG. 2.

and D is $k \cos \alpha$, or $k \sin \left(\alpha - \frac{\pi}{2}\right)$; *i.e.*, the current in C and D lags 90° behind that in A and B. Now interpose between A and B, instead of one, two other pairs of coils at angles of $\frac{\pi}{3}$, or 60° (Fig. 2), and let the currents in them lag to that extent: *i.e.*, at any instant

the current in A and B is $C_1 = k \sin \alpha$;

that in C and D is $C_2 = k \sin \left(\alpha - \frac{\pi}{3}\right)$;

that in E and F is $C_3 = k \sin \left(\alpha - \frac{2\pi}{3}\right)$

Now use a common return for C_1 and C_3. The current in this will be

$$C_1 + C_3 = k \left[\sin \alpha + \sin \left(\alpha - \frac{2\pi}{3}\right)\right];$$

simplifying this, we have,

$$C_1 + C_3 = k \sin \left(\alpha - \frac{\pi}{3}\right),$$

or exactly the current required for the remaining pair of coils; so by winding them with this return wire we have the three sets of coils with the required lag of 60° between each, and only requiring three wires. The Hutin and Leblanc machine (Fig. 3) is practically a multipolar form of Fig. 1. The circumference of the field magnets is divided into some multiple of 4; and two alternating currents are used, the one lagging 90° behind the other. The one current passes round A, and may be expressed as $\sin \alpha$; it then passes in the opposite direction round B, and has therefore the same effect as a current of $-\sin \alpha$. Thence it passes to the winding of A_1, wound in the same direction as A, and so on round the circle. In the same way the second alternating current passes round C and D in opposite directions, and may be written as $\cos \alpha$ and

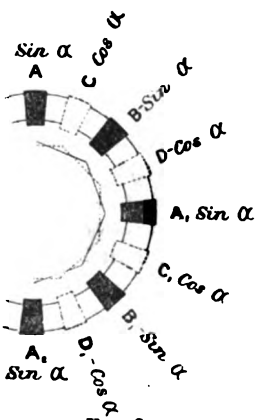

FIG. 3.

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$-\cos\alpha$ in the two respectively. So that when A (and every fourth pole from it) is N., C is neutral, B is S., and D is neutral; then A becomes neutral, C is N., B neutral, and D is S., and so on. By the use of condensers MM. Hutin and Leblanc get two currents differing 90° in phase from the two leads of an ordinary alternating circuit. The armature consists of two closed circuits, each having half as many poles as the armature. Suppose the field does not rotate—*i.e.*, excite the magnets, by direct currents, to the same state in which they simultaneously are at some particular moment with the alternating current. Now rotate the armature (say) counter clockwise: a current will be set up in it, alternating once for every time an armature pole passes four magnet poles; these currents being, of course, in such a direction as to tend to cause revolution in a clockwise direction. Fix the armature and revolve the field magnets bodily, clockwise, and the effect is the same. But the effect of this last is exactly the same as that of a clockwise revolving field with alternating current, which, therefore, also causes clockwise rotation, the limiting velocity of rotation being $\frac{N}{n}$, N being the $\sim$ per second of the driving current, and n one-fourth the number of poles in the field-magnet circuit. The author concludes with mathematical calculations (without calculus) on torque, power, speed, &c.

Dr. J. PULUJ—MEASUREMENT OF COEFFICIENT OF SELF-INDUCTION BY ALTERNATING CURRENT AND DYNAMOMETER.

The author uses as a source of alternating current a horse-shoe electro-magnet with two windings; the one of low resistance, 0·16 ω, traversed by the current from a couple of accumulators and a fine wire secondary circuit of 0·700 ω. In front of the poles he rotates an iron wheel carrying a number of soft iron pole-pieces, thus obtaining alternating currents in the secondary winding of his magnet. The speed is kept constant by using a heavy driving wheel and keeping the note of the machine in unison with that of a pitch-pipe or tuning-fork. The dynamometer used is a very low reading one of the Siemens type, the moving coil being spherical and having an iron core. An inductionless resistance is put in series with the coil whose λ is to be measured and the alternating current passed through them. The dynamometer is then joined first across the terminals of the coil and then across those of the resistance, and λ of the coil is obtained from a long expression in terms of the two deflections, the λ of the dynamometer, the number of $\sim$, and the various resistances. The author gives fully worked tests on two coils of 1·2 × 0·08 secohms—by calculation, by the Raleigh method, and by his own. The three methods agree within one or two per cent.

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LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Months of DECEMBER, 1891, and JANUARY and FEBRUARY, 1892.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

C. DEHLMANN—The Breslau Central Station (I.).—*E. T. Z.*, vol. 13, No. 1, p. 1.

A. BOUCHET—The Water-Power Installation of the Electro-chemical Co., Vallorbes, Switzerland (I.).—*Lum. El.*, vol. 43, No. 1, p. 13.

— SALADIN—Electrical Matériel in Mines.—*Bull. Soc. Int.*, Dec., 1891, p. 885.

— AUBERT—Meter (I.).—*Lum. El.*, vol. 42, No. 50, p. 525.

— KUMMER—Dynamo (I.).—*Lum. El.*, vol. 43, No. 2, p. 67.

D. DUJON—Life of Glow Lamps.—*Lum. El.*, vol. 43, No. 5, p. 205.

Arc Lamps.

CANNEVEL	*Lum. El.*, vol. 43, No. 1, p. 19.
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FERGUSON and CEUTER—Variation of Light in Glow Lamps with Current and E.M.F.—*E. T. Z.*, vol. 13, No. 7, p. 89; *Lum. El.*, vol. 42, No. 51, p. 577.

M. KUGEL—Local Action in Negative Secondary Battery Plates.—*E. T. Z.*, vol. 13, No. 1, p. 8, No. 2, p. 19.

MAGNETISM.

S. HENRICHSEN—Magnetic Properties of Organic Compounds.—*Ann.*, vol. 45, No. 1, p. 38.

A. ABT—The Magnetic Properties of Magnetite.—*Ann.*, vol. 45, No. 1, p. 80.

S. KALISCHER—The Behaviour of Iron Membranes in a Periodically Varying Magnetic Field.—*E. T. Z.*, vol. 13, No. 5, p. 59.

T. MOUREAUX—Value of the Magnetic Elements, Jan. 1, 1892.—*C. R.*, vol. 114, No. 1, p. 31.

E. MARCHAND—The Effect of Neighbouring Thunderstorms on a Registering Magnetometer.—*C. R.*, vol. 114, No. 1, p. 29.

C. DECHARME—Experiments on various Methods of Magnetising Steel.—*Lum. El.*, vol. 43, No. 4, p. 155.

C. DECHARME—Effects of Heating One Part of a Permanent Magnet.—*Lum. El.*, vol. 43, No. 6, p. 258.

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TELEGRAPHS AND TELEPHONES.

P. MARCILLAC—The Marseilles Postal Telegraph Office.—*Lum. El.*, vol. 43, Nos. 1 and 2.

E. MÜLLER—The Construction and Testing of Relays (German Post Office).—*E. T. Z.*, vol. 12, No. 51, p. 697.

SIEMENS and HALSKE—Tape Machine.—*Lum. El.*, vol. 43, No. 5, p. 209.

SEITZ and LINHART—Telegraph.—*Lum. El.*, vol. 43, No. 3, p. 123.

ANON.—Subterranean Telephone Leads in Hamburg.—*E. T. Z.*, vol. 13, No. 3, p. 32.

W. LAHMEYER—Running Wires near Lighting Leads.—*E. T. Z.*, vol. 13, No. 3, p. 31.

W. CHRISTIANI—Twisted Overhead Telephone Wiring.—*E. T. Z.*, vol. 12, No. 51, p. 687.

C. GRAWINKEL—Remarks on the above.—*E. T. Z.*, vol. 13, No. 2, p. 22.

— BERTHON—Spring Jack (I.).—*Lum. El.*, vol. 42, No. 50, p. 537.

INSTRUMENTS AND MEASUREMENTS.

A. ELSAS—A New Method of Determining S.I.C.—*Ann.*, vol. 44, No. 12, p. 654.

MAX. WIEN—The Optical Telephone for Current Measurement: The Determination of S.I.C. thereby.—*Ann.*, vol. 44, No. 12, pp. 681, 689.

A. ELSAS—Resistance Measurement with Telephone.—*Ann.*, vol. 44, No. 12, p. 666.

R. A. LEHFELDT—Galvanometric Method of Measuring H.—*Phil. Mag.*, Jan., 1892, p. 78.

F. STENGER—The Back E.M.F. of the Arc.—*Ann.*, vol. 45, No. 1, p. 33.

— CARDANI—Absolute Capillary Electrometer.—*Lum. El.*, vol. 43, No. 6, p. 286.

A. FRANKE—Use of the Quadrant Electrometer in Investigations on Electric Waves in Conductors.—*Ann.*, vol. 44, No. 12, p. 713; *Lum. El.*, vol. 43, No. 6, p. 285.

F. BRAUN—Absolute Electrometer for Lecture Purposes.—*Ann.*, vol. 44, No. 12, p. 771.

Dr. M. EDELMANN—Torsion Galvanometer.—*E. T. Z.*, vol. 12, No. 51, p. 689.

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RIGHI—Discharges consisting of Moving Luminous Clouds (I.). Cf. *Jour. I.E.E.*, 1891, p. 626.—*Lum. El.*, vol. 42, No. 50, p. 501, *et seq.*

WESENDONCK—Electrification by Friction of Gases on Metals.—*Beibl.*, vol. 15, No. 11, p. 773.

E. BRANLY—Loss of Charge under the Influence of Rays of High Refrangibility.—*C. R.*, vol. 114, No. 2, p. 68.

O. LEHMAN—On the Discharge of a Large Influence Machine.—*Ann.*, vol. 44, No. 12, p. 642.

P. HOHO.—Two Lightning Strokes.—*Lum. El.*, vol. 43, No. 5, p. 217.

Prof. Dr. NEESEN—A Remarkable Lightning Stroke.—*E. T. Z.*, vol. 12, No. 51, p. 694.

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J. KLEMENCIC—The Reflection of Electric Radiation by Plates of Sulphur or Metal.—*Ann.*, vol. 45, No. 1, p. 62.

H. HERTZ—The Passage of Radiation from the Cathode through Metallic Films.—*Ann.*, vol. 45, No. 1, p. 28.

— GOUY—Electro-capillary Phenomena; Contact Differences of Potential.—*C. R.*, vol. 113, No. 1, p. 22, No. 5, p. 211.

A. GRAY—Calculation of λ of Coils; Absolute Electro-Dynamometers.—*Phil. Mag.*, Jan., 1892, p. 62.

J. BROWN—Theory of Electrolytic Conduction of Clausius; On some Recent Evidence for the Dissociation Theory of Electrolysis.—*Phil. Mag.*, Jan., 1892, p. 82.

F. BRAUN—Electro-capillary Reactions.—*Lum. El.*, vol. 43, No. 1, p. 41.

A. BLONDEL—Experiments on the Alternating-Current Arc (S.).—*Lum. El.*, vol. 42, No. 51, p. 551, *et seq.*

RAILWAY APPLIANCES.

F. GÉRALDY—The Electric Tramways of Paris.—*Lum. El.*, vol. 43, No. 6, p. 251.

ANON.—The Berlin Electric Underground Railway.—*Lum. El.*, vol. 42, No. 51, p. 581.

THOMSON-HOUSTON—Tramway System (I.).—*Lum. El.*, vol. 43, No. 3, p. 108.

ANON.—The Halle Electric Tramway.—*Lum. El.*, vol. 43, No. 4, p. 179.

— SIEMENS—Block Signal for Single Line Railways.—*Lum. El.*, vol. 43, No. 4, p. 181.

VARIOUS.

— GREFEN—Electric Lock (I.).—*Lum. El.*, vol. 42, No. 49, p. 454.

— BARLOW—Burglar Alarm System (I.).—*Lum. El.*, vol. 43, No. 1, p. 27.

A. SPRUNG—Automatic Registering Apparatus (I.).—*Lum. El.*, vol. 43, No. 4, p. 178.

ANON.—Fire Alarm Apparatus (various) (I.).—*Lum. El.*, vol. 42, No. 49, p. 457.

— SIEMENS—Range-Finder.—*Lum. El.*, vol. 42, No. 49, p. 459.

NICHOLS and SNOW—Character of Light emitted by Incandescent Zinc Oxide.—*Phil. Mag.*, Jan., 1892, p. 19.

E. MÜLLER—Vulcanised Fibre.—*E. T. Z.*, vol. 13, No. 6, p. 72.

E. BERG—Ship's Telegraph.—*E. T. Z.*, vol. 13, No. 2, p. 24.

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JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXI. 1892. No. 97.

An Extraordinary General Meeting of the Institution was held at the Royal Institution, Albemarle Street, on Wednesday evening, February 3rd—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The following lecture was delivered by NIKOLA TESLA, Foreign Member, on

EXPERIMENTS WITH ALTERNATE CURRENTS OF HIGH POTENTIAL AND HIGH FREQUENCY.

Ladies and Gentlemen,—I cannot find words to express how deeply I feel the honour of addressing some of the foremost thinkers of the present times, and so many able scientific men, engineers and electricians, of the country greatest in scientific achievements.

The results which I have the honour to present before *such* gathering I cannot call my own. There are among you not a few who can lay better claim than myself on any feature of merit which this work may contain. I need not mention many names which are world-known—names of those among you who are recognised as the leaders in this enchanting science; but *one*, at least, I must mention—a name which could not be omitted in a demonstration of this kind. It is a name associated with the most beautiful invention ever made: it is Crookes!

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When I was at college, a good time ago, I read, in a translation (for then I was not familiar with your magnificent language), the description of his experiments on radiant matter. I read it only *once* in my life—*that* time—yet every detail about that charming work I can remember this day. Few are the books, let me say, which can make such an impression upon the mind of a student.

But if on the present occasion I mention this name, as one of many your Institution can boast of, it is because I have more than one reason to do so. For what I have to tell you and to show you this evening concerns in a large measure that same vague world which Professor Crookes has so ably explored; and, more than this, when I trace back the mental process which led me to these advances—which even by myself cannot be considered trifling, since they are so appreciated by you—I believe that their real origin, that which started me to work in this direction, and brought me to them after a long period of constant thought, was that fascinating little book which I read many years ago.

And now that I have made a feeble effort to express my homage and acknowledge my indebtedness to him and others among you, I will make a second effort, which I hope you will not find so feeble as the first, to entertain you.

Give me leave to introduce the subject in a few words.

A short time ago I had the honour to bring before our American Institute of Electrical Engineers some results then arrived at by me in a novel line of work. I need not assure you that the many evidences which I have received that English scientific men and engineers were interested in this work, have been for me a great reward and encouragement. I will not dwell upon the experiments already described, except with the view of completing, or more clearly expressing, some ideas advanced by me before, and also with the view of rendering the study here presented self-contained, and my remarks on the subject of this evening's lecture consistent.

This investigation, then, it goes without saying, deals with alternating currents, and, to be more precise, with alternating currents of high potential and high frequency. Just in how

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much a very high frequency is essential for the production of the results presented, is a question which, even with my present experience, would embarrass me to answer. Some of the experiments may be performed with low frequencies; but very high frequencies are desirable, not only on account of the many effects secured by their use, but also as a convenient means of obtaining, in the induction apparatus employed, the high potentials which, in their turn, are necessary to the demonstration of most of the experiments here contemplated.

Of the various branches of electrical investigation, perhaps the most interesting and immediately the most promising is that dealing with alternating currents. The progress in this branch of applied science has been so great in recent years that it justifies the most sanguine hopes. Hardly have we become familiar with one fact, when novel experiences are made, and new avenues of research are opened. Even at this hour possibilities not dreamt of before are, by the use of these currents, partly realised. As in Nature all is ebb and tide, all is wave motion, so it seems that in all branches of industry alternating currents—electric wave motion—will have the sway.

One reason, perhaps, why this branch of science is being so rapidly developed is to be found in the interest which is attached to its experimental study. We wind a simple ring of iron with coils; we establish the connections to the generator, and with wonder and delight we note the effects of strange forces which we bring into play, which allow us to transform, to transmit and direct energy at will. We arrange the circuits properly, and we see the mass of iron and wires behave as though it were endowed with life, spinning a heavy armature, through invisible connections, with great speed and power—with the energy possibly conveyed from a great distance. We observe how the energy of an alternating current traversing the wire manifests itself—not so much in the wire as in the surrounding space—in the most surprising manner, taking the forms of heat, light, mechanical energy, and, most surprising of all, even chemical affinity. All these observations fascinate us, and fill us with an intense desire to know more about the nature of these phenomena. Each day

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we go to our work in the hope of discovering,—in the hope that some one, no matter who, may find a solution of one of the pending great problems,—and each succeeding day we return to our task with renewed ardour; and even if we *are* unsuccessful, our work has not been in vain, for in these strivings, in these efforts, we have found hours of untold pleasure, and we have directed our energies to the benefit of mankind.

We may take—at random, if you choose—any of the many experiments which may be performed with alternating currents; a few of which only, and by no means the most striking, form the subject of this evening's demonstration: they are all equally interesting, equally inciting to thought.

Here is a simple glass tube from which the air has been partially exhausted. I take hold of it; I bring my body in contact with a wire conveying alternating currents of high potential, and the tube in my hand is brilliantly lighted. In whatever position I may put it, wherever I may move it in space, as far as I can reach, its soft, pleasing light persists with undiminished brightness.

Here is an exhausted bulb suspended from a single wire. Standing on an insulated support, I grasp it, and a platinum button mounted in it is brought to vivid incandescence.

Here, attached to a leading wire, is another bulb, which, as I touch its metallic socket, is filled with magnificent colours of phosphorescent light.

Here still another, which by my fingers' touch casts a shadow—the Crookes shadow of the stem inside of it.

Here, again, insulated as I stand on this platform, I bring my body in contact with one of the terminals of the secondary of this induction coil—with the end of a wire many miles long—and you see streams of light break forth from its distant end, which is set in violent vibration.

Here, once more, I attach these two plates of wire gauze to the terminals of the coil, I set them a distance apart, and I set the coil to work. You may see a small spark pass between the plates. I insert a thick plate of one of the best dielectrics between them, and instead of rendering altogether

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impossible, as we are used to expect, I *aid* the passage of the discharge, which, as I insert the plate, merely changes in appearance and assumes the form of luminous streams.

Is there, I ask, can there be, a more interesting study than that of alternating currents?

In all these investigations, ladies and gentlemen, in all these experiments, which are so very, very interesting, for many years past—ever since the greatest experimenter who lectured in this hall discovered its principle—we have had a steady companion, an appliance familiar to everyone, a plaything *once*, a thing of momentous importance *now*—the induction coil. There is no dearer appliance to the electrician. From the ablest among you, I dare say, down to the inexperienced student, to your lecturer, we all have passed many delightful hours in experimenting with the induction coil. We have watched its play, and thought and pondered over the beautiful phenomena which it disclosed to our ravished eyes. So well known is this apparatus, so familiar are these phenomena to everyone, that my courage nearly fails me when I think that I have ventured to address so able an audience, that I have hazarded to entertain you with that same old subject. Here in reality is the same apparatus, and here are the same phenomena, only the apparatus is operated somewhat differently, the phenomena are presented in a different aspect. Some of the results we find as expected, others surprise us, but all captivate our attention, for in scientific investigation each novel result achieved may be the centre of a new departure, each novel fact learned may lead to important developments.

Usually in operating an induction coil we have set up a vibration of moderate frequency in the primary, either by means of an interruptor or break, or by the use of an alternator. Earlier English investigators, to mention only Spottiswoode and J. E. H. Gordon, have used a rapid break in connection with the coil. Our knowledge and experience of to-day enables us to see clearly why these coils under the conditions of the tests did not disclose any remarkable phenomena, and why able experimenters failed to perceive many of the curious effects which have since been observed.

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In the experiments such as performed this evening, we operate the coil either from a specially constructed alternator capable of giving many thousands of reversals of current per second, or, by disruptively discharging a condenser through the primary, we set up a vibration in the secondary circuit of a frequency of many hundred thousands or millions per second, if we so desire; and in using either of these means we enter a field as yet unexplored.

It is impossible to pursue an investigation in any novel line without finally making some interesting observation or learning some useful fact. That this statement is applicable to the subject of this lecture the many curious and unexpected phenomena which we observe afford a convincing proof. By way of illustration take, for instance, the most obvious phenomena, those of the discharge of the induction coil.

Here is a coil which is operated by currents vibrating with extreme rapidity, obtained by disruptively discharging a Leyden jar. It would not surprise a student were the lecturer to say that the secondary of this coil consists of a small length of comparatively stout wire; it would not surprise him were the lecturer to state that, in spite of this, the coil is capable of giving any potential which the best insulation of the turns is able to withstand; but although he may be prepared, and even indifferent as to the anticipated result, yet the aspect of the discharge of the coil will surprise and interest him. Everyone is familiar with the discharge of an ordinary coil; it need not be reproduced here. But, by way of contrast, here is a form of discharge of a coil the primary current of which is vibrating several hundred thousand times per second. The discharge of an ordinary coil appears as a simple line or band of light. The discharge of this coil appears in the form of powerful brushes and luminous streams issuing from all points of the two straight wires attached to the terminals of the secondary.

Now compare this phenomenon which you have just witnessed with the discharge of a Holtz or Wimshurst machine—that other interesting appliance so dear to the experimenter. What a difference there is between both these phenomena! And yet, had I made the necessary arrangements—which could have been made

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easily, were it not that they would interfere with other experiments—I could have produced with this coil sparks which, had I the coil hidden from your view and only two knobs exposed, even the keenest observer among you would find it difficult, if not

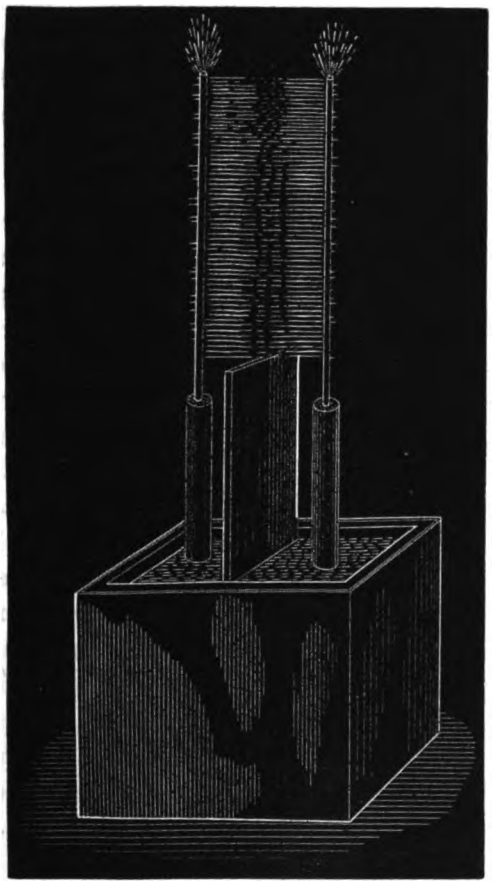

FIG. 1.—Discharge between Two Wires with Frequencies of a Few Hundred Thousands per Second.

impossible, to distinguish from those of an influence or friction machine. This may be done in many ways—for instance, by operating the induction coil which charges the condenser from an alternating-current machine of very low frequency, and preferably adjusting the discharge circuit so that there are no

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oscillations set up in it. We then obtain in the secondary circuit, if the knobs are of the required size and properly set, a more or less rapid succession of sparks of great intensity and small quantity, which possess the same brilliancy, and are accompanied by the same sharp crackling sound, as those obtained from a friction or influence machine.

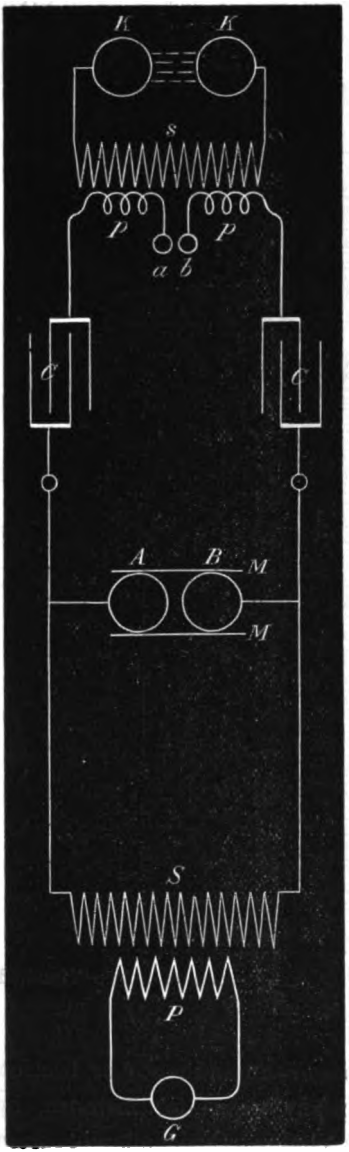

FIG. 2.—Imitating the Spark of a Holtz Machine.

Another way is to pass through two primary circuits, having a common secondary, two currents of a slightly different period, which produce in the secondary circuit sparks occurring at comparatively long intervals. But, even with the means at hand this evening, I may succeed in imitating the spark of a Holtz machine. For this purpose I establish between the terminals of the coil which charges the condenser a long, unsteady arc, which is periodically interrupted by the upward current of air produced by it. To increase the current of air I place on each side of the arc, and close to it, a large plate of mica. The condenser charged from this coil discharges into the primary circuit of a second coil through a small air gap, which is necessary to produce a sudden rush of current through the primary. The scheme of connections in the present experiment is indicated in Fig. 2.

G is an ordinarily constructed alternator, supplying the

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primary, P, of an induction coil, the secondary, S, of which charges the condensers, or jars, C C. The terminals of the secondary are connected to the inside coatings of the jars, the outer coatings being connected to the ends of the primary, p p, of a second induction coil. This primary p p has a small air gap, a b.

The secondary, s, of this coil is provided with knobs, or spheres, K K, of the proper size, and set at a distance suitable for the experiment.

A long arc is established between the terminals, A B, of the first induction coil. M M are the mica plates.

Each time the arc is broken between A and B the jars are quickly charged and discharged through the primary p p, producing a snapping spark between the knobs K K. Upon the arc forming between A and B the potential falls, and the jars cannot be charged to such high potential as to break through the air gap a b until the arc is again broken by the draught.

In this manner sudden impulses, at long intervals, are produced in the primary p p, which in the secondary s give a corresponding number of impulses of great intensity. If the secondary knobs, or spheres, K K, are of the proper size, the sparks show much resemblance to those of a Holtz machine.

But these two effects, which to the eye appear so very different, are only two of the many discharge phenomena. We only need to change the conditions of the test, and again we make other observations of interest.

When, instead of operating the induction coil as in the two last experiments, we operate it from a high-frequency alternator, as in the next experiment, a systematic study of the phenomena is rendered much more easy. In such case, in varying the strength and frequency of the currents through the primary, we may observe five distinct forms of discharge, which I have described in my former paper on this subject before the American Institute of Electrical Engineers, May 20th, 1891.

It would take too much time, and it would lead us too far from the subject presented this evening, to reproduce all these forms, but it seems to me desirable to show you one of them.

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It is a brush discharge, which is interesting in more than one respect. Viewed from a near position, it resembles much a jet of gas escaping under great pressure. We know that the phenomenon is due to the agitation of the molecules near the terminal, and we anticipate that some heat must be developed by the impact of the molecules against the terminal or against each other. Indeed, we find that the brush is hot, and only a little thought leads us to the conclusion that, could we but reach sufficiently high frequencies, we could produce a brush which would give intense light and heat, and which would resemble in every particular an ordinary flame, save, perhaps, that both phenomena might not be due to the same agent,—save, perhaps, that chemical affinity might not be *electrical* in its nature.

As the production of heat and light is here due to the impact of the molecules, or atoms, of air, or of something else besides, and as we can augment the energy simply by raising the potential, we might, even with frequencies obtained from a dynamo machine, intensify the action to such a degree as to bring the terminal to melting heat. But with such low frequencies we would have to deal always with something of the nature of an electric current. If I approach a conducting object to the brush, a thinny little spark passes, yet, even with the frequencies used this evening, the tendency to spark is not very great. So, for instance, if I hold a metallic sphere at some distance above the terminal, you may see the whole space between the terminal and sphere illuminated by the streams without the spark passing; and with the much higher frequencies obtainable by the disruptive discharge of a condenser, were it not for the sudden impulses, which are comparatively few in number, sparking would not occur even at very small distances. However, with incomparably higher frequencies, which we may yet find means to produce efficiently, and provided that electric impulses of such high frequencies could be transmitted through a conductor, the electrical characteristics of the brush discharge would completely vanish—no spark would pass, no shock would be felt—yet we would still have to deal with an *electric* phenomenon, but in the broad,

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modern interpretation of the word. In my first paper before referred to, I have pointed out the curious properties of the brush, and described the best manner of producing it, but I have thought it worth while to endeavour to express myself more clearly in regard to this phenomenon, because of its absorbing interest.

When a coil is operated with currents of very high frequency, beautiful brush effects may be produced, even if the coil be of comparatively small dimensions. The experimenter may vary them in many ways, and, if it were nothing else, they afford a pleasing sight. What adds to their interest is that they may be produced with one single terminal as well as with two—in fact, often better with one than with two.

But of all the discharge phenomena observed, the most pleasing to the eye, and the most instructive, are those observed with a coil which is operated by means of the disruptive discharge of a condenser. The power of the brushes, the abundance of the sparks, when the conditions are patiently adjusted, is often amazing. With even a very small coil, if it be so well insulated as to stand a difference of potential of several thousand volts per turn, the sparks may be so abundant that the whole coil may appear a complete mass of fire.

Curiously enough, the sparks, when the terminals of the coil are set at a considerable distance, seem to dart in every possible direction, as though the terminals were perfectly independent of each other. As the sparks would soon destroy the insulation, it is necessary to prevent them. This is best done by immersing the coil in a good liquid insulator, such as boiled-out oil. Immersion in a liquid may be considered almost an absolute necessity for the continued and successful working of such a coil.

It is of course out of question, in an experimental lecture, with only a few minutes at disposal for the performance of each experiment, to show these discharge phenomena to advantage, as to produce each phenomenon at its best a very careful adjustment is required. But even if imperfectly produced, as they are likely to be this evening, they are sufficiently striking to interest an intelligent audience.

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Before showing some of these curious effects I must, for the sake of completeness, give a short description of the coil and other apparatus used in the experiments with the disruptive discharge this evening.

It is contained in a box, *B* (Fig. 3), of thick boards of hard

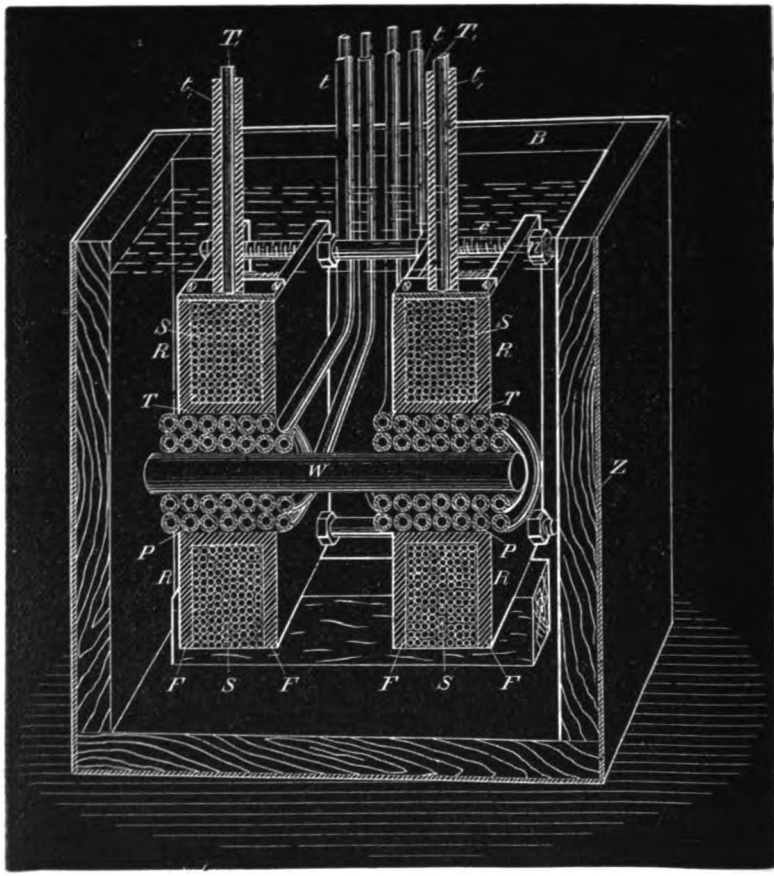

FIG. 3.—Disruptive Discharge Coil.

wood, covered on the outside with zinc sheet, *Z*, which is carefully soldered all around. It might be advisable, in a strictly scientific investigation, when accuracy is of great importance, to do away with the metal cover, as it might introduce many errors, principally on account of its complex action upon the coil, as a

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condenser of very small capacity, and as an electrostatic and electro-magnetic screen. When the coil is used for such experiments as are here contemplated, the employment of the metal cover offers some practical advantages, but these are of not sufficient importance to be dwelt upon.

The coil should be placed symmetrically to the metal cover, and the space between should of course not be too small—certainly not less than, say, 5 centimetres, but much more if possible; especially the two sides of the zinc box, which are at right angles to the axis of the coil, should be sufficiently remote from the latter, as otherwise they might impair its action and be a source of loss.

The coil consists of two spools of hard rubber, *R R*, held apart at a distance of 10 centimetres by bolts, *c*, and nuts, *n*, likewise of hard rubber. Each spool comprises a tube, *T*, of approximately 8 centimetres inside diameter and 3 millimetres thick, upon which are screwed two flanges, *F F*, 24 centimetres square, the space between the flanges being about 3 centimetres. The secondary, *S S*, of the best gutta-percha-covered wire, has 26 layers, 10 turns in each, giving for each half a total of 260 turns. The two halves are wound oppositely and connected in series, the connection between both being made over the primary. This disposition, besides being convenient, has the advantage that when the coil is well balanced—that is, when both of its terminals, T_1 T_1, are connected to bodies or devices of equal capacity—there is not much danger of breaking through to the primary, and the insulation between the primary and the secondary need not be thick. In using the coil it is advisable to attach to *both* terminals devices of nearly equal capacity, as, when the capacity of the terminals is not equal, sparks will be apt to pass to the primary. To avoid this, the middle point of the secondary may be connected to the primary, but this is not always practicable.

The primary, *P P*, is wound in two parts, and oppositely, upon a wooden spool, *W*, and the four ends are led out of the oil through hard rubber tubes, *t t*. The ends of the secondary, T_1 T_1, are also led out of the oil through rubber tubes, t_1 t_1, of great thickness. The primary and secondary layers are insulated by

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cotton cloth, the thickness of the insulation, of course, bearing some proportion to the difference of potential between the turns of the different layers. Each half of the primary has four layers, 24 turns in each, this giving a total of 96 turns. When both the parts are connected in series, this gives a ratio of conversion of about 1 : 2·7, and with the primaries in multiple, 1 : 5·4; but in operating with very rapidly alternating currents this ratio does not convey even an approximate idea of the ratio of the E.M.F.'s in the primary and secondary circuits. The coil is held in position in the oil on wooden supports, there being about 5 centimetres thickness of oil all around. Where the oil is not specially needed, the space is filled with pieces of wood, and for this purpose principally the wooden box *B* surrounding the whole is used.

The construction here shown is of course not the best on general principles, but I believe it is a good and convenient one for the production of effects in which an excessive potential and a very small current is needed.

In connection with the coil, I use either the ordinary form of discharger or a modified form. In the former I have introduced two changes which secure some advantages, and which are obvious. If they are mentioned, it is only in the hope that some experimenter may find them of use.

One of the changes is that the adjustable knobs, *A* and *B* (Fig. 4), of the discharger are held in jaws, *J J*, of brass by spring pressure, this allowing to turn them successively in different positions and so do away with the tedious process of frequent polishing up.

The other change consists in the employment of a strong electro-magnet, *N S*, which is placed with its axis at right angles to the line joining the knobs *A* and *B*, and produces a strong magnetic field between them. The pole-pieces of the magnet are movable, and properly formed so as to protrude between the brass knobs, in order to make the field as intense as possible; but to prevent the discharge from jumping to the magnet, the pole-pieces are protected by a layer of mica, *M M*, of sufficient thickness. s_1 s_1 and s_2 s_2 are screws for fastening the wires. On each side one

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of the screws is for large and the other for small wires. L L are screws for fixing in position the rods, R R, which support the knobs.

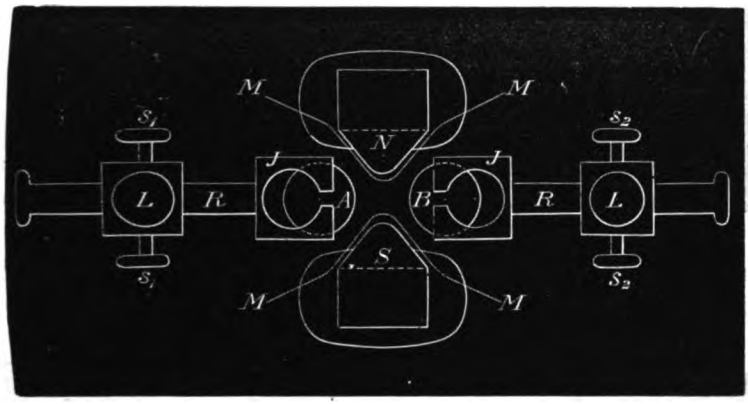

FIG. 4.—Arrangement of Improved Discharger and Magnet.

In another arrangement with the magnet I take the discharge between the rounded pole-pieces themselves, which in such case are insulated and preferably provided with polished brass caps.

The employment of an intense magnetic field is of advantage principally when the induction coil or transformer which charges the condenser is operated by currents of very low frequency. In such a case the number of the fundamental discharges between the knobs may be so small as to render the currents produced in the secondary unsuitable for many experiments. The intense magnetic field then serves to blow out the arc between the knobs as soon as it is formed, and the fundamental discharges occur in quicker succession.

Instead of the magnet, a draught or blast of air may be employed with some advantage. In this case the arc is preferably established between the knobs A B, in Fig. 2 (the knobs a b being generally joined, or entirely done away with), as in this disposition the arc is long and unsteady, and is easily affected by the draught.

When a magnet is employed to break the arc, it is better to choose the connection indicated schematically in Fig. 5, as in this

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case the currents forming the arc are much more powerful, and the magnetic field exercises a greater influence. The use of the

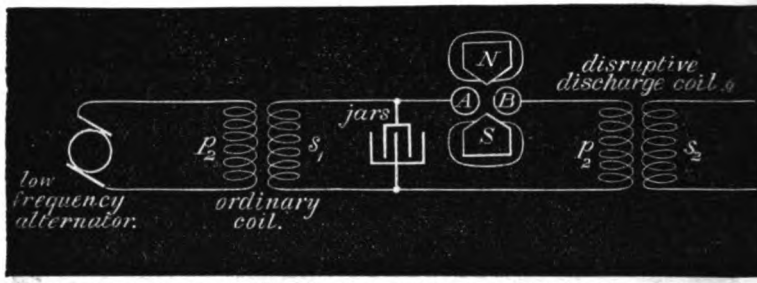

Fig. 5.—Arrangement with Low-Frequency Alternator and Improved Discharger.

magnet permits, however, of the arc being replaced by a vacuum tube, but I have encountered great difficulties in working with an exhausted tube.

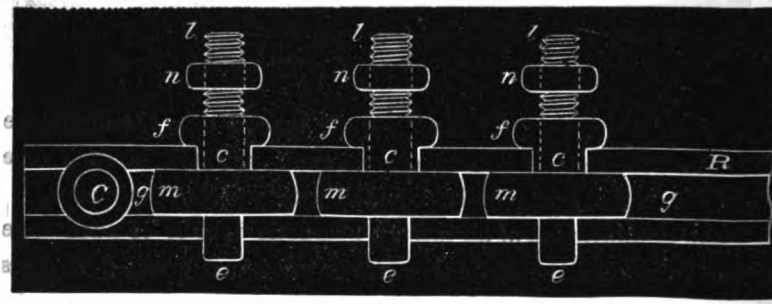

Fig. 6.

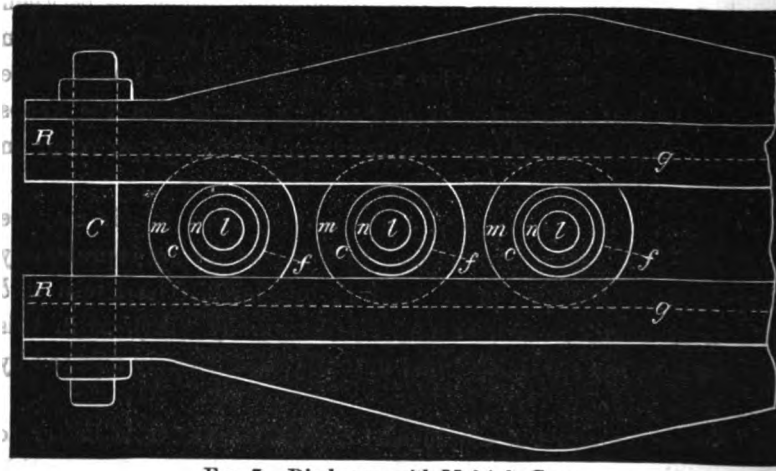

Fig. 7.—Discharger with Multiple Gaps.

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The other form of discharger used in these and similar experiments is indicated in Figs. 6 and 7. It consists of a number of brass pieces, *c c* (Fig. 6), each of which comprises a spherical middle portion, *m*, with an extension, *e*, below—which is merely used to fasten the piece in a lathe when polishing up the discharging surface—and a column above, which consists of a knurled flange, *f*, surmounted by a threaded stem, *l*, carrying a nut, *n*, by means of which a wire is fastened to the column. The flange *f* conveniently serves for holding the brass piece when fastening the wire, and also for turning it in any position when it becomes necessary to present a fresh discharging surface. Two stout strips of hard rubber, *R R*, with planed grooves, *g g* (Fig. 7), to fit the middle portion of the pieces *c c*, serve to clamp the latter and hold them firmly in position by means of two bolts, *C C* (of which only one is shown), passing through the ends of the strips.

In the use of this kind of discharger I have found three principal advantages over the ordinary form. Firstly, the dielectric strength of a given total width of air space is greater when a great many small air gaps are used instead of one, which permits of working with a smaller length of air gap, and that means smaller loss and less deterioration of the metal; secondly, by reason of splitting the arc up into smaller arcs, the polished surfaces are made to last much longer; and thirdly, the apparatus affords some gauge in the experiments. I usually set the pieces, by putting between them sheets of uniform thickness, at a certain very small distance which is known from the experiments of Sir William Thomson to require a certain electro-motive force to be bridged by the spark. It should, of course, be remembered that the sparking distance is much diminished as the frequency is increased. By taking any number of spaces the experimenter has a rough idea of the electro-motive force, and he finds it also easier to repeat an experiment, as he has not the trouble of setting the knobs again and again. With this kind of discharger I have been able to maintain an oscillating motion without any spark being visible with the naked eye between the knobs, and they would not show a very appreciable

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rise in temperature. This form of discharger also lends itself to many arrangements of condensers and circuits which are often very convenient and time-saving. I have used it preferably in a disposition similar to that indicated in Fig. 2, when the currents forming the arc are small.

I may here mention that I have also used dischargers with single or multiple air gaps, in which the discharge surfaces were rotated with great speed. No particular advantage was, however, gained by this method, except in cases where the currents from the condenser were large and the keeping cool of the surfaces was necessary, and in cases when, the discharge not being oscillating of itself, the arc as soon as established was broken by the air current, thus starting the vibration at intervals in rapid succession. I have also used mechanical interruptors in many ways. To avoid the difficulties with frictional contacts, the preferred plan adopted was to establish the arc and rotate through it at great speed a rim of mica provided with many holes, and fastened to a steel plate. It is understood, of course, that the employment of a magnet, air current, or other interruptor, produces no effect worth noticing, unless the self-induction, capacity, and resistance are so related that there are oscillations set up upon each interruption.

I will now endeavour to show you some of the most noteworthy of these discharge phenomena.

I have stretched across the room two ordinary cotton-covered wires, each about 7 metres in length. They are supported on insulating cords at a distance of about 30 centimetres. I attach now to each of the terminals of the coil one of the wires, and set the coil in action. Upon turning the lights off in the room you see the wires strongly illuminated by the streams issuing abundantly from their whole surface in spite of the cotton covering, which may even be very thick. When the experiment is performed under good conditions, the light from the wires is sufficiently intense to allow distinguishing the objects in a room. To produce the best result it is, of course, necessary to adjust carefully the capacity of the jars, the arc between the knobs, and the length of the wires. My experience is that calculation

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of the length of the wires leads in such case to no result whatever. The experimenter will do best to take the wires at the start very long, and then adjust by cutting off first long pieces, and then smaller and smaller ones as he approaches the right length.

A convenient way is to use an oil condenser of very small capacity, consisting of two small adjustable metal plates, in connection with this and similar experiments. In such case I take wires rather short, and set at the beginning the condenser plates at maximum distance. If the streams from the wires increase by approach of the plates, the length of the wires is about right; if they diminish, the wires are too long for that frequency and potential. When a condenser is used in connection with experiments with such a coil, it should be an oil condenser by all means, as in using an air condenser considerable energy might be wasted. The wires leading to the plates in the oil should be very thin, heavily coated with some insulating compound, and provided with a conducting covering—this preferably extending under the surface of the oil. The conducting cover should not be too near the terminals, or ends, of the wire, as a spark would be apt to jump from the wire to it. The conducting coating is used to diminish the air losses, in virtue of its action as an electrostatic screen. As to the size of the vessel containing the oil, and the size of the plates, the experimenter gains at once an idea from a rough trial. The size of the plates *in oil* is, however, calculable, as the dielectric losses are very small.

In the preceding experiment it is of considerable interest to know what relation the quantity of the light emitted bears to the frequency and potential of the electric impulses. My opinion is that the heat as well as light effects produced should be proportionate, under otherwise equal conditions of test, to the product of frequency and square of potential, but the experimental verification of the law, whatever it may be, would be exceedingly difficult. One thing is certain, at any rate, and that is, that in augmenting the potential and frequency we rapidly intensify the streams; and, though it may be very sanguine, it is

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surely not altogether hopeless to expect that we may succeed in producing a practical illuminant on these lines. We would then be simply using burners or flames, in which there would be no chemical process, no consumption of material, but merely a transfer of energy, and which would in all probability emit more light and less heat than ordinary flames.

The luminous intensity of the streams is, of course, considerably increased when they are focused upon a small surface. This may be shown by the following experiment.

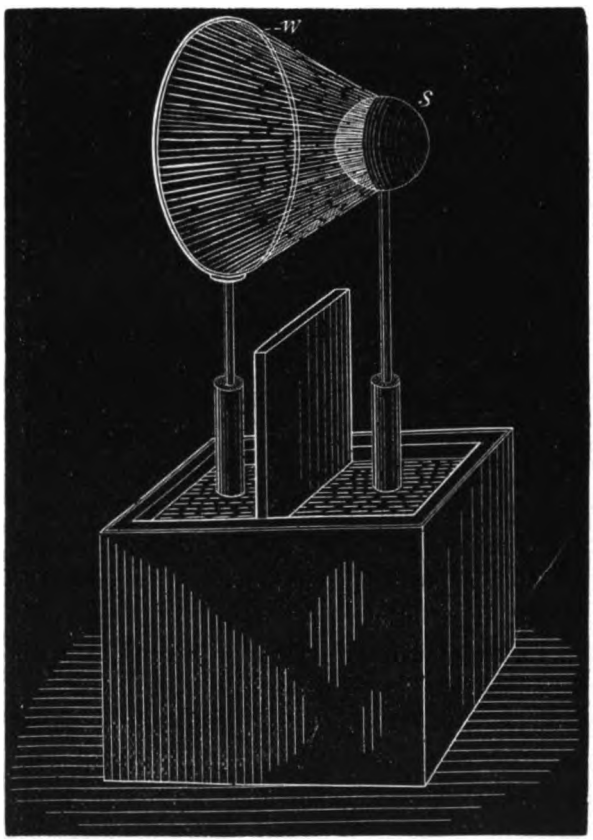

FIG. 8.—Effect produced by Concentrating Streams.

I attach to one of the terminals of the coil a wire, *w* (Fig. 8), bent in a circle of about 30 centimetres in diameter, and to the

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other terminal I fasten a small brass sphere, *s*; the surface of the wire being preferably equal to the surface of the sphere, and the centre of the latter being in a line at right angles to the plane of the wire circle and passing through its centre. When the discharge is established under proper conditions, a luminous hollow cone is formed, and in the dark one half of the brass sphere is strongly illuminated, as shown in the cut.

By some artifice or other it is easy to concentrate the streams upon small surfaces and to produce very strong light effects. Two thin wires may thus be rendered intensely luminous.

In order to intensify the streams the wires should be very thin and short; but as in this case their capacity would be generally too small for the coil, at least for such a one as the present, it is necessary to augment the capacity to the required value, while, at the same time, the surface of the wires remains very small. This may be done in many ways.

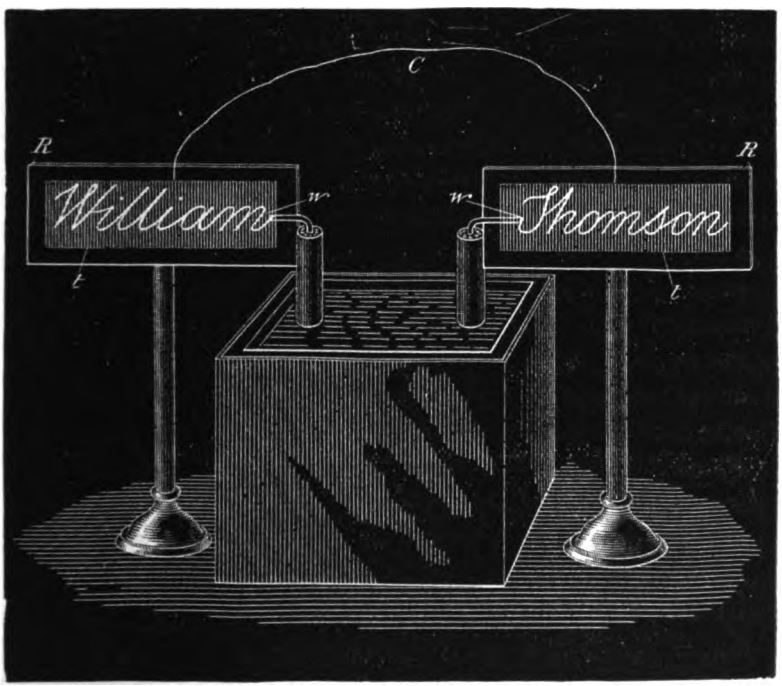

FIG. 9.—Wires rendered intensely Luminous.

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Here, for instance, I have two plates, *R R*, of hard rubber (Fig 9), upon which I have glued two very thin wires, *w w*, so as to form a name. The wires may be bare or covered with the best insulation—it is immaterial for the success of the experiment. Well, insulated wires, if anything, are preferable. On the back of each plate, indicated by the shaded portion, is a tinfoil coating *t t*. The plates are placed in line at a sufficient distance to prevent a spark passing from one to the other wire. The two tinfoil coatings I have joined by a conductor, *C*, and the two wires I presently connect to the terminals of the coil. It is now easy, by varying the strength and frequency of the currents through the primary, to find a point at which the capacity of the system is best suited to the conditions, and the wires become so strongly luminous that, when the light in the room is turned off, the name formed by them appears in brilliant letters.

It is perhaps preferable to perform this experiment with a coil operated from an alternator of high frequency, as then, owing to the harmonic rise and fall, the streams are very uniform, though they are less abundant than when produced with such a coil as the present. This experiment, however, may be performed with low frequencies, but much less satisfactorily.

When two wires, attached to the terminals of the coil, are set at the proper distance, the streams between them may be so intense as to produce a continuous luminous sheet. To show this phenomenon I have here two circles, *C* and *c* (Fig. of 10), of rather stout wire, one being about 80 centimetres and the other 30 centimetres in diameter. To each of the terminals of the coil I attach one of the circles. The supporting wires are so bent that the circles may be placed in the same plane, coinciding as nearly as possible. When the light in the room is turned off and the coil set to work, you see the whole space between the wires uniformly filled with streams, forming a luminous disc, which could be seen from a considerable distance, such is the intensity of the streams. The outer circle could have been much larger than the present one; in fact, with this coil I have used much larger circles, and I have been able to produce a strongly luminous sheet, covering an area of more than 1 square metre, which is a

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remarkable effect with this very small coil. To avoid uncertainty, the circle has been taken smaller, and the area is now about 0·43 square metre.

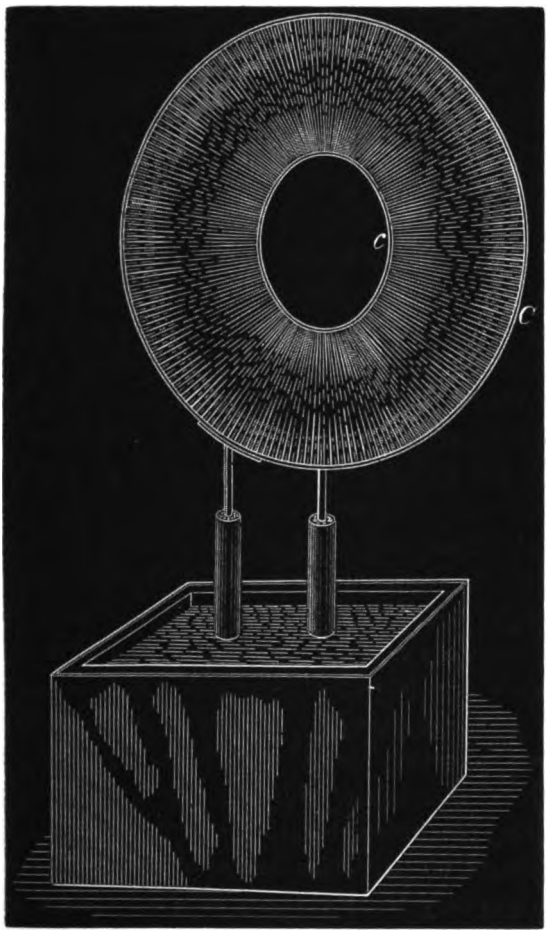

Fig. 10.—Luminous Discs.

The frequency of the vibration, and the quickness of succession of the sparks between the knobs, affect to a marked degree the appearance of the streams. When the frequency is very low, the air gives way in more or less the same manner, as by a steady difference of potential, and the streams consist of distinct threads, generally mingled with thin sparks, which probably correspond

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to the successive discharges occurring between the knobs. But when the frequency is extremely high, and the arc of the discharge produces a very *loud* but *smooth* sound—showing both that oscillation takes place and that the sparks succeed each other with great rapidity—then the luminous streams formed are perfectly uniform. To reach this result very small coils and jars of small capacity should be used. I take two tubes of thick Bohemian glass, about 5 centimetres in diameter and 20 centimetres long. In each of the tubes I slip a primary of very thick copper wire. On the top of each tube I wind a secondary of much thinner gutta-percha-covered wire. The two secondaries I connect in series, the primaries preferably in multiple arc. The tubes are then placed in a large glass vessel, at a distance of 10 to 15 centimetres from each other, on insulating supports, and the vessel is filled with boiled-out oil, the oil reaching about an inch above the tubes. The free ends of the secondary are lifted out of the oil and placed parallel to each other at a distance of about 10 centimetres. The ends which are scraped should be dipped in the oil. Two four-pint jars joined in series may be used to discharge through the primary. When the necessary adjustments in the length and distance of the wires above the oil and in the arc of discharge are made, a luminous sheet is produced between the wires which is perfectly smooth and textureless, like the ordinary discharge through a moderately exhausted tube.

I have purposely dwelt upon this apparently insignificant experiment. In trials of this kind the experimenter arrives at the startling conclusion that, to pass ordinary luminous discharges through gases, no particular degree of exhaustion is needed, but that the gas may be at ordinary or even greater pressure. To accomplish this, a very high frequency is essential; a high potential is likewise required, but this is a merely incidental necessity. These experiments teach us that, in endeavouring to discover novel methods of producing light by the agitation of atoms, or molecules, of a gas, we need not limit our research to the vacuum tube, but may look forward quite seriously to the possibility of obtaining the light effects without the use of any vessel whatever, with air at ordinary pressure.

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Such discharges of very high frequency, which render luminous the air at ordinary pressures, we have probably often occasion to witness in Nature. I have no doubt that if—as many believe—the *Aurora borealis* is produced by sudden cosmic disturbances, such as eruptions at the sun's surface, which set the electrostatic charge of the earth in an extremely rapid vibration, the red glow observed is not confined to the upper rarefied strata of the air, but the discharge traverses, by reason of its very high frequency, also the dense atmosphere in the form of a *glow*, such as we ordinarily produce in a slightly exhausted tube. If the frequency were very low, or even more so, if the charge were not at all vibrating, the dense air would break down as in a lightning discharge. Indications of such breaking down of the lower dense strata of the air have been repeatedly observed at the occurrence of this marvellous phenomenon; but if it does occur, it can only be attributed to the fundamental disturbances, which are few in number, for the vibration produced by them would be far too rapid to allow a disruptive break. It is the original and irregular impulses which affect the instruments; the superimposed vibrations probably pass unnoticed.

When an ordinary low-frequency discharge is passed through moderately rarefied air, the air assumes a purplish hue. If by some means or other we increase the intensity of the molecular, or atomic, vibration, the gas changes to a white colour. A similar change occurs at ordinary pressures with electric impulses of very high frequency. If the molecules of the air around a wire are moderately agitated, the brush formed is reddish or violet; if the vibration is rendered sufficiently intense, the streams become white. We may accomplish this in various ways. In the experiment before shown with the two wires across the room, I have endeavoured to secure the result by pushing to a high value both the frequency and potential; in the experiment with the thin wires glued on the rubber plate, I have concentrated the action upon a very small surface—in other words, I have worked with a great electric density.

A most curious form of discharge is observed with such

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a coil when the frequency and potential are pushed to the extreme limit. To perform the experiment, every part of the coil should be heavily insulated, and only two small spheres — or, better still, two sharp-edged metal discs (*d d*, Fig. 11) of no more than a few centimetres in diameter—

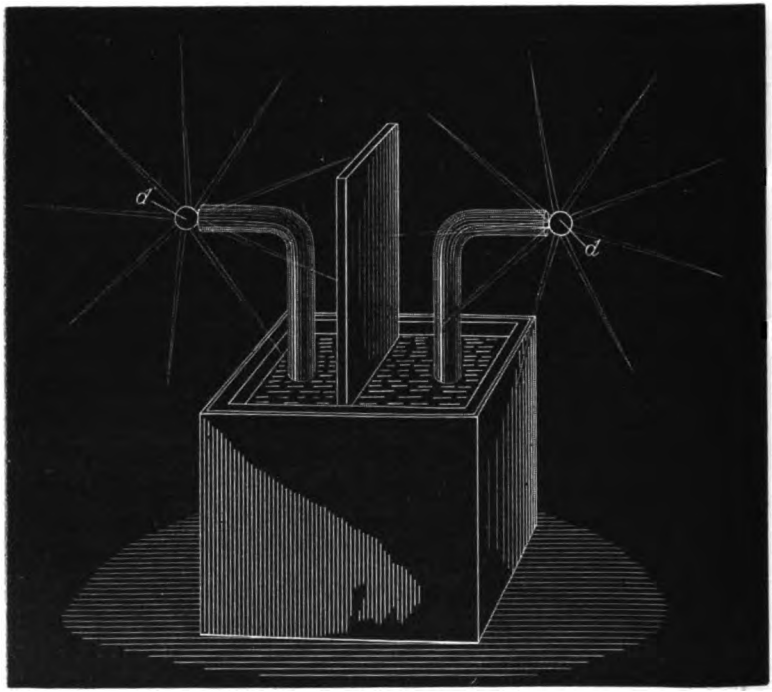

FIG. 11.—Phantom Streams.

should be exposed to the air. The coil here used is immersed in oil, and the ends of the secondary reaching out of the oil are covered with an air-tight cover of hard rubber of great thickness. All cracks, if there are any, should be carefully stopped up, so that the brush discharge cannot form anywhere except on the small spheres or plates which are exposed to the air. In this case, since there are no large plates or other bodies of capacity attached to the terminals, the coil is capable of an extremely rapid vibration. The potential may be raised by increasing, as far as the experimenter judges proper, the rate of change of the primary current. With a coil not widely

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differing from the present, it is best to connect the two primaries in multiple arc; but if the secondary should have a much greater number of turns, the primaries should preferably be used in series, as otherwise the vibration might be too fast for the secondary. It occurs under these conditions that misty white streams break forth from the edges of the discs and spread out phantom-like into space. With this coil, when fairly well produced, they are about 25 to 30 centimetres long. When the hand is held against them, no sensation is produced, and a spark, causing a shock, jumps from the terminal only upon the hand being brought much nearer. If the oscillation of the primary current is rendered intermittent by some means or other, there is a corresponding throbbing of the streams, and now the hand or other conducting object may be brought in still greater proximity to the terminal without a spark being caused to jump.

Among the many beautiful phenomena which may be produced with such a coil, I have here selected only those which appear to possess some features of novelty, and lead us to some conclusions of interest. One will not find it at all difficult to produce in the laboratory, by means of it, many other phenomena which appeal to the eye even more than these here shown, but present no particular feature of novelty.

Early experimenters describe the display of sparks produced by an ordinary large induction coil upon an insulating plate separating the terminals. Quite recently Siemens performed some experiments in which fine effects were obtained, which were seen by many with interest. No doubt large coils, even if operated with currents of low frequencies, are capable of producing beautiful effects. But the largest coil ever made could not, by far, equal the magnificent display of streams and sparks obtained from such a disruptive discharge coil when properly adjusted. To give an idea, a coil such as the present one will cover easily a plate of 1 metre in diameter completely with the streams. The best way to perform such experiments is to take a very thin rubber or a glass plate and glue on one side of it a narrow ring of tinfoil of very large diameter, and on the other a circular washer, the centre of the latter coinciding with

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that of the ring, and the surfaces of both being preferably equal, so as to keep the coil well balanced. The washer and ring should be connected to the terminals by heavily insulated thin wires. It is easy in observing the effect of the capacity to produce a sheet of uniform streams, or a fine network of thin silvery threads, or a mass of loud brilliant sparks, which cover completely the plate.

Since I have advanced the idea of the conversion by means of the disruptive discharge, in my paper before the American Institute of Electrical Engineers at the beginning of the past year, the interest excited in it has been considerable. It affords us a means for producing any potentials by the aid of inexpensive coils operated from ordinary systems of distribution, and —what is perhaps more appreciated—it enables us to convert currents of any frequency into currents of any other lower or higher frequency. But its chief value will perhaps be found in the help which it will afford us in the investigations of the phenomena of phosphorescence, which a disruptive discharge coil is capable of exciting in innumerable cases where ordinary coils, even the largest, would utterly fail.

Considering its probable uses for many practical purposes, and its possible introduction into laboratories for scientific research, a few additional remarks as to the construction of such a coil will perhaps not be found superfluous.

It is, of course, absolutely necessary to employ in such a coil wires provided with the best insulation.

Good coils may be produced by employing wires covered with several layers of cotton, boiling the coil a long time in pure wax, and cooling under moderate pressure. The advantage of such a coil is that it can be easily handled, but it cannot probably give as satisfactory results as a coil immersed in pure oil. Besides, it seems that the presence of a large body of wax affects the coil disadvantageously, whereas this does not seem to be the case with oil. Perhaps it is because the dielectric losses in the liquid are smaller.

I have tried at first silk- and cotton-covered wires with oil immersion, but I have been gradually led to use gutta-percha-

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covered wires, which proved most satisfactory. Gutta-percha insulation adds, of course, to the capacity of the coil, and this, especially if the coil be large, is a great disadvantage when extreme frequencies are desired; but, on the other hand, gutta-percha will withstand much more than an equal thickness of oil, and this advantage should be secured at any price. Once the coil has been immersed, it should never be taken out of the oil for more than a few hours, else the gutta-percha will crack up and the coil will not be worth half as much as before. Gutta-percha is probably slowly attacked by the oil, but after an immersion of eight to nine months I have found no ill effects.

I have obtained in commerce two kinds of gutta-percha wire: in one the insulation sticks tightly to the metal, in the other it does not. Unless a special method is followed to expel all air, it is much safer to use the first kind. I wind the coil within an oil tank so that all interstices are filled up with the oil. Between the layers I use cloth boiled out thoroughly in oil, calculating the thickness according to the difference of potential between the turns. There seems not to be a very great difference whatever kind of oil is used; I use paraffin or linseed oil.

To exclude more perfectly the air, an excellent way to proceed, and easily practicable with small coils, is the following:—Construct a box of hard wood of very thick boards which have been for a long time boiled in oil. The boards should be so joined as to safely withstand the external air pressure. The coil being placed and fastened in position within the box, the latter is closed with a strong lid, and covered with closely fitting metal sheet, the joints of which are soldered very carefully. On the top two small holes are drilled, passing through the metal sheet and the wood, and in these holes two small glass tubes are inserted and the joints made air-tight. One of the tubes is connected to a vacuum pump, and the other with a vessel containing a sufficient quantity of boiled-out oil. The latter tube has a very small hole at the bottom, and is provided with a stop-cock. When a fairly good vacuum has been obtained, the stop-cock is opened and the oil slowly fed in. Proceeding in this manner, it is impossible that any big bubbles, which are the principal danger, should remain between the turns.

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The air is most completely excluded, probably better than by boiling out, which, however, when gutta-percha-coated wires are used, is not practicable.

For the primaries I use ordinary line wire with a thick cotton coating. Strands of very thin insulated wires properly interlaced would, of course, be the best to employ for the primaries, but they are not to be had.

In an experimental coil the size of the wires is not of great importance. In the coil here used the primary is No. 12, and the secondary No. 24 Brown and Sharpe W.G. wire; but the sections may be varied considerably: it would only imply different adjustments, the results aimed at would not be materially affected.

I have dwelt at some length upon the various forms of brush discharge because, in studying them, we not only observe phenomena which please our eye, but also afford us food for thought, and lead us to conclusions of practical importance. In the use of alternating currents of very high tension, not too much precaution can be taken to prevent the brush discharge. In a main conveying such currents, in an induction coil or transformer, or in a condenser, the brush discharge is a source of great danger to the insulation. In a condenser especially the gaseous matter must be most carefully expelled, for in it the charged surfaces are near each other, and if the potentials are high, just as sure as a weight will fall if let go, so the insulation will give way if a single gaseous bubble of some size be present, whereas, if all gaseous matter were carefully excluded, the condenser would safely withstand a much higher difference of potential. A main conveying alternating currents of very high tension may be injured merely by a blow-hole or small crack in the insulation, the more so as a blow-hole is apt to contain gas at low pressure; and as it appears almost impossible to completely obviate such little imperfections, I am led to believe that in our future distributions of electrical energy by currents of very high tension, liquid insulation will be used. The cost is a great drawback, but if we employ an oil as an insulator, the distributions of electrical energy with something like 100,000 volts, and even more, become, at least with higher

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frequencies, so easy that they could be hardly called engineering feats. With oil insulation and alternate-current motors transmissions of power can be effected with safety, and upon an industrial basis, at distances of as much as a thousand miles.

A peculiar property of oils, and liquid insulation in general, when subjected to rapidly changing electric stresses, is to disperse any gaseous bubbles which may be present, and diffuse them through its mass, generally long before any injurious break can occur. This feature may be easily observed with an ordinary induction coil by taking the primary out, plugging up the end of the tube upon which the secondary is wound, and filling it with some fairly transparent iusulator, such as paraffin oil. A primary of a diameter something like 6 mm. smaller than the inside of the tube may be inserted in the oil. When the coil is set to work, one may see, looking from the top through the oil, many luminous points—air bubbles which are caught by inserting the primary, and which are rendered luminous in consequence of the violent bombardment. The occluded air, by its impact against the oil, heats it; the oil begins to circulate, carrying some of the air along with it, until the bubbles are dispersed and the luminous points disappear. In this manner, unless large bubbles are occluded in such way that circulation is rendered impossible, a damaging break is averted, the only effect being a moderate warming up of the oil. If, instead of the liquid, a solid insulation, no matter how thick, were used, a breaking through and injury of the apparatus would be inevitable.

The exclusion of gaseous matter from any apparatus in which the dielectric is subjected to more or less rapidly changing electric forces is, however, not only desirable in order to avoid a possible injury of the apparatus, but also on account of economy. In a condenser, for instance, as long as only a solid or only a liquid dielectric is used, the loss is small; but if a gas under ordinary or small pressure be present the loss may be very great. Whatever the nature of the force acting in the dielectric may be, it seems that in a solid or liquid the molecular displacement produced by the force is small: hence the product of force and displacement is insignificant, unless the force be very great; but

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in a gas the displacement, and therefore this product, is considerable: the molecules are free to move, they reach high speeds, and the energy of their impact is lost in heat or otherwise. If the gas be strongly compressed, the displacement due to the force is made smaller, and the losses are reduced.

In most of the succeeding experiments I prefer, chiefly on account of the regular and positive action, to employ the alternator before referred to. This is one of the several machines constructed by me for the purposes of these investigations. It has 384 pole projections, and is capable of giving currents of a frequency of about 10,000 per second. This machine has been illustrated and briefly described in my first paper before the American Institute of Electrical Engineers, May 20th, 1891, to which I have already referred. A more detailed description, sufficient to enable any engineer to build a similar machine, will be found in several electrical journals of that period.

The induction coils operated from the machine are rather small, containing from 5,000 to 15,000 turns in the secondary. They are immersed in boiled-out linseed oil, contained in wooden boxes covered with zinc sheet.

I have found it advantageous to reverse the usual position of the wires, and to wind, in these coils, the primaries on the top; this allowing the use of a much bigger primary, which, of course, reduces the danger of overheating, and increases the output of the coil. I make the primary on each side at least 1 centimetre shorter than the secondary, to prevent the breaking through on the ends, which would surely occur unless the insulation on the top of the secondary be very thick, and this, of course, would be disadvantageous.

When the primary is made movable, which is necessary in some experiments, and many times convenient for the purposes of adjustment, I cover the secondary with wax, and turn it off in a lathe to a diameter slightly smaller than the inside of the primary coil. The latter I provide with a handle reaching out of the oil, which serves to shift it in any position along the secondary.

I will now venture to make, in regard to the general manipu-

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lation of induction coils, a few observations bearing upon points which have not been fully appreciated in earlier experiments with such coils, and are even now often overlooked.

The secondary of the coil possesses usually such a high self-induction that the current through the wire is inappreciable, and may be so even when the terminals are joined by a conductor of small resistance. If capacity is added to the terminals, the self-induction is counteracted, and a stronger current is made to flow through the secondary, though its terminals are insulated from each other. To one entirely unacquainted with the properties of alternating currents nothing will look more puzzling. This feature was illustrated in the experiment performed at the beginning with the top plates of wire gauze attached to the terminals and the rubber plate. When the plates of wire gauze were close together, and a small arc passed between them, the arc *prevented* a strong current to pass through the secondary, because it did away with the capacity on the terminals; when the rubber plate was inserted between, the capacity of the condenser formed counteracted the self-induction of the secondary, a stronger current passed now, the coil performed more work, and the discharge was by far more powerful.

The first thing, then, in operating the induction coil is to combine capacity with the secondary, to overcome the self-induction. If the frequencies and potentials are very high, gaseous matter should be carefully kept away from the charged surfaces. If Leyden jars are used, they should be immersed in oil, as otherwise considerable dissipation may occur if the jars are greatly strained. When high frequencies are used, it is of equal importance to combine a condenser with the primary. One may use a condenser connected to the ends of the primary or to the terminals of the alternator, but the latter is not to be recommended, as the machine might be injured. The best way is, undoubtedly, to use the condenser in series with the primary and with the alternator, and to adjust its capacity so as to annul the self-induction of both the latter. The condenser should be adjustable by very small steps, and for a finer adjustment

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a small oil condenser with movable plates may be used conveniently.

I think it best at this juncture to bring before you a phenomenon, observed by me some time ago, which to the purely scientific investigator may perhaps appear more interesting than any of the results which I have the privilege to present to you this evening.

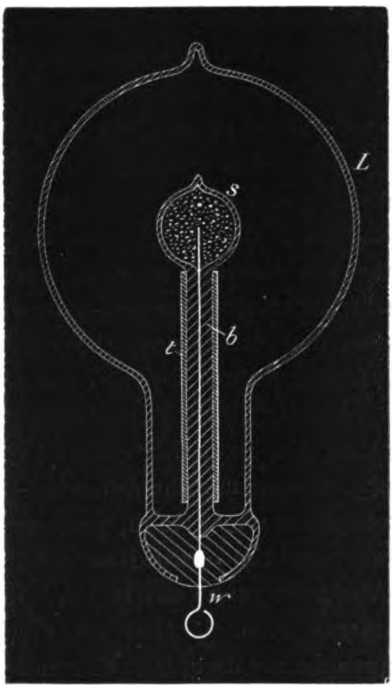

FIG. 12.

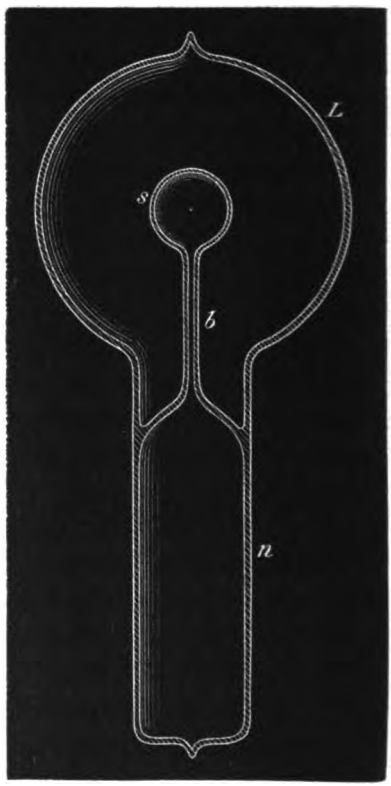

FIG. 13.

Bulbs for producing Rotating Brush.

It may be quite properly ranked among the brush phenomena —in fact, it is a brush, formed at, or near, a single terminal in high vacuum.

In bulbs provided with a conducting terminal, though it be of aluminium, the brush has but an ephemeral existence, and cannot, unfortunately, be indefinitely preserved in its most sensitive

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state, even in a bulb devoid of any conducting electrode. In studying the phenomenon, by all means a bulb having no leading-in wire should be used. I have found it best to use bulbs constructed as indicated in Figs. 12 and 13.

In Fig. 12 the bulb comprises an incandescent lamp globe, *L*, in the neck of which is sealed a barometer tube, *b*, the end of which is blown out to form a small sphere, *s*. This sphere should be sealed as closely as possible in the centre of the large globe. Before sealing, a thin tube, *t*, of aluminium sheet may be slipped in the barometer tube, but it is not important to employ it.

The small hollow sphere *s* is filled with some conducting powder, and a wire, *w*, is cemented in the neck for the purpose of connecting the conducting powder with the generator.

The construction shown in Fig. 13 was chosen in order to remove from the brush any conducting body which might possibly affect it. The bulb consists in this case of a lamp globe, *L*, which has a neck, *n*, provided with a tube, *b*, and small sphere, *s*, sealed to it, so that two entirely independent compartments are formed, as indicated in the drawing. When the bulb is in use, the neck *n* is provided with a tinfoil coating, which is connected to the generator, and acts inductively upon the moderately rarefied and highly conducting gas enclosed in the neck. From there the current passes through the tube *b* into the small sphere *s*, to act by induction upon the gas contained in the globe *L*.

It is of advantage to make the tube *t* very thick, the hole through it very small, and to blow the sphere *s* very thin. It is of the greatest importance that the sphere *s* be placed in the centre of the globe *L*.

Figs. 14, 15, and 16 indicate different forms, or stages, of the brush. Fig. 14 shows the brush as it first appears in a bulb provided with a conducting terminal; but, as in such a bulb it very soon disappears—often after a few minutes—I will confine myself to the description of the phenomenon as seen in a bulb without conducting electrode. It is observed under the following conditions:—

When the globe *L* (Figs. 12 and 13) is exhausted to

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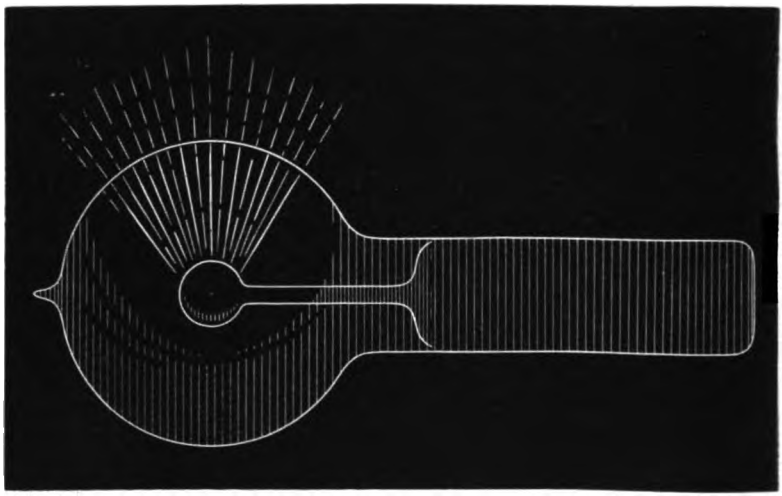

Fig. 16.

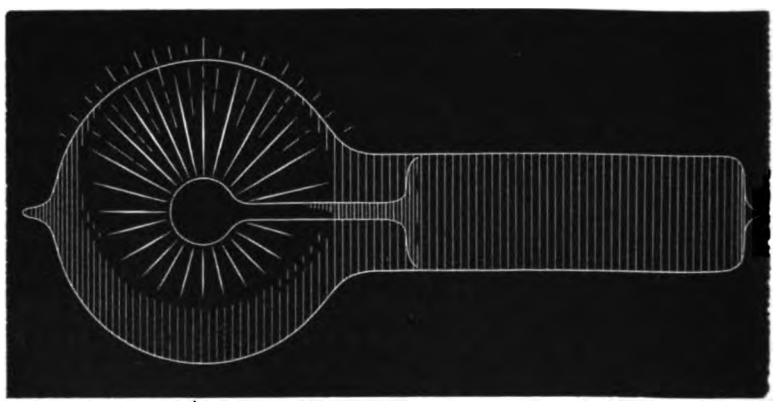

Fig. 15.

Forms and Phases of the Rotating Brush.

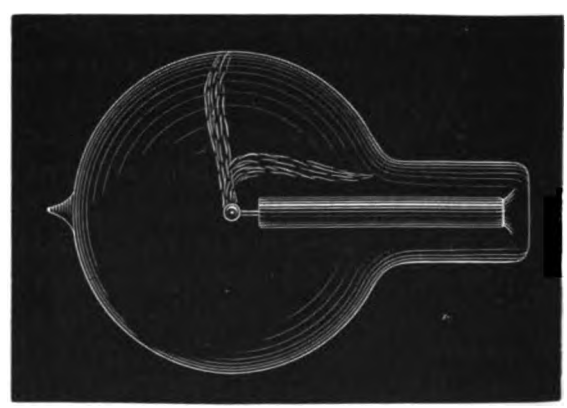

Fig. 14.

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a very high degree, generally the bulb is not excited upon connecting the wire w (Fig. 12) or the tinfoil coating of the bulb (Fig. 13) to the terminal of the induction coil. To excite it, it is usually sufficient to grasp the globe L with the hand. An intense phosphorescence then spreads at first over the globe, but soon gives place to a white, misty light. Shortly afterwards one may notice that the luminosity is unevenly distributed in the globe, and after passing the current for some time the bulb appears as in Fig. 15. From this stage the phenomenon will gradually pass to that indicated in Fig. 16, after some minutes, hours, days, or weeks, according how the bulb is worked. Warming the bulb or increasing the potential hastens the transit.

When the brush assumes the form indicated in Fig. 16, it may be brought to a state of extreme sensitiveness to electrostatic and magnetic influence. The bulb hanging straight down from a wire, and all objects being remote from it, the approach of the observer at a few paces from the bulb will cause the brush to fly to the opposite side, and if he walks around the bulb it will always keep on the opposite side. It may begin to spin around the terminal long before it reaches that sensitive stage. When it begins to turn around principally, but also before, it is affected by a magnet, and at a certain stage it is susceptible to magnetic influence to an astonishing degree. A small permanent magnet, with its poles at a distance of no more than 2 centimetres, will affect it visibly at a distance of 2 metres, slowing down or accelerating the rotation according to how it is held relatively to the brush. I think I have observed that at the stage when it is most sensitive to magnetic, it is not most sensitive to electrostatic influence. My explanation is, that the electrostatic attraction between the brush and the glass of the bulb, which retards the rotation, grows much quicker than the magnetic influence when the intensity of the stream is increased.

When the bulb hangs with the globe L down, the rotation is always clockwise. In the Southern Hemisphere it would occur in the opposite direction, and on the Equator the brush should not turn at all. The rotation may be reversed by a magnet kept

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at some distance. The brush rotates, seemingly, best when it is at right angles to the lines of force of the earth. It very likely rotates, when at its maximum speed, in synchronism with the alternations, say 10,000 times a second. The rotation can be slowed down or accelerated by the approach or receding of the observer, or any conducting body, but it cannot be reversed by putting the bulb in any position. When it is in the state of the highest sensitiveness, and the potential or frequency be varied, the sensitiveness is rapidly diminished. Changing either of these but little will generally stop the rotation. The sensitiveness is likewise affected by the variations of temperature. To attain great sensitiveness it is necessary to have the small sphere *s* in the centre of the globe *L*, as otherwise the electrostatic action of the glass of the globe will tend to stop the rotation. The sphere *s* should be small and of uniform thickness: any dissymmetry of course has the effect to diminish the sensitiveness.

The fact that the brush rotates in a definite direction in a permanent magnetic field seems to show that in alternating currents of very high frequency the positive and negative impulses are not equal, but that one always preponderates over the other.

Of course, this rotation in one direction may be due to the action of two elements of the same current upon each other, or to the action of the field produced by one of the elements upon the other, as in a series motor, without necessarily one impulse being stronger than the other. The fact that the brush turns, as far as I could observe, in any position, would speak for this view. In such case it would turn at any point of the earth's surface. But, on the other hand, it is then hard to explain why a permanent magnet should reverse the rotation, and one must assume the preponderation of impulses of one kind.

As to the causes of the formation of the brush or stream, I think it is due to the electrostatic action of the globe and the dissymmetry of the parts. If the small bulb *s* and the globe *L* were perfect concentric spheres, and the glass throughout of the same thickness and quality, I think the brush would not form, as the tendency to pass would be equal on all sides. That the

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formation of the stream is due to an irregularity is apparent from the fact that it has the tendency to remain in one position, and rotation occurs most generally only when it is brought out of this position by electrostatic or magnetic influence. When in an extremely sensitive state it rests in one position, most curious experiments may be performed with it. For instance, the experimenter may, by selecting a proper position, approach the hand at a certain considerable distance to the bulb, and he may cause the brush to pass off by merely stiffening the muscles of the arm. When it begins to rotate slowly, and the hands are held at a proper distance, it is impossible to make even the slightest motion without producing a visible effect upon the brush. A metal plate connected to the other terminal of the coil affects it at great distance, slowing down the rotation often to one turn a second.

I am firmly convinced that such a brush, when we learn how to produce it properly, will prove a valuable aid in the investigation of the nature of the forces acting in an electrostatic or magnetic field. If there is any motion which is measurable going on in the space, such a brush ought to reveal it. It is, so to speak, a beam of light, frictionless, devoid of inertia.

I think that it may find practical applications in telegraphy. With such a brush it would be possible to send despatches across the Atalantic, for instance, with any speed, since its sensitiveness may be so great that the slightest changes will affect it. If it were possible to make the stream more intense and very narrow, its deflections could be easily photographed.

I have been interested to find whether there is a rotation of the stream itself, or whether there is simply a stress travelling around in the bulb. For this purpose I mounted a light mica fan so that its vanes were in the path of the brush. If the stream itself was rotating, the fan would be spun around. I could produce no distinct rotation of the fan, although I tried the experiment repeatedly; but as the fan exerted a noticeable influence on the stream, and the apparent rotation of the latter was in this case never quite satisfactory, the experiment did not appear to be conclusive.

I have been unable to produce the phenomenon with the

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disruptive discharge coil, although every other of these phenomena can be well produced by it—many, in fact, much better than with coils operated from an alternator.

It may be possible to produce the brush by impulses of one direction, or even by a steady potential, in which case it would be still more sensitive to magnetic influence.

In operating an induction coil with rapidly alternating currents, we realise with astonishment, for the first time, the great importance of the relation of capacity, self-induction, and frequency as regards the general result. The effects of capacity are the most striking, for in these experiments, since the self-induction and frequency both are high, the critical capacity is very small, and need be but slightly varied to produce a very considerable change. The experimenter may bring his body in contact with the terminals of the secondary of the coil, or attach to one or both terminals insulated bodies of very small bulk, such as bulbs, and he may produce a considerable rise or fall of potential, and greatly affect the flow of the current through the primary. In the experiment before shown, in which a brush appears at a wire attached to one terminal, and the wire is vibrated when the experimenter brings his insulated body in contact with the other terminal of the coil, the sudden rise of potential was made evident.

I may show you the behaviour of the coil in another manner which possesses a feature of some interest. I have here a little light fan of aluminum sheet, fastened to a needle and arranged to rotate freely in a metal piece screwed to one of the terminals of the coil. When the coil is set to work, the molecules of the air are rhythmically attracted and repelled. As the force with which they are repelled is greater than that with which they are attracted, it results that there is a repulsion exerted on the surfaces of the fan. If the fan were made simply of a metal sheet, the repulsion would be equal on the opposite sides, and would produce no effect. But if one of the opposing surfaces is screened, or if, generally speaking, the bombardment on this side is weakened in some way or other, there remains the repulsion exerted upon the other, and the fan is set in rotation. The

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screening is best effected by fastening upon one of the opposing sides of the fan insulated conducting coatings, or, if the fan is made in the shape of an ordinary propeller screw, by fastening on one side, and close to it, an insulated metal plate. The static screen may, however, be omitted, and simply a thickness of insulating material fastened to one of the sides of the fan.

To show the behaviour of the coil, the fan may be placed upon the terminal and it will readily rotate when the coil is operated by currents of very high frequency. With a steady potential, of course, and even with alternating currents of very low frequency, it would not turn, because of the very slow exchange of air and, consequently, smaller bombardment; but in the latter case it might turn if the potential were excessive. With a pin wheel, quite the opposite rule holds good: it rotates best with a steady potential, and the effort is the smaller the higher the frequency. Now it is very easy to adjust the conditions so that the potential is normally not sufficient to turn the fan, but that by connecting the other terminal of the coil with an insulated body it rises to a much greater value, so as to rotate the fan, and it is likewise possible to stop the rotation by connecting to the terminal a body of different size, thereby diminishing the potential.

Instead of using the fan in this experiment, we may use the "electric" radiometer with similar effect. But in this case it will be found that the vanes will rotate only at high exhaustion or at ordinary pressures; they will not rotate at moderate pressures, when the air is highly conducting. This curious observation was made conjointly by Professor Crookes and myself. I attribute the result to the high conductivity of the air, the molecules of which then do not act as independent carriers of electric charges, but act all together as a single conducting body. In such case, of course, if there is any repulsion at all of the molecules from the vanes, it must be very small. It is possible, however, that the result is in part due to the fact that the greater part of the discharge passes from the leading-in wire through the highly conducting gas, instead of passing off from the conducting vanes.

In trying the preceding experiment with the electric radio-

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meter the potential should not exceed a certain limit, as then the electrostatic attraction between the vanes and the glass of the bulb may be so great as to stop the rotation.

A most curious feature of alternate currents of high frequencies and potentials is that they enable us to perform many experiments by the use of one wire only. In many respects this feature is of great interest.

In a type of alternate-current motor invented by me some years ago, I produced rotation by inducing, by means of a single alternating current passed through a motor circuit, in the mass or other circuits of the motor, secondary currents, which, jointly with the primary or inducing current, created a moving field of force. A simple but crude form of such a motor is obtained by winding upon an iron core a primary, and close to it a secondary coil, joining the ends of the latter and placing a freely movable metal disc within the influence of the field produced by both. The iron core is employed for obvious reasons, but it is not essential to the operation. To improve the motor, the iron core is made to encircle the armature. Again to improve, the secondary coil is made to overlap partly the primary, so that it cannot free itself from a strong inductive action of the latter, repel it its lines as it may. Once more to improve, the proper difference of phase is obtained between the primary and secondary currents by a condenser, self-induction, resistance, or equivalent windings.

I had discovered, however, that rotation is produced by means of a single coil and core; my explanation of the phenomenon, and leading thought in trying the experiment, being that there must be a true time lag in the magnetisation of the core. I remember the pleasure I had when, in the writings of Professor Ayrton, which came later to my hand, I found the idea of the time lag advocated. Whether there is a true time lag, or whether the retardation is due to eddy-currents circulating in minute paths, must remain an open question, but the fact is that a coil wound upon an iron core and traversed by an alternating current creates a moving field of force, capable of setting an armature in rotation. It is of some interest, in conjunction with the historical Arago experiment, to mention that in lag, or phase, motors I have pro-

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duced rotation in opposite direction to the moving field, which means that in that experiment the magnet may not rotate, or may even rotate in opposite direction to the moving disc. Here, then, is a motor (schematically illustrated in Fig. 17), comprising a coil and iron core, and a freely movable copper disc in proximity to the latter.

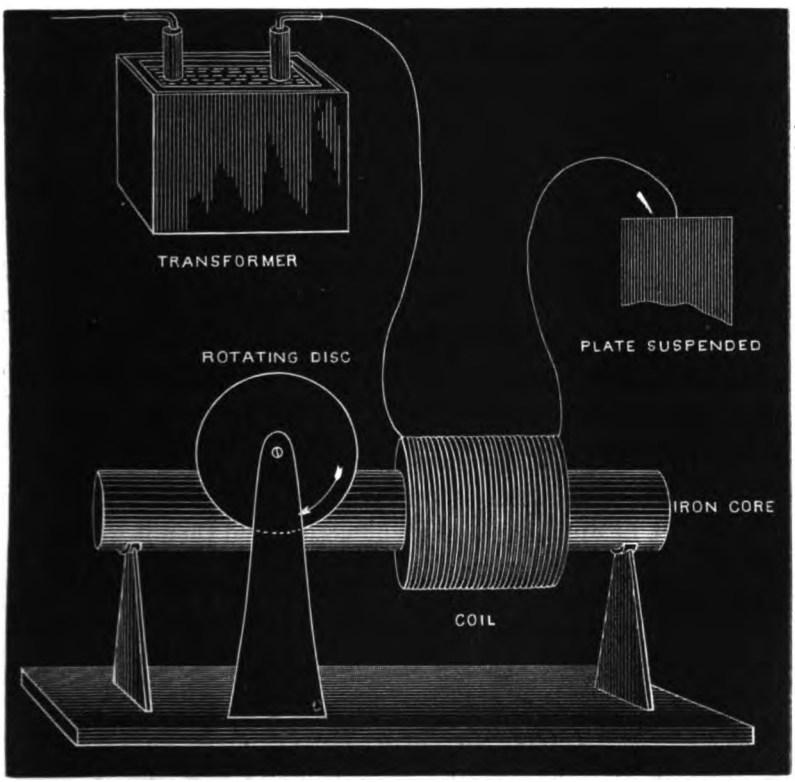

FIG. 17.—Single-Wire and "No-Wire" Motor.

To demonstrate a novel and interesting feature, I have, for a reason which I will explain, selected this type of motor. When the ends of the coil are connected to the terminals of an alternator, the disc is set in rotation. But it is not *this* experiment, now well known, which I desire to perform. What I wish to show you is that this motor rotates with *one single* connection between it and the generator; that is to say, one terminal of

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the motor is connected to one terminal of the generator—in this case the secondary of a high-tension induction coil—the other terminals of motor and generator being insulated in space. To produce rotation it is generally (but not absolutely) necessary to connect the free end of the motor coil to an insulated body of some size. The experimenter's body is more than sufficing. If he touches the free terminal with an object held in the hand, a current passes through the coil and the copper disc is set in rotation. If an exhausted tube is put in series with the coil, the tube lights brilliantly, showing the passage of a strong current. Instead of the experimenter's body, a small metal sheet suspended on a cord may be used with the same result. In this case the plate acts as a condenser in series with the coil. It counteracts the self-induction of the latter and allows a strong current to pass. In such a combination, the greater the self-induction of the coil the smaller need be the plate, and this means that a lower frequency, or eventually a lower potential, is required to operate the motor. A single coil wound upon a core has a high self-induction: for this reason principally, this type of motor was chosen to perform the experiment. Were a secondary closed coil wound upon the core, it would tend to diminish the self-induction, and then it would be necessary to employ a much higher frequency and potential. Neither would be advisable, for a higher potential would endanger the insulation of the small primary coil, and a higher frequency would result in a materially diminished torque.

It should be remarked that when such a motor with a closed secondary is used, it is not at all easy to obtain rotation with excessive frequencies, as the secondary cuts off almost completely the lines of the primary—and this, of course, the more, the higher the frequency—and allows the passage of but a minute current. In such a case, unless the secondary is closed through a condenser, it is almost essential, in order to produce rotation, to make the primary and secondary coils overlap each other more or less.

But there is an additional feature of interest about this motor. It is, namely, not necessary to have even a single connection

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between the motor and generator, except, perhaps, through the ground; for not only is an insulated plate capable of giving off energy into space, but it is likewise capable of deriving it from an alternating electrostatic field, though in the latter case the available energy is much smaller. In this instance one of the motor terminals is connected to the insulated plate or body located within the alternating electrostatic field, and the other terminal preferably to the ground.

It is quite possible, however, that such "no-wire" motors, as they might be called, could be operated by conduction through the rarefied air at considerable distances. Alternate currents, especially of high frequencies, pass with astonishing freedom through even slightly rarefied gases. The upper strata of the air are rarefied. To reach a number of miles out into space requires the overcoming of difficulties of a merely mechanical nature. There is no doubt that with the enormous potentials obtainable by the use of high frequencies and oil insulation luminous discharges might be passed through many miles of rarefied air, and that, by thus directing the energy of many hundreds or thousands of horse-power, motors or lamps might be operated at considerable distances from stationary sources. But such schemes are mentioned merely as possibilities. We shall have no need to transmit power in this way. We shall have no need to *transmit* power at all. Ere many generations pass, our machinery will be driven by a power obtainable at any point of the universe. This idea is not novel. Men have been led to it long ago by instinct or reason. It has been expressed in many ways, and in many places, in the history of old and new. We find it in the delightful myth of Antheus, who derives power from the earth; we find it among the subtile speculations of one of your splendid mathematicians, and in many hints and statements of thinkers of the present time. Throughout space there is energy. Is this energy static or kinetic? If static, our hopes are in vain; if kinetic—and this we know it is, for certain—then it is a mere question of time when men will succeed in attaching their machinery to the very wheelwork of Nature. Of all, living or dead, Crookes came nearest to doing it. His radiometer will turn in the light of day and in the darkness

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of the night; it will turn everywhere where there is heat, and heat is everywhere. But, unfortunately, this beautiful little machine, while it goes down to posterity as the most interesting, must likewise be put on record as the most inefficient machine ever invented!

The preceding experiment is only one of many equally interesting experiments which may be performed by the use of only one wire with alternate currents of high potential and frequency. We may connect an insulated line to a source of such currents, we may pass an inappreciable current over the line, and on any point of the same we are able to obtain a heavy current, capable of fusing a thick copper wire. Or we may, by the help of some artifice, decompose a solution in an electrolytic cell by connecting only one pole of the cell to the line or source of energy. Or we may, by attaching to the line, or only bringing into its vicinity, light up an incandescent lamp, an exhausted tube, or a phosphorescent bulb.

However impracticable this plan of working may appear in many cases, it certainly seems practicable, and even recommendable, in the production of light. A perfected lamp would require but little energy, and if wires are used at all, we ought to be able to supply that energy without a return wire.

It is now a fact that a body may be rendered incandescent or phosphorescent by bringing it either in single contact or merely in the vicinity of a source of electric impulses of the proper character, and that in this manner a quantity of light sufficient to afford a practical illuminant may be produced. It is, therefore, to say the least, worth while to attempt to determine the best conditions and to invent the best appliances for attaining this object.

Some experiences have already been gained in this direction, and I will dwell on them briefly, in the hope that they might prove useful.

The heating of a conducting body enclosed in a bulb, and connected to a source of rapidly alternating electric impulses, is dependent on so many things of a different nature, that it would be difficult to give a generally applicable rule under which the

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maximum heating occurs. As regards the size of the vessel, I have lately found that at ordinary or only slightly differing atmospheric pressures, when air is a good insulator, and hence practically the same amount of energy by a certain potential and frequency is given off from the body, whether the bulb be small or large, the body is brought to a higher temperature if enclosed in a small bulb, because of the better confinement of heat in this case.

At lower pressures, when air becomes more or less conducting, or if the air be sufficiently warmed as to become conducting, the body is rendered more intensely incandescent in a large bulb, obviously because, under otherwise equal conditions of test, more energy may be given off from the body when the bulb is large.

At very high degrees of exhaustion, when the matter in the bulb becomes "radiant," a large bulb has still an advantage, but a comparatively slight one, over the small bulb.

Finally, at excessively high degrees of exhaustion, which cannot be reached except by the employment of special means, there seems to be, beyond a certain and rather small size of vessel, no perceptible difference in the heating.

These observations were the result of a number of experiments, of which one, showing the effect of the size of the bulb at a high degree of exhaustion, may be described and shown here, as it presents a feature of interest. Three spherical bulbs of 2 inches, 3 inches, and 4 inches diameter were taken, and in the centre of each was mounted an equal length of an ordinary incandescent lamp filament of uniform thickness. In each bulb the piece of filament was fastened to the leading-in wire of platinum, contained in a glass stem sealed in the bulb; care being taken, of course, to make everything as nearly alike as possible. On each glass stem in the inside of the bulb was slipped a highly polished tube made of aluminum sheet, which fitted the stem and was held on it by spring pressure. The function of this aluminum tube will be explained subsequently. In each bulb an equal length of filament protruded above the metal tube. It is sufficient to say now that under these conditions equal lengths of filament of the same thickness—in other words, bodies of equal bulk—were brought to

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incandescence. The three bulbs were sealed to a glass tube, which was connected to a Sprengel pump. When a high vacuum had been reached, the glass tube carrying the bulbs was sealed off. A current was then turned on successively on each bulb, and it was found that the filaments came to about the same brightness, and, if anything, the smallest bulb, which was placed midway between the two larger ones, may have been slightly brighter. This result was expected, for when either of the bulbs was connected to the coil the luminosity spread through the other two, hence the three bulbs constituted really one vessel. When all the three bulbs were connected in multiple arc to the coil, in the largest of them the filament glowed brightest, in the next smaller it was a little less bright, and in the smallest it only came to redness. The bulbs were then sealed off and separately tried. The brightness of the filaments was now such as would have been expected on the supposition that the energy given off was proportionate to the surface of the bulb, this surface in each case representing one of the coatings of a condenser. Accordingly, there was less difference between the largest and the middle-sized than between the latter and the smallest bulb.

An interesting observation was made in this experiment. The three bulbs were suspended from a straight bare wire connected to a terminal of the coil, the largest bulb being placed at the end of the wire, at some distance from it the smallest bulb, and an equal distance from the latter the middle-sized one. The carbons glowed then in both the larger bulbs about as expected, but the smallest did not get its share by far. This observation led me to exchange the positions of the bulbs, and I then observed that whichever of the bulbs was in the middle it was by far less bright than it was in any other position. This mystifying result was, of course, found to be due to the elctrostatic action between the bulbs. When they were placed at considerable distance, or when they were attached to the corners of an equilateral triangle of copper wire, they glowed about in the order determined by their surfaces.

As to the shape of the vessel, it is also of some importance, especially at high degrees of exhaustion. Of all the possible

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constructions, it seems that a spherical globe with the refractory body mounted in its centre is the best to employ. In experience it has been demonstrated that in such a globe a refractory body of a given bulk is more easily brought to incandescence than when otherwise shaped bulbs are used. There is also an advantage in giving to the incandescent body the shape of a sphere, for self-evident reasons. In any case, the body should be mounted in the centre, where the atoms rebounding from the glass collide. This object is best attained in the spherical bulb; but it is also attained in a cylindrical vessel with one or two straight filaments coinciding with its axis, and possibly also in parabolical or spherical bulbs with the refractory body or bodies placed in the focus or foci of the same, though the latter is not probable, as the electrified atoms should in all cases rebound normally from the surface they strike, unless the speed were excessive, in which case they *would* probably follow the general law of reflection. No matter what shape the vessel may have, if the exhaustion be low, a filament mounted in the globe is brought to the same degree of incandescence in all parts; but if the exhaustion be high, and the bulb be spherical or pear-shaped, as usual, focal points form, and the filament is heated to a higher degree at or near such points.

To illustrate the effect, I have here two small bulbs which are alike, only one is exhausted to a low and the other to a very high degree. When connected to the coil, the filament in the former glows uniformly throughout all its length; whereas in the latter, that portion of the filament which is in the centre of the bulb glows far more intensely than the rest. A curious point is that the phenomenon occurs even if two filaments are mounted in a bulb, each being connected to one terminal of the coil, and, what is still more curious, if they be very near together, provided the vacuum be very high. I noted in experiments with such bulbs that the filaments would give way usually at a certain point, and in the first trials I attributed it to a defect in the carbon. But when the phenomenon occurred many times in succession I recognised its real cause.

In order to bring a refractory body enclosed in a bulb to

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incandescence, it is desirable, on account of economy, that all the energy supplied to the bulb from the source should reach without loss the body to be heated: from there, and from nowhere else, it should be radiated. It is, of course, out of the question to reach this theoretical result, but it is possible by a proper construction of the illuminating device to approximate it more or less.

For many reasons, the refractory body is placed in the centre of the bulb, and it is usually supported on a glass stem containing the leading-in wire. As the potential of this wire is alternated, the rarefied gas surrounding the stem is acted upon inductively, and the glass stem is violently bombarded and heated. In this manner by far the greater portion of the energy supplied to the bulb—especially when exceedingly high frequencies are used—may be lost for the purpose contemplated. To obviate this loss, or at least to reduce it to a minimum, I usually screen the rarefied gas surrounding the stem from the inductive action of the leading-in wire by providing the stem with a tube or coating of conducting material. It seems beyond doubt that the best among metals to employ for this purpose is aluminum, on account of its many remarkable properties. Its only fault is that it is easily fusible, and therefore its distance from the incandescing body should be properly estimated. Usually, a thin tube, of a diameter somewhat smaller than that of the glass stem, is made of the finest aluminum sheet, and slipped on the stem. The tube is conveniently prepared by wrapping around a rod fastened in a lathe a piece of aluminum sheet of the proper size, grasping the sheet firmly with clean chamois leather or blotting paper, and spinning the rod very fast. The sheet is wound tightly around the rod, and a highly polished tube of two or three layers of the sheet is obtained. When slipped on the stem, the pressure is generally sufficient to prevent it from slipping off, but, for safety, the lower edge of the sheet may be turned inside. The upper inside corner of the sheet—that is, the one which is nearest to the refractory incandescent body—should be cut out diagonally, as it often happens that, in consequence of the intense heat, this corner turns towards the inside and comes very near

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to, or in contact with, the wire, or filament, supporting the refractory body. The greater part of the energy supplied to the bulb is then used up in heating the metal tube, and the bulb is rendered useless for the purpose. The aluminum sheet should project above the glass stem more or less—1 inch or so—or else, if the glass be too close to the incandescing body, it may be strongly heated, and become more or less conducting, whereupon it may be ruptured, or may, by its conductivity, establish a good electrical connection between the metal tube and the leading-in wire, in which case, of course, again, most of the energy will be lost in heating the former. Perhaps the best way is to make the top of the glass tube, for about an inch, of a much smaller diameter. To still further reduce the danger arising from the heating of the glass stem, and also with the view of preventing an electrical connection between the metal tube and the electrode, I preferably wrap the stem with several layers of thin mica, which extends at least as far as the metal tube. In some bulbs I have also used an outside insulating cover.

The preceding remarks are only made to aid the experimenter in the first trials, for the difficulties which he encounters he may soon find means to overcome in his own way.

To illustrate the effect of the screen, and the advantage of using it, I have here two bulbs of the same size, with their stems, leading-in wires, and incandescent lamp filaments tied to the latter, as nearly alike as possible. The stem of one bulb is provided with an aluminum tube, the stem of the other has none. Originally the two bulbs were joined by a tube which was connected to a Sprengel pump. When a high vacuum had been reached, first the connecting tube, and then the bulbs, were sealed off; they are therefore of the same degree of exhaustion. When they are separately connected to the coil giving a certain potential, the carbon filament in the bulb provided with the aluminum screen is rendered highly incandescent, while the filament in the other bulb may, with the same potential, not even come to redness, although in reality the latter bulb takes generally more energy than the former. When they are both connected together to the terminal, the difference is even more

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apparent, showing the importance of the screening. The metal tube placed on the stem containing the leading-in wire performs really two distinct functions: first, it acts more or less as an electrostatic screen, thus economising the energy supplied to the bulb; and, second, to whatever extent it may fail to act electrostatically, it acts mechanically, preventing the bombardment, and consequently intense heating and possible deterioration, of the slender support of the refractory incandescent body, or of the glass stem containing the leading-in wire. I say *slender* support, for it is evident that in order to confine the heat more completely to the incandescing body its support should be very thin, so as to carry away the smallest possible amount of heat by conduction. Of all the supports used I have found an ordinary incandescent lamp filament to be the best, principally because, among conductors, it can withstand the highest degrees of heat.

The effectiveness of the metal tube as an electrostatic screen depends largely on the degree of exhaustion.

At excessively high degrees of exhaustion—which are reached by using great care and special means in connection with the Sprengel pump—when the matter in the globe is in the ultra-radiant state, it acts most perfectly. The shadow of the upper edge of the tube is then sharply defined upon the bulb.

At a somewhat lower degree of exhaustion, which is about the ordinary "non-striking" vacuum, and generally as long as the matter moves predominantly in straight lines, the screen still does well. In elucidation of the preceding remark it is necessary to state that what is a "non-striking" vacuum for a coil operated, as ordinarily, by impulses, or currents, of low frequency, is not, by far, so when the coil is operated by currents of very high frequency. In such case the discharge may pass with great freedom through the rarefied gas through which a low-frequency discharge may not pass, even though the potential be much higher. At ordinary atmospheric pressures just the reverse rule holds good: the higher the frequency, the less the spark discharge is able to jump between the terminals, especially if they are knobs or spheres of some size.

Finally, at very low degrees of exhaustion, when the gas is

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well conducting, the metal tube not only does not act as an electrostatic screen, but even is a drawback, aiding to a considerable extent the dissipation of the energy laterally from the leading-in wire. This, of course, is to be expected. In this case, namely, the metal tube is in good electrical connection with the leading-in wire, and most of the bombardment is directed upon the tube. As long as the electrical connection is not good, the conducting tube is always of some advantage, for although it may not greatly economise energy, still it protects the support of the refractory button, and is a means for concentrating more energy upon the same.

To whatever extent the aluminum tube performs the function of a screen, its usefulness is therefore limited to very high degrees of exhaustion when it is insulated from the electrode—that is, when the gas as a whole is non-conducting, and the molecules, or atoms, act as independent carriers of electric charges.

In addition to acting as a more or less effective screen, in the true meaning of the word, the conducting tube or coating may also act, by reason of its conductivity, as a sort of equaliser or dampener of the bombardment against the stem. To be explicit, I assume the action as follows:—Suppose a rhythmical bombardment to occur against the conducting tube by reason of its imperfect action as a screen, it certainly must happen that some molecules, or atoms, strike the tube sooner than others. Those which come first in contact with it give up their superfluous charge, and the tube is electrified, the electrification instantly spreading over its surface. But this must diminish the energy lost in the bombardment, for two reasons: firstly, the charge given up by the atoms spreads over a great area, and hence the electric density at any point is small, and the atoms are repelled with less energy than they would be if they would strike against a good insulator; secondly, as the tube is electrified by the atoms which first come in contact with it, the progress of the following atoms against the tube is more or less checked by the repulsion which the electrified tube must exert upon the similarly electrified atoms. This repulsion may perhaps be sufficient to prevent a large portion of the atoms from striking the tube, but at any rate it must diminish

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the energy of their impact. It is clear that when the exhaustion is very low, and the rarefied gas well conducting, neither of the above effects can occur, and, on the other hand, the fewer the atoms, with the greater freedom they move; in other words, the higher the degree of exhaustion, up to a limit, the more telling will be both the effects.

What I have just said may afford an explanation of the phenomenon observed by Professor Crookes, namely, that a discharge through a bulb is established with much greater facility when an insulator than when a conductor is present in the same. In my opinion, the conductor acts as a dampener of the motion of the atoms in the two ways pointed out; hence, to cause a visible discharge to pass through the bulb, a much higher potential is needed if a conductor, especially of much surface, be present.

For the sake of clearness of some of the remarks before made, I must now refer to Figs. 18, 19, and 20, which illustrate various arrangements with a type of bulb most generally used.

Fig. 18 is a section through a spherical bulb, L, with the glass stem, s, containing the leading-in wire, w, which has a lamp filament, l, fastened to it, serving to support the refractory button, m, in the centre. M is a sheet of thin mica wound in several layers around the stem s, and a is the aluminum tube.

Fig. 19 illustrates such a bulb in a somewhat more advanced stage of perfection. A metallic tube, S, is fastened by means of some cement to the neck of the tube. In the tube is screwed a plug, P, of insulating material, in the centre of which is fastened a metallic terminal, t, for the connection to the leading-in wire, w. This terminal must be well insulated from the metal tube S, therefore, if the cement used is conducting—and most generally it is sufficiently so—the space between the plug P and the neck of the bulb should be filled with some good insulating material, as mica powder.

Fig. 20 shows a bulb made for experimental purposes. In this bulb the aluminum tube is provided with an external connection, which serves to investigate the effect of the tube under various conditions. It is referred to chiefly to suggest a line of experiment followed.

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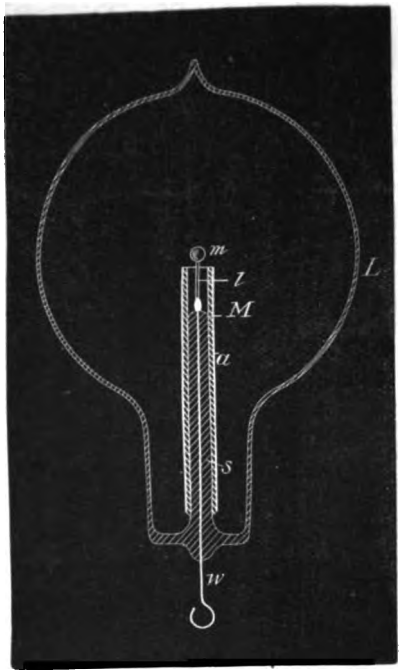

FIG. 18.—Bulb with Mica Tube and Aluminum Screen.

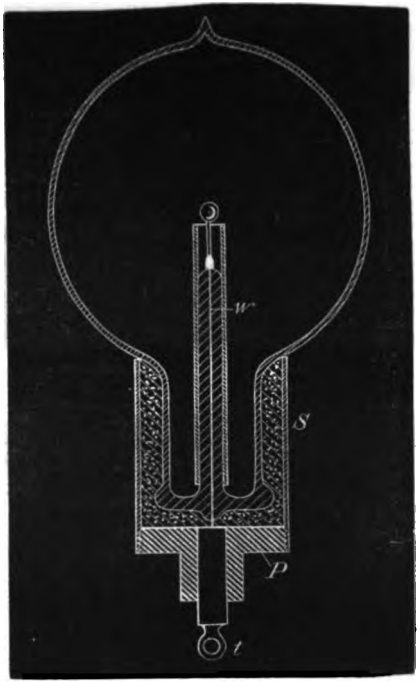

FIG. 19.—Improved Bulb with Socket and Screen.

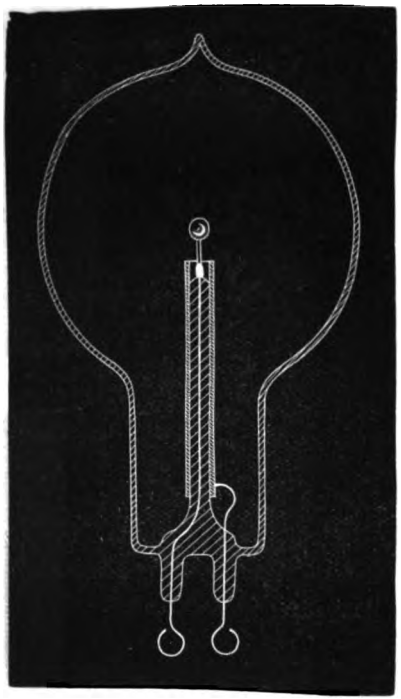

FIG. 20.—Bulb for Experiments with Conducting Tube.

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Since the bombardment against the stem containing the leading-in wire is due to the inductive action of the latter upon the rarefied gas, it is of advantage to reduce this action as far as practicable by employing a very thin wire, surrounded by a very thick insulation of glass or other material, and by making the wire passing through the rarefied gas as short as practicable. To combine these features I employ a large tube, T (Fig. 21), which

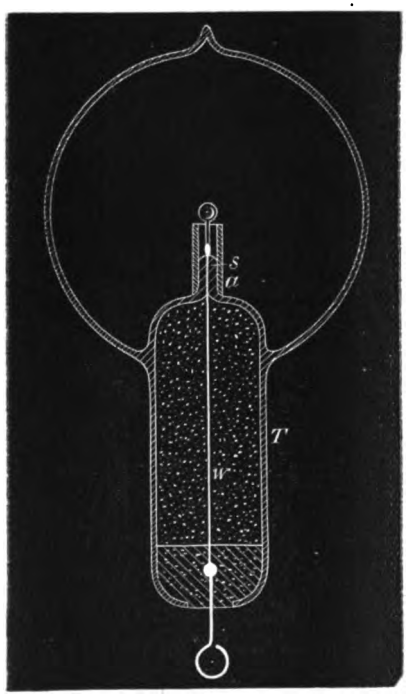

Fig. 21.—Iproved Bulb with Non-conducting Button.

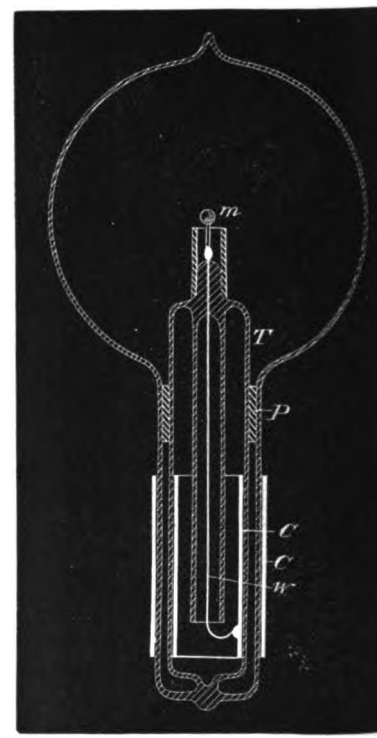

Fig. 22.—Type of Bulb without Leading-in Wire.

protrudes into the bulb to some distance, and carries on the top a very short glass stem, s, into which is sealed the leading-in wire, w, and I protect the top of the glass stem against the heat by a small aluminum tube, a, and a layer of mica underneath the same, as usual. The wire w, passing through the large tube to the outside of the bulb, should be well insulated—with a glass tube,

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for instance—and the space between ought to be filled out with some excellent insulator. Among many insulating powders I have tried, I have found that mica powder is the best to employ. If this precaution is not taken, the tube *T*, protruding into the bulb, will surely be cracked in consequence of the heating by the brushes, which are apt to form in the upper part of the tube, near the exhausted globe, especially if the vacuum be excellent, and therefore the potential necessary to operate the lamp very high.

Fig. 22 illustrates a similar arrangement, with a large tube, *T*, protruding into the part of the bulb containing the refractory button, *m*. In this case the wire leading from the outside into the bulb is omitted, the energy required being supplied through condenser coatings, *C C*. The insulating packing, *P*, should in this construction be tightly fitting to the glass, and rather wide, or otherwise the discharge might avoid passing through the wire, *w*, which connects the inside condenser coating to the incandescent button, *m*.

The molecular bombardment against the glass stem in the bulb is a source of great trouble. As illustration I will cite a phenomenon only too frequently and unwillingly observed. A bulb, preferably a large one, may be taken, and a good conducting body, such as a piece of carbon, may be mounted in it upon a platinum wire sealed in the glass stem. The bulb may be exhausted to a fairly high degree, nearly to the point when phosphorescence begins to appear. When the bulb is connected with the coil, the piece of carbon, if small, may become highly incandescent at first, but its brightness immediately diminishes, and then the discharge may break through the glass somewhere in the middle of the stem, in the form of bright sparks, in spite of the fact that the platinum wire is in good electrical connection with the rarefied gas through the piece of carbon or metal at the top. The first sparks are singularly bright, recalling those drawn from a clear surface of mercury. But, as they heat the glass rapidly, they of course lose their brightness, and cease when the glass at the ruptured place becomes incandescent, or generally sufficiently hot to conduct. When observed for the first time the phenomenon must appear very curious, and shows in a striking manner

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how radically different alternate currents, or impulses, of high frequency behave, as compared with steady currents, or currents of low frequency. With such currents—namely, the latter—the phenomenon would of course not occur. When frequencies such as are obtained by mechanical means are used, I think that the rupture of the glass is more or less the consequence of the bombardment, which warms it up and impairs its insulating power; but with frequencies obtainable with condensers, I have no doubt that the glass may give way without previous heating. Although this appears most singular at first, it is in reality what we might expect to occur. The energy supplied to the wire leading into the bulb is given off partly by direct action through the carbon button, and partly by inductive action through the glass surrounding the wire. The case is thus analogous to that in which a condenser shunted by a conductor of low resistance is connected to a source of alternating currents. As long as the frequencies are low, the conductor gets the most, and the condenser is perfectly safe; but when the frequency becomes excessive, the *rôle* of the conductor may become quite insignificant. In the latter case the difference of potential at the terminals of the condenser may become so great as to rupture the dielectric, notwithstanding the fact that the terminals are joined by a conductor of low resistance.

It is, of course, not necessary, when it is desired to produce the incandescence of a body enclosed in a bulb by means of these currents, that the body should be a conductor, for even a perfect non-conductor may be quite as readily heated. For this purpose it is sufficient to surround a conducting electrode with the non-conducting material, as, for instance, in the bulb described before in Fig. 21, in which a thin incandescent lamp filament is coated with a non-conductor, and supports a button of the same material on the top. At the start the bombardment goes on by inductive action through the non-conductor, until the same is sufficiently heated to become conducting, when the bombardment continues in the ordinary way.

A different arrangement used in some of the bulbs constructed is illustrated in Fig. 23. In this instance a non-

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conductor, m, is mounted in a piece of common arc light carbon so as to project some small distance above the latter.

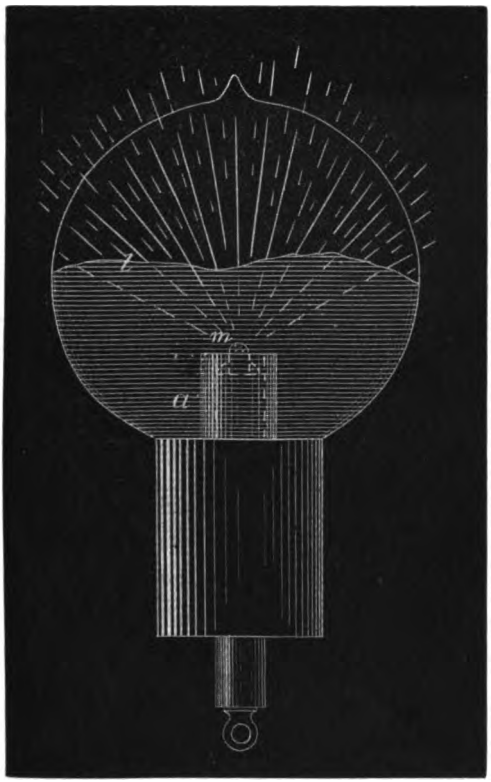

Fig. 23.—Effect produced by a Ruby Drop.

The carbon piece is connected to the leading-in wire passing through a glass stem, which is wrapped with several layers of mica. An aluminum tube, a, is employed, as usual for screening. It is so arranged that it reaches very nearly as high as the carbon, and only the non-conductor, m, projects a little above it. The bombardment goes at first against the upper surface of carbon, the lower parts being protected by the aluminum tube. As soon, however, as the non-conductor, m, is heated, it is rendered good conducting, and then it becomes the centre of the bombardment, being most exposed to the same.

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I have also constructed during these experiences many such single-wire bulbs with or without internal electrode, in which the radiant matter was projected against, or focused upon, the body to be rendered incandescent. Fig. 24 illustrates one of the

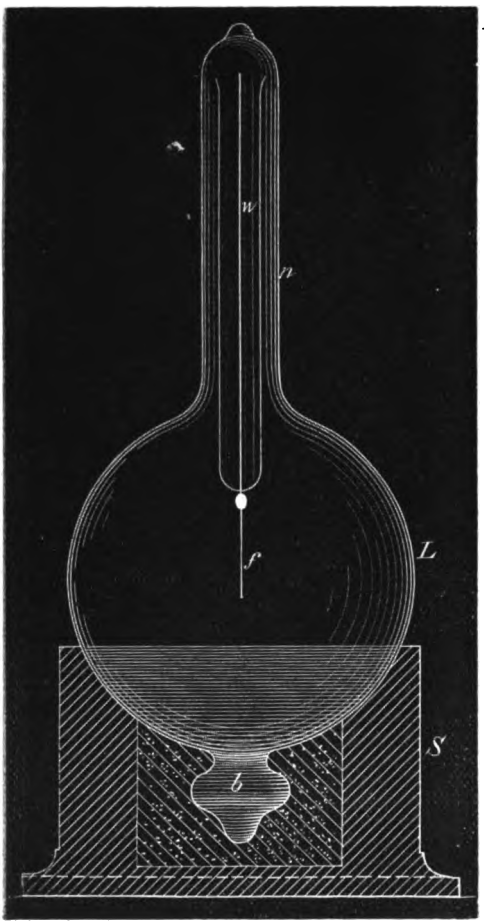

FIG. 24.—BULB WITHOUT LEADING-IN WIRE, SHOWING EFFECT OF PROJECTED MATTER.

bulbs used. It consists of a spherical globe, *L*, provided with a long neck, *n*, on the top, for increasing the action in some cases by the application of an external conducting coating. The globe *L* is blown out on the bottom into a very small bulb, *b*, which

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serves to hold it firmly in a socket, S, of insulating material into which it is cemented. A fine lamp filament, f, supported on a wire, w, passes through the centre of the globe L. The filament is rendered incandescent in the middle portion, where the bombardment proceeding from the lower inside surface of the globe is most intense. The lower portion of the globe, as far as the socket S reaches, is rendered conducting, either by a tinfoil coating or otherwise, and the external electrode is connected to a terminal of the coil.

The arrangement diagramatically indicated in Fig. 24 was found to be an inferior one when it was desired to render incandescent a filament or button supported in the centre of the globe, but it was convenient when the object was to excite phosphorescence.

In many experiments in which bodies of a different kind were mounted in the bulb as, for instance, indicated in Fig. 23, some observations of interest were made.

It was found, among other things, that in such cases, no matter where the bombardment began, just as soon as a high temperature was reached there was generally one of the bodies which seemed to take most of the bombardment upon itself, the other, or others, being thereby relieved. This quality appeared to depend principally on the point of fusion, and on the facility with which the body was "evaporated," or, generally speaking, disintegrated—meaning by the latter term not only the throwing off of atoms, but likewise of larger lumps. The observation made was in accordance with generally accepted notions. In a highly exhausted bulb electricity is carried off from the electrode by independent carriers, which are partly the atoms, or molecules, of the residual atmosphere, and partly the atoms, molecules, or lumps thrown off from the electrode. If the electrode is composed of bodies of different character, and if one of these is more easily disintegrated than the others, most of the electricity supplied is carried off from that body, which is then brought to a higher temperature than the others, and this the more, as upon an increase of the temperature the body is still more easily disintegrated.

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It seems to me quite probable that a similar process takes place in the bulb even with a homogeneous electrode, and I think it to be the principal cause of the disintegration. There is bound to be some irregularity, even if the surface is highly polished, which, of course, is impossible with most of the refractory bodies employed as electrodes. Assume a point of the electrode gets hotter, instantly most of the discharge passes through that point, and a minute patch is probably fused and evaporated. It is now possible that in consequence of the violent disintegration the spot attacked sinks in temperature, or that a counter-force is created, as in an arc; at any rate, the local tearing-off meets with the limitations incident to the experiment, whereupon the same process occurs on another place. To the eye the electrode appears uniformly brilliant, but there are upon it points, constantly shifting and wandering around, of a temperature far above the mean, and this materially hastens the process of deterioration. That some such thing occurs, at least when the electrode is at a lower temperature, sufficient experimental evidence can be obtained in the following manner:—Exhaust a bulb to a very high degree, so that with a fairly high potential the discharge cannot pass—that is, not a *luminous* one, for a weak invisible discharge occurs always, in all probability. Now raise slowly and carefully the potential, leaving the primary current on no more than for an instant. At a certain point, two, three, or half a dozen phosphorescent spots will appear on the globe. These places of the glass are evidently more violently bombarded than others, this being due to the unevenly distributed electric density, necessitated, of course, by sharp projections, or, generally speaking, irregularities of the electrode. But the luminous patches are constantly changing in position, which is especially well observable if one manages to produce very few, and this indicates that the configuration of the electrode is rapidly changing.

From experiences of this kind I am led to infer that, in order to be most durable, the refractory button in the bulb should be in the form of a sphere, with a highly polished surface. Such a small sphere could be manufactured from a diamond or some

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other crystal, but a better way would be to fuse, by the employment of extreme degrees of temperature, some oxide—as, for instance, zirconia—into a small drop, and then keep it in the bulb at a temperature somewhat below its point of fusion.

Interesting and useful results can no doubt be reached in the direction of extreme degrees of heat. How can such high temperatures be arrived at? How are the highest degrees of heat reached in Nature? By the impact of stars, by high speeds and collisions. In a collision any rate of heat generation may be attained. In a chemical process we are limited. When oxygen and hydrogen combine, they fall, metaphorically speaking, from a definite height. We cannot go very far with a blast, nor by confining heat in a furnace, but in an exhausted bulb we can concentrate any amount of energy upon a minute button. Leaving practicability out of consideration, this, then, would be the means which, in my opinion, would enable us to reach the highest temperature. But a great difficulty, when proceeding in this way, is encountered: namely, in most cases the body is carried off before it can fuse and form a drop. This difficulty exists principally with an oxide such as zirconia, because it cannot be compressed in so hard a cake that it would not be carried off quickly. I endeavoured repeatedly to fuse zirconia, placing it in a cup or arc light carbon as indicated in Fig. 23. It glowed with a most intense light, and the stream of the particles projected out of the carbon cup was of a vivid white; but, whether it was compressed in a cake or made into a paste with carbon, it was carried off before it could be fused. The carbon cup containing the zirconia had to be mounted very low in the neck of a large bulb, as the heating of the glass by the projected particles of the oxide was so rapid that in the first trial the bulb was cracked almost in an instant when the current was turned on. The heating of the glass by the projected particles was found to be always greater when the carbon cup contained a body which was rapidly carried off—I presume because in such cases, with the same potential, higher speeds were reached, and also because, per unit of time, more matter was projected—that is, more particles would strike the glass.

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The before-mentioned difficulty did not exist, however, when the body mounted in the carbon cup offered great resistance to deterioration. For instance, when an oxide was first fused in an oxygen blast and then mounted in the bulb, it melted very readily into a drop.

Generally during the process of fusion magnificent light effects were noted, of which it would be difficult to give an adequate idea. Fig. 23 is intended to illustrate the effect observed with a ruby drop. At first one may see a narrow funnel of white light projected against the top of the globe, where it produces an irregularly outlined phosphorescent patch. When the point of the ruby fuses the phosphorescence becomes very powerful; but as the atoms are projected with much greater speed from the surface of the drop, soon the glass gets hot and "tired," and now only the outer edge of the patch glows. In this manner an intensely phosphorescent, sharply defined line, *l*, corresponding to the outline of the drop, is produced, which spreads slowly over the globe as the drop gets larger. When the mass begins to boil, small bubbles and cavities are formed, which cause dark-coloured spots to sweep across the globe. The bulb may be turned downwards without fear of the drop falling off, as the mass possesses considerable viscosity.

I may mention here another feature of some interest, which I believe to have noted in the course of these experiments, though the observations do not amount to a certitude. It *appeared* that under the molecular impact caused by the rapidly alternating potential the body was fused, and maintained in that state at a lower temperature in a highly exhausted bulb than was the case at normal pressure and application of heat in the ordinary ways—that is, at least, judging from the quantity of the light emitted. One of the experiments performed may be mentioned here by way of illustration. A small piece of pumice-stone was stuck on a platinum wire, and first melted to it in a gas burner. The wire was next placed between two pieces of charcoal and a burner applied so as to produce an intense heat, sufficient to melt down the pumice-stone into a small glass-like button. The platinum wire had to be taken of sufficient thickness to prevent

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its melting in the fire. While in the charcoal fire, or when held in a burner to gain a better idea of the degree of heat, the button glowed with great brilliancy. The wire with the button was then mounted in a bulb, and upon exhausting the same to a high degree the current was turned on slowly so as to prevent the cracking of the button. The button was heated to the point of fusion, and when it melted it did not, apparently, glow with the same brilliancy as before; and this would indicate a lower temperature. Leaving out of consideration the observer's possible, and even probable, error, the question is, can a body under these conditions be brought from a solid to a liquid state with evolution of *less* light?

When the potential of a body is rapidly alternated it is certain that the structure is jarred. When the potential is very high, although the vibrations may be few—say 20,000 per second—the effect upon the structure may be considerable. Suppose, for example, that a ruby is melted into a drop by a steady application of energy. When it forms a drop, it will emit visible and invisible waves, which will be in a definite ratio, and to the eye the drop will appear to be of a certain brilliancy. Nex' suppose we diminish to any degree we choose the energy ste supplied, and, instead, supply energy which rises and falls a/ ng to a certain law. Now, when the drop is formed, will be emitted from it three different kinds of vibrations— ordinary visible, and two kinds of invisible waves: that is, ordinary dark waves of all lengths, and, in addition, waves of a ell-defined character. The latter would not exist by a steady upply of the energy, still they help to jar and loosen the structure. If this really be the case, then the ruby drop will emit relatively less visible and more invisible waves than before. Thus it would seem that when a platinum wire, for instance, is fused by currents alternating with extreme rapidity, it emits at the point of fusion less light and more invisible radiation than it does when melted by a steady current, though the total energy used up in the process of fusion is the same in both cases. Or, to cite another example, a lamp filament is not capable of withstanding as long with currents of extreme frequency as it does with steady currents,

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assuming that it be worked at the same luminous intensity. This means that for rapidly alternating currents the filament should be shorter and thicker. The higher the frequency—that is, the greater the departure from the steady flow—the worse it would be for the filament. But if the truth of this remark were demonstrated, it would be erroneous to conclude that such a refractory button as used in these bulbs would be deteriorated quicker by currents of extremely high frequency than by steady or low-frequency currents. From experience I may say that just the opposite holds good: the button withstands the bombardment better with currents of very high frequency. But this is due to the fact that a high-frequency discharge passes through a rarefied gas with much greater freedom than a steady or low-frequency discharge, and this will say that with the former we can work with a lower potential or with a less violent impact. As long, then, as the gas is of no consequence, a steady or low-frequency current is better; but as soon as the action of the gas is desired and important, high frequencies are preferable.

In the course of these experiences great many trials were made with all kinds of carbon buttons. Electrodes made of ordinary carbon buttons were decidedly more durable when the buttons were obtained by the application of enormous pressure. Electrodes prepared by depositing carbon in well-known ways did not show up well: they blackened the globe very quickly. From many experiences I conclude that lamp filaments obtained in this manner can be advantageously used only with low potentials and low-frequency currents. Some kinds of carbon withstand so well that, in order to bring them to the point of fusion, it is necessary to employ very small buttons. In this case the observation is rendered very difficult, on account of the intense light produced. Nevertheless, there can be no doubt that all kinds of carbon are fused under the molecular bombardment, but the liquid state must be one of great instability. Of all the bodies tried, there were two which withstood best—diamond and carborundum. These two showed up about equally, but the latter was preferable, for many reasons. As it is more than likely that this body is not yet generally known, I will venture to call your attention to it.

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It has been recently produced by Mr. E. G. Acheson, of Monongahela City, Pennsylvania, U.S.A. It is intended to replace ordinary diamond powder for polishing precious stones, &c., and I have been informed that it accomplishes this object quite successfully. I do not know why the name "carborundum" has been given to it, unless there is something in the process of its manufacture which justifies this selection. Through the kindness of the inventor, I obtained a short while ago some samples which I desired to test in regard to their qualities of phosphorescence and capability of withstanding high degrees of heat.

Carborundum can be obtained in two forms—in the form of "crystals" and of powder. The former appear to the naked eye dark-coloured, but are very brilliant; the latter is of nearly the same colour as ordinary diamond powder, but very much finer. When viewed under a microscope, the samples of crystals given to me did not appear to have any definite form, but rather resembled pieces of broken-up egg coal of fine quality. The majority were opaque, but there were some which were transparent and coloured. The crystals are a kind of carbon containing some impurities; they are extremely hard, and withstand for a long time even an oxygen blast. When the blast is directed against them, they at first form a cake of some compactness, probably in consequence of the fusion of impurities they contain. The mass withstands for a very long time the blast without further fusion; but a slow carrying off, or burning, occurs, and finally a small quantity of a glass-like residue is left, which, I suppose, is melted alumina. When compressed strongly they conduct very well, but not as well as ordinary carbon. The powder, which is obtained from the crystals in some way, is practically non-conducting. It affords a magnificent polishing material for stones.

The time has been too short to make a satisfactory study of the properties of this product, but enough experience has been gained in a few weeks I have experimented upon it to say that it does possess some remarkable properties in many respects. It withstands excessively high degrees of heat, it is little deteriorated

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by molecular bombardment. and it does not blacken the globe as ordinary carbon does. The only difficulty which I have found in its use in connection with these experiments was to find some binding material which would resist the heat and the effect of the bombardment as successfully as carborundum itself does.

I have here a number of bulbs which I have provided with buttons of carborundum. To make such a button of carborundum crystals I proceed in the following manner:—I take an ordinary lamp filament and dip its point in tar, or some other thick substance or paint which may be readily carbonised. I next pass the point of the filament through the crystals, and then hold it vertically over a hot plate. The tar softens and forms a drop on the point of the filament, the crystals adhering to the surface of the drop. By regulating the distance from the plate the tar is slowly dried out and the button becomes solid. I then once more dip the button in tar and hold it again over a plate until the tar is evaporated, leaving only a hard mass which firmly binds the crystals. When a larger button is required, I repeat the process several times, and I generally also cover the filament a certain distance below the button with crystals. The button being mounted in a bulb, when a good vacuum has been reached, first a weak, and then a strong discharge is passed through the bulb to carbonise the tar and expel all gases, and later it is brought to a very intense incandescence.

When the powder is used I have found it best to proceed as follows:—I make a thick paint of carborundum and tar, and pass a lamp filament through the paint. Taking then most of the paint off by rubbing the filament against a piece of chamois leather, I hold it over a hot plate until the tar evaporates and the coating becomes firm. I repeat this process as many times as it is necessary to obtain a certain thickness of coating. On the point of the coated filament I form a button in the same manner.

There is no doubt that such a button—properly prepared under great pressure—of carborundum, especially of powder of the best quality, will withstand the effect of the bombardment fully as good as anything we know. The difficulty is that the binding material gives way, and the carborundum is slowly thrown off

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after some time. As it does not seem to blacken the globe in the least, it might be found useful for coating the filaments of ordinary incandescent lamps, and I think that it is even possible to produce thin threads or sticks of carborundum which will replace the ordinary filaments in an incandescent lamp. A carborundum coating seems to be more durable than other coatings, not only because the carborundum can withstand high degrees of heat, but also because it seems to unite with the carbon better than any other material I have tried. A coating of zirconia or any other oxide, for instance, is far more quickly destroyed. I prepared buttons of diamond dust in the same manner as of carborundum, and these came in durability nearest to those prepared of carborundum, but the binding paste gave way much more quickly in the diamond buttons: this, however, I attributed to the size and irregularity of the grains of the diamond.

It was of interest to find whether carborundum possesses the quality of phosphorescence. One is, of course, prepared to encounter two difficulties: first, as regards the rough product, the "crystals," they are good conducting, and it is a fact that conductors do not phosphoresce; second, the powder, being exceedingly fine, would not be apt to exhibit very prominently this quality, since we know that when crystals, even such as diamond or ruby, are finely powdered they lose the property of phosphorescence to a considerable degree.

The question presents itself here, Can a conductor phosphoresce? What is there in such a body as a metal, for instance, that would deprive it of the quality of phosphorescence, unless it is that property which characterises it as a conductor? for it is a fact that most of the phosphorescent bodies lose that quality when they are sufficiently heated to become more or less conducting. Then, if a metal be in a large measure, or perhaps entirely, deprived of that property, it should be capable of phosphorescence. Therefore it is quite possible that at some extremely high frequency, when behaving practically as a non-conductor, a metal or any other conductor might exhibit the quality of phosphorescence, even though it be entirely incapable of phosphorescing under the impact of a low-frequency discharge. There is,

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however, another possible way how a conductor might at least *appear* to phosphoresce.

Considerable doubt still exists as to what really is phosphorescence, and as to whether the various phenomena comprised under this head are due to the same causes. Suppose that in an exhausted bulb, under the molecular impact, the surface of a piece of metal or other conductor is rendered strongly luminous, but at the same time it is found that it remains comparatively cool, would not this luminosity be called phosphorescence? Now such a result, theoretically at least, is possible, for it is a mere question of potential, or speed. Assume the potential of the electrode, and consequently the speed of the projected atoms, to be sufficiently high, the surface of the metal piece against which the atoms are projected would be rendered highly incandescent, since the process of heat generation would be incomparably faster than that of radiating or conducting away from the surface of the collision. In the eye of the observer, a single impact of the atoms would cause an instantaneous flash; but if the impacts were repeated with sufficient rapidity, they would produce a continuous impression upon his retina. To him then the surface of the metal would appear continuously incandescent and of constant luminous intensity, while in reality the light would be either intermittent or at least changing periodically in intensity. The metal piece would rise in temperature until equilibrium was attained—that is, until the energy continuously radiated would equal that intermittently supplied. But the supplied energy might under such conditions not be sufficient to bring the body to any more than a very moderate mean temperature, especially if the frequency of the atomic impacts be very low—just enough that the fluctuation of the intensity of the light emitted could not be detected by the eye. The body would now, owing to the manner in which the energy is supplied, emit a strong light, and yet be at a comparatively very low mean temperature. How could the observer call the luminosity thus produced? Even if the analysis of the light would teach him something definite, still he would probably rank it under the phenomena of phosphorescence. It is conceivable that in such a way both con-

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ducting and non-conducting bodies may be maintained at a certain luminous intensity, but the energy required would very greatly vary with the nature and properties of the bodies.

These and some foregoing remarks of a speculative nature were made merely to bring out curious features of alternate currents or electric impulses. By their help we may cause a body to emit *more* light, while at a certain mean temperature, than it would emit if brought to that temperature by a steady supply; and, again, we may bring a body to the point of fusion, and cause it to emit *less* light than when fused by the application of energy in ordinary ways. It all depends on how we supply the energy, and what kind of vibrations we set up: in one case the vibrations are more, in the other less, adapted to affect our sense of vision.

Some effects, which I had not observed before, obtained with carborundum in the first trials, I attributed to phosphorescence, but in subsequent experiments it appeared that it was devoid of that quality. The crystals possess a noteworthy feature. In a bulb provided with a single electrode in the shape of a small circular metal disc, for instance, at a certain degree of exhaustion the electrode is covered with a milky film, which is separated by a dark space from the glow filling the bulb. When the metal disc is covered with carborundum crystals, the film is far more intense, and snow-white. This I found later to be merely an effect of the bright surface of the crystals, for when an aluminum electrode was highly polished it exhibited more or less the same phenomenon. I made a number of experiments with the samples of crystals obtained, principally because it would have been of special interest to find that they are capable of phosphorescence, on account of their being conducting. I could not produce phosphorescence distinctly, but I must remark that a decisive opinion cannot be formed until other experimenters have gone over the same ground.

The powder behaved in some experiments as though it contained alumina, but it did not exhibit with sufficient distinctness the red of the latter. Its dead colour brightens considerably under the molecular impact, but I am now convinced it does not

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phosphoresce. Still, the tests with the powder are not conclusive, because powdered carborundum probably does not behave like a phosphorescent sulphide, for example, which could be finely powdered without impairing the phosphorescence, but rather like powdered ruby or diamond, and therefore it would be necessary, in order to make a decisive test, to obtain it in a large lump and polish up the surface.

If the carborundum proves useful in connection with these and similar experiments, its chief value will be found in the production of coatings, thin conductors, buttons, or other electrodes capable of withstanding extremely high degrees of heat.

The production of a small electrode capable of withstanding enormous temperatures I regard as of the greatest impo.tance in the manufacture of light. It would enable us to obtain, by means of currents of very high frequencies, certainly twenty times, if not more, the quantity of light which is obtained in the present incandescent lamp by the same expenditure of energy. This estimate may appear to many exaggerated, but in reality I think it is far from being so. As this statement might be misunderstood, I think it necessary to expose clearly the problem with which in this line of work we are confronted, and the manner in which, in my opinion, a solution will be arrived at.

Anyone who begins a study of the problem will be apt to think that what is wanted in a lamp with an electrode is a very high degree of incandescence of the electrode. There he will be mistaken. The high incandescence of the button is a necessary evil, but what is really wanted is the high incandescence of the gas surrounding the button. In other words, the problem in such a lamp is to bring a mass of gas to the highest possible incandescence. The higher the incandescence, the quicker the mean vibration, the greater is the economy of the light production. But to maintain a mass of gas at a high degree of incandescence in a glass vessel, it will always be necessary to keep the incandescent mass away from the glass—that is, to confine it as much as possible to the central portion of the globe.

In one of the experiments this evening a brush was produced at the end of a wire. This brush was a flame, a source of heat

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and light. It did not emit much perceptible heat, nor did it glow with an intense light; but is it the less a flame because it does not scorch my hand? is it the less a flame because it does not hurt my eye by its brilliancy? The problem is precisely to produce in the bulb such a flame, much smaller in size, but incomparably more powerful. Were there means at hand for producing electric impulses of a sufficiently high frequency, and for transmitting them, the bulb could be done away with, unless it were used to protect the electrode, or to economise the energy by confining the heat. But as such means are not at disposal, it becomes necessary to place the terminal in a bulb and rarefy the air in the same. This is done merely to enable the apparatus to perform the work which it is not capable of performing at ordinary air pressure. In the bulb we are able to intensify the action to any degree—so far that the brush emits a powerful light. The intensity of the light emitted depends principally on the frequency and potential of the impulses, and on the electric density on the surface of the electrode. It is of the greatest importance to employ the smallest possible button, in order to push the density very far. Under the violent impact of the molecules of the gas surrounding it, the small electrode is of course brought to an extremely high temperature, but around it is a mass of highly incandescent gas, a flame or photosphere, many hundred times the volume of the electrode. With a diamond, carborundum, or zirconia button the photosphere can be as much as one thousand times the volume of the button. Without much reflecting one would think that in pushing so far the incandescence of the electrode it would be instantly volatilised. But after a careful consideration he would find that, theoretically, it should not occur, and in this fact—which, however, is experimentally demonstrated—lies principally the future value of such a lamp.

At first, when the bombardment begins, most of the work is performed on the surface of the button, but when a highly conducting photosphere is formed the button is comparatively relieved. The higher the incandescence of the photosphere, the more it approaches in conductivity to that of the electrode, and

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the more, therefore, the solid and the gas form one conducting body. The consequence is that, the further is forced the incandescence, the more work, comparatively, is performed on the gas, and the less on the electrode. The formation of a powerful photosphere is consequently the very means for protecting the electrode. This protection, of course, is a relative one, and it should not be thought that by pushing the incandescence higher the electrode is actually less deteriorated. Still, theoretically, with extreme frequencies, this result must be reached, but probably at a temperature too high for most of the refractory bodies known. Given, then, an electrode which can withstand to a certain very high limit the effect of the bombardment and outward strain, it would be safe no matter how much it is forced beyond that limit. In an incandescent lamp quite different considerations apply. There the gas is not at all concerned: the whole of the work is performed on the filament; and the life of the lamp diminishes so rapidly with the increase of the degree of incandescence, that economical reasons compel us to work it at a low incandescence. But if an incandescent lamp is operated with currents of very high frequency, the action of the gas cannot be neglected, and the rules for the most economical working must be considerably modified.

In order to bring such a lamp with one or two electrodes to a great perfection, it is necessary to employ impulses of very high frequency. The high frequency secures, among others, two chief advantages, which have a most important bearing upon the economy of the light production. Firstly, the deterioration of the electrode is reduced by reason of the fact that we employ a great many small impacts, instead of few violent ones, which shatter quickly the structure; secondly, the formation of a large photosphere is facilitated.

In order to reduce the deterioration of the electrode to the minimum, it is desirable that the vibration be harmonic, for any suddenness hastens the process of destruction. An electrode lasts much longer when kept at incandescence by currents, or impulses, obtained from a high-frequency alternator, which rise and fall more or less harmonically, than by impulses obtained

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from a disruptive discharge coil. In the latter case there is no doubt that most of the damage is done by the fundamental sudden discharges.

One of the elements of loss in such a lamp is the bombardment of the globe. As the potential is very high, the molecules are projected with great speed; they strike the glass, and usually excite a strong phosphorescence. The effect produced is very pretty, but for economical reasons it would be perhaps preferable to prevent, or at least reduce to the minimum, the bombardment against the globe, as in such case it is, as a rule, not the object to excite phosphorescence, and as some loss of energy results from the bombardment. This loss in the bulb is principally dependent on the potential of the impulses, and on the electric density on the surface of the electrode. In employing very high frequencies the loss of energy by the bombardment is greatly reduced, for, firstly, the potential needed to perform a given amount of work is much smaller, and, secondly, by producing a highly conducting photosphere around the electrode, the same result is obtained as though the electrode were much larger, which is equivalent to a smaller electric density. But be it by the diminution of the maximum potential or of the density, the gain is effected in the same manner, namely, by avoiding violent shocks, which strain the glass much beyond its limit of elasticity. If the frequency could be brought high enough, the loss due to the imperfect elasticity of the glass would be entirely negligible. The loss due to bombardment of the globe may, however, be reduced by using two electrodes instead of one. In such case each of the electrodes may be connected to one of the terminals; or else, if it is preferable to use only one wire, one electrode may be connected to one terminal, and the other to the ground or to an insulated body of some surface, as, for instance, a shade on the lamp. In the latter case, unless some judgment is used, one of the electrodes might glow more intensely than the other.

But, on the whole, I find it preferable when using such high frequencies to employ only one electrode and one connecting wire. I am convinced that the illuminating device of the near

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future will not require for its operation more than one lead, and, at any rate, it will have no leading-in wire, since the energy required can be as well transmitted through the glass. In experimental bulbs the leading-in wire is most generally used on account of convenience, as in employing condenser coatings in the manner indicated in Fig. 22, for example, there is some difficulty in fitting the parts, but these difficulties would not exist if a great many bulbs were manufactured; otherwise the energy can be conveyed through the glass as well as through a wire, and with these high frequencies the losses are very small. Such illuminating devices will necessarily involve the use of very high potentials, and this, in the eyes of practical men, might be an objectionable feature. Yet, in reality, high potentials are not objectionable—certainly not in the least as far as the safety of the devices is concerned.

There are two ways of rendering an electric appliance safe. One is to use low potentials, the other is to determine the dimensions of the apparatus so that it is safe no matter how high a potential is used. Of the two, the latter seems to me the better way, for then the safety is absolute, unaffected by any possible combination of circumstances, which might render even a low-potential appliance dangerous to life and property. But the practical conditions require not only the judicious determination of the dimensions of the apparatus; they likewise necessitate the employment of energy of the proper kind. It is easy, for instance, to construct a transformer capable of giving, when operated from an ordinary alternate-current machine of low tension, say 50,000 volts, which might be required to light a highly exhausted phosphorescent tube, so that, in spite of the high potential, it is perfectly safe, the shock from it producing no inconvenience. Still, such a transformer would be expensive, and in itself inefficient; and, besides, what energy was obtained from it would not be economically used for the production of light. The economy demands the employment of energy in the form of extremely rapid vibrations. The problem of producing light has been likened to that of maintaining a certain high-pitch note by means of a bell. It should be said a *barely*

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audible note; and even these words would not express it, so wonderful is the sensitiveness of the eye. We may deliver powerful blows at long intervals, waste a good deal of energy, and still not get what we want; or we may keep up the note by delivering frequent gentle taps, and get nearer to the object sought by the expenditure of much less energy. In the production of light, as far as the illuminating device is concerned, there can be only one rule—that is, to use as high frequencies as can be obtained; but the means for the production and conveyance of impulses of such character impose, at present at least, great limitations. Once it is decided to use very high frequencies, the return wire becomes unnecessary, and all the appliances are simplified. By the use of obvious means the same result is obtained as though the return wire were used. It is sufficient for this purpose to bring in contact with the bulb, or merely in the vicinity of the same, an insulated body of some surface. This surface need, of course, be the smaller, the higher the frequency and potential used, and necessarily, also, the higher the economy of the lamp or other device.

This plan of working has been resorted to on several occasions this evening. So, for instance, when the incandescence of a button was produced by grasping the bulb with the hand, the body of the experimenter merely served to intensify the action. The bulb used was similar to that illustrated in Fig. 19, and the coil was excited to a small potential, not sufficient to bring the button to incandescence when the bulb was hanging from the wire; and incidentally, in order to perform the experiment in a more suitable manner, the button was taken so large that a perceptible time had to elapse before, upon grasping the bulb, it could be rendered incandescent. The contact with the bulb was of course quite unnecessary. It is easy, by using a rather large bulb with an exceedingly small electrode, to adjust the conditions so that the latter is brought to bright incandescence by the mere approach of the experimenter within a few feet of the bulb, and that the incandescence subsides upon his receding.

In another experiment, when phosphorescence was excited, a similar bulb was used. Here again, originally, the potential

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was not sufficient to excite phosphorescence until the action was intensified—in this case, however, to present a different feature, by touching the socket with a metallic object held in the hand. The electrode in the bulb was a carbon button so large that it could not be brought to incandescence and thereby spoil the effect produced by phosphorescence.

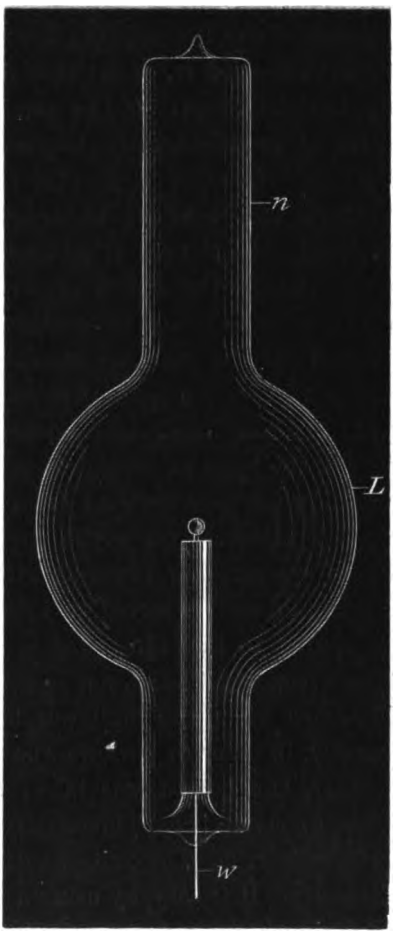

Fig. 25.—Improved Experimental Bulb.

Again, in another of the early experiments, a bulb was used as illustrated in Fig. 12. In this instance, by touching the bulb with one or two fingers, one or two shadows of the stem inside

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were projected against the glass, the touch of the finger producing the same result as the application of an external negative electrode under ordinary circumstances.

In all these experiments the action was intensified by augmenting the capacity at the end of the lead connected to the terminal. As a rule, it is not necessary to resort to such means, and would be quite unnecessary with still higher frequencies, but when it *is* desired, the bulb or tube can be easily adapted to the purpose.

In Fig. 25, for example, an experimental bulb, *L*, is shown, which is provided with a neck, *n*, on the top for the application of an external tinfoil coating, which may be connected to a body of

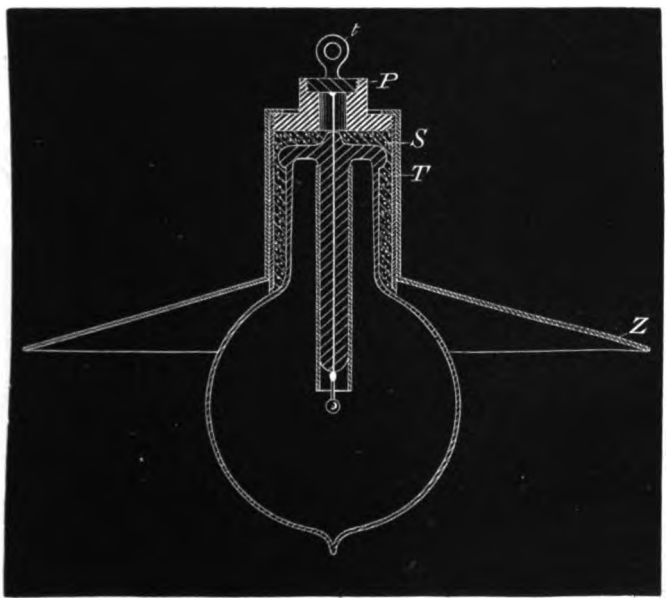

FIG. 26.—Improved Bulb with Intensifying Reductor.

larger surface. Such a lamp as illustrated in Fig. 25 may also be lighted by connecting the tinfoil coating on the neck *n* to the terminal, and the leading-in wire, *w*, to an insulated plate. If the bulb stands in a socket upright, as shown in the cut, a shade of conducting material may be slipped in the neck *n*, and the action thus magnified.

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A more perfected arrangement used in some of these bulbs is illustrated in Fig. 26. In this case the construction of the bulb is as shown and described before, when reference was made to Fig. 19. A zinc sheet, *Z*, with a tubular extension, *T*, is slipped over the metallic socket, *S*. The bulb hangs downwards from the terminal, *t*, the zinc sheet *Z* performing the double office of intensifier and reflector. The reflector is separated from the terminal *t* by an extension of the insulating plug, *P*.

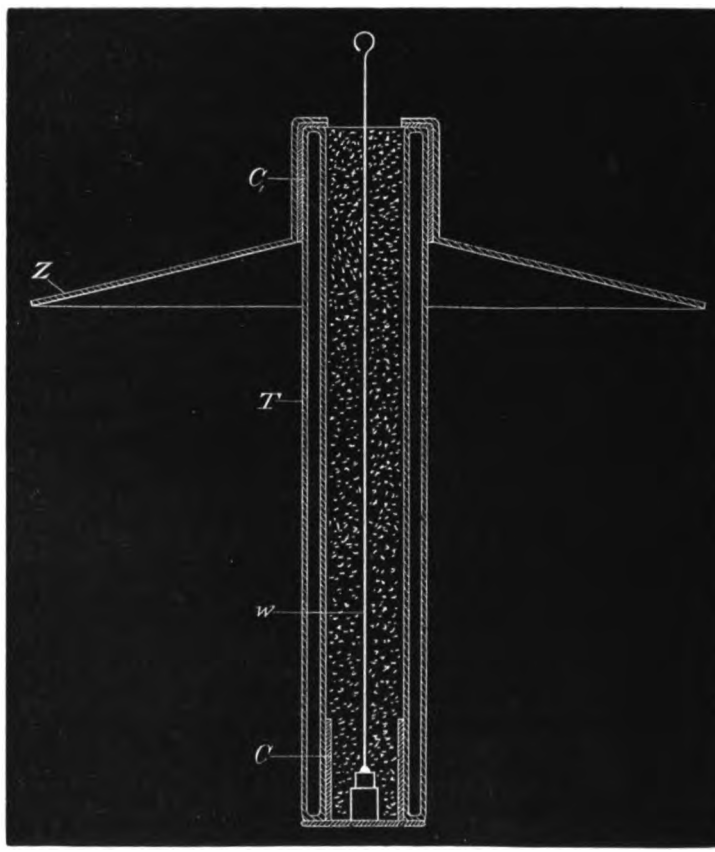

FIG. 27.—Phosphorescent Tube with Intensifying Reflector.

A similar disposition with a phosphorescent tube is illustrated in Fig. 27. The tube, *T*, is prepared from two short tubes of a different diameter, which are sealed on the ends. On the lower

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end is placed an outside conducting coating, C, which connects to the wire, w. The wire has a hook on the upper end for suspension, and passes through the centre of the inside tube, which is filled with some good and tightly packed insulator. On the outside of the upper end of the tube T is another conducting coating, C_1, upon which is slipped a metallic reflector, Z, which should be separated by a thick insulation from the end of wire w.

The economical use of such a reflector or intensifier would require that all energy supplied to an air condenser should be recoverable, or, in other words, that there should not be any losses, neither in the gaseous medium nor through its action elsewhere. This is far from being so, but, fortunately, the losses may be reduced to anything desired. A few remarks are necessary on this subject, in order to make the experiences gathered in the course of these investigations perfectly clear.

Suppose a small helix with many well-insulated turns, as in experiment Fig. 17, has one of its ends connected to one of the terminals of the induction coil, and the other to a metal plate, or, for the sake of simplicity, a sphere, insulated in space. When the coil is set to work, the potential of the sphere is alternated, and the small helix now behaves as though its free end were connected to the other terminal of the induction coil. If an iron rod be held within the small helix, it is quickly brought to a high temperature, indicating the passage of a strong current through the helix. How does the insulated sphere act in this case? It can be a condenser, storing and returning the energy supplied to it, or it can be a mere sink of energy, and the conditions of the experiment determine whether it is more one or the other. The sphere being charged to a high potential, it acts inductively upon the surrounding air, or whatever gaseous medium there might be. The molecules, or atoms, which are near the sphere are of course more attracted, and move through a greater distance than the farther ones. When the nearest molecules strike the sphere they are repelled, and collisions occur at all distances within the inductive action of the sphere. It is now clear that, if the potential be steady, but little loss of energy can be caused in this way, for the molecules which are nearest

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to the sphere, having had an additional charge imparted to them by contact, are not attracted until they have parted, if not with all, so with most of the additional charge, which can be accomplished only after a great many collisions. From the fact that with a steady potential there is but little loss in dry air, one must come to such a conclusion. When the potential of the sphere, instead of being steady, is alternating, the conditions are entirely different. In this case a rhythmical bombardment occurs, no matter whether the molecules after coming in contact with the sphere lose the imparted charge or not; what is more, if the charge is not lost, the impacts are only the more violent. Still, if the frequency of the impulses be very small, the loss caused by the impacts and collisions would not be serious unless the potential were excessive. But when extremely high frequencies and more or less high potentials are used, the loss may be very great. The total energy lost per unit of time is proportionate to the product of the number of impacts per second, or the frequency, and the energy lost in each impact. But the energy of an impact must be proportionate to the square of the electric density of the sphere, since the charge imparted to the molecule is proportionate to that density. I conclude from this that the total energy lost must be proportionate to the product of the frequency and the square of the electric density; but this law needs experimental confirmation. Assuming the preceding considerations to be true, then, by rapidly alternating the potential of a body immersed in an insulating gaseous medium, any amount of energy may be dissipated into space. Most of that energy, then, I believe, is not dissipated in the form of long ether waves, propagated to considerable distance, as is thought most generally, but is consumed—in the case of an insulated sphere, for example—in impact and collisional losses—that is, heat vibrations—on the surface and in the vicinity of the sphere. To reduce the dissipation it is necessary to work with a small electric density—the smaller, the higher the frequency.

But since, on the assumption before made, the loss is diminished with the square of the density, and since currents of very high frequencies involve considerable waste when transmitted through

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conductors, it follows that, on the whole, it is better to employ one wire than two. Therefore, if motors, lamps, or devices of any kind are perfected, capable of being advantageously operated by currents of extremely high frequency, economical reasons will make it advisable to use only one wire, especially if the distances are great.

When energy is absorbed in a condenser, the same behaves as though its capacity were increased. Absorption exists more or less always, but generally it is small and of no consequence as long as the frequencies are not very great. In using extremely high frequencies, and, necessarily, in such case also high potentials, the absorption—or, what is here meant more particularly by this term, the loss of energy due to the presence of a gaseous medium—is an important factor to be considered, as the energy absorbed in the air condenser may be any fraction of the supplied energy. This would seem to make it very difficult to tell from the measured or computed capacity of an air condenser its actual capacity or vibration period, especially if the condenser is of very small surface and is charged to a very high potential. As many important results are dependent upon the correctness of the estimation of the vibration period, this subject demands the most careful scrutiny of other investigators. To reduce the probable error as much as possible in experiments of the kind alluded to, it is advisable to use spheres or plates of large surface, so as to make the density exceedingly small. Otherwise, when it is practicable, an oil condenser should be used in preference. In oil or other liquid dielectrics there are seemingly no such losses as in gaseous media. It being impossible to exclude entirely the gas in condensers with solid dielectrics, such condensers should be immersed in oil, for economical reasons if nothing else: they can then be strained to the utmost and will remain cool. In Leyden jars the loss due to air is comparatively small, as the tinfoil coatings are large, close together, and the charged surfaces not directly exposed; but when the potentials are very high, the loss may be more or less considerable at, or near, the upper edge of the foil, where the air is principally acted upon. If the jar be immersed in boiled-out

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oil, it will be capable of performing four times the amount of work which it can for any length of time when used in the ordinary way, and the loss will be inappreciable.

It should not be thought that the loss in heat in an air condenser is necessarily associated with the formation of *visible* streams, or brushes. If a small electrode, enclosed in an unexhausted bulb, is connected to one of the terminals of the coil, streams can be seen to issue from the electrode, and the air in the bulb is heated; if, instead of a small electrode, a large sphere is enclosed in the bulb, no streams are observed, still the air is heated.

Nor should it be thought that the temperature of an air condenser would give even an approximate idea of the loss in heat incurred, as in such case heat must be given off much more quickly, since there is, in addition to the ordinary radiation, a very active carrying away of heat by independent carriers going on, and since not only the apparatus, but the air at some distance from it, is heated in consequence of the collisions which must occur.

Owing to this, in experiments with such a coil, a rise of temperature can be distinctly observed only when the body connected to the coil is very small. But with apparatus on a larger scale, even a body of considerable bulk would be heated, as, for instance, the body of a person; and I think that skilled physicians might make observations of utility in such experiments, which, if the apparatus were judiciously designed, would not present the slightest danger.

A question of some interest, principally to meteorologists, presents itself here. How does the earth behave? The earth is an air condenser, but is it a perfect or a very imperfect one—a mere sink of energy? There can be little doubt that to any small disturbance, as might be caused in an experiment, the earth behaves as an almost perfect condenser. But it might be different when its charge is set in vibration by some sudden disturbance occurring in the heavens. In such case, as before stated, probably only little of the energy of the vibrations set up would be lost into space in form of long ether radiations;

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but most of the energy, I think, would spend itself in molecular impacts and collisions, and pass off into space in the form of short heat, and possibly light, waves. As both the frequency of the vibrations of the charge and the potential are in all probability excessive, the energy converted into heat may be considerable. Since the density must be unevenly distributed, either in consequence of the irregularity of the earth's surface, or on account of the condition of the atmosphere on various places, the effect produced would accordingly vary from place to place. Considerable variations in the temperature and pressure of the atmosphere may in this manner be caused at any point of the surface of the earth. The variations may be gradual or very sudden, according to the nature of the original disturbance, and may produce rain and storms, or locally modify the weather in any way.

From the remarks before made one may see what an important factor of loss the air in the neighbourhood of a charged surface becomes when the electric density is great and the frequency of the impulses excessive. But the action as explained implies that the air is insulating—that is, that it is composed of independent carriers immersed in an insulating medium. This is the case only when the air is at something like ordinary or greater, or at extremely small, pressure. When the air is slightly rarefied and conducting, then true conduction losses occur also. In such case, of course, considerable energy may be dissipated into space even with a steady potential, or with impulses of low frequency, if the density is very great.

When the gas is at very low pressure, an electrode is heated more because higher speeds can be reached. If the gas around the electrode is strongly compressed, the displacements, and consequently the speeds, are very small, and the heating is insignificant. But if in such case the frequency could be sufficiently increased, the electrode would be brought to a high temperature as well as if the gas were at very low pressure; in fact, exhausting the bulb is only necessary because we cannot produce (and possibly not convey) currents of the required frequency.

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Returning to the subject of electrode lamps, it is obviously of advantage in such a lamp to confine as much as possible the heat to the electrode by preventing the circulation of the gas in the bulb. If a very small bulb be taken, it would confine the heat better than a large one, but it might not be of sufficient capacity to be operated from the coil, or, if so, the glass might get too hot. A simple way to improve in this direction is to employ a globe of the required size, but to place a small bulb, the diameter of which is properly estimated, over the refractory button contained in the globe. This arrangement is illustrated in Fig. 28.

The globe, *L*, has in this case a large neck, *n*, allowing the small bulb, *b*, to slip through. Otherwise the construction is the same as shown in Fig. 18, for example. The small bulb is conveniently supported upon the stem, *s*, carrying the refractory button, *m*. It is separated from the aluminum tube, *a*, by several layers of mica, *M*, in order to prevent the cracking of the neck by the rapid heating of the aluminum tube upon a sudden turning on of the current. The inside bulb should be as small as possible when it is desired to obtain light only by incandescence of the electrode. If it is desired to produce phosphorescence, the bulb should be larger, else it would be apt to get too hot, and the phosphorescence would cease. In this arrangement usually only the small bulb shows phosphorescence, as there is practically no bombardment against the outer globe. In some of these bulbs constructed as illustrated in Fig. 28 the small tube was coated with phosphorescent paint, and beautiful effects were obtained. Instead of taking the inside bulb large, in order to avoid undue heating, it answers the purpose to make the electrode *m* larger. In this case the bombardment is weakened by reason of the smaller electric density.

Many bulbs were constructed on the plan illustrated in Fig. 29. Here a small bulb, *b*, containing the refractory button, *m*, upon being exhausted to a very high degree, was sealed in a large globe, *L*, which was then moderately exhausted and sealed off. The principal advantage of this construction was that it allowed to reach extremely high vacua, and, at the same time-

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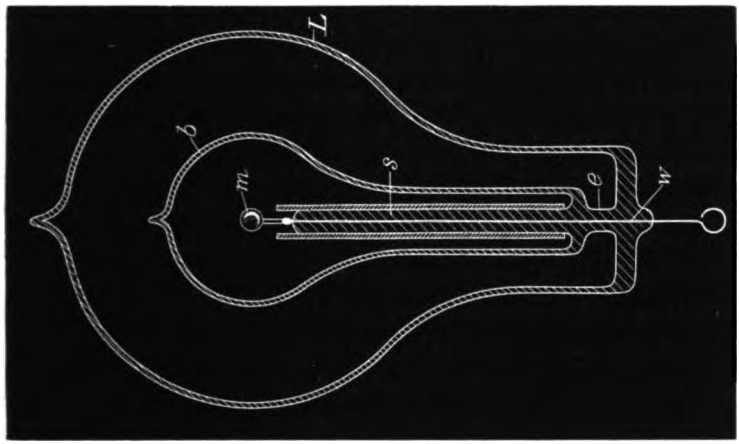

Fig. 29.—Lamp with Independent Auxiliary Bulb.

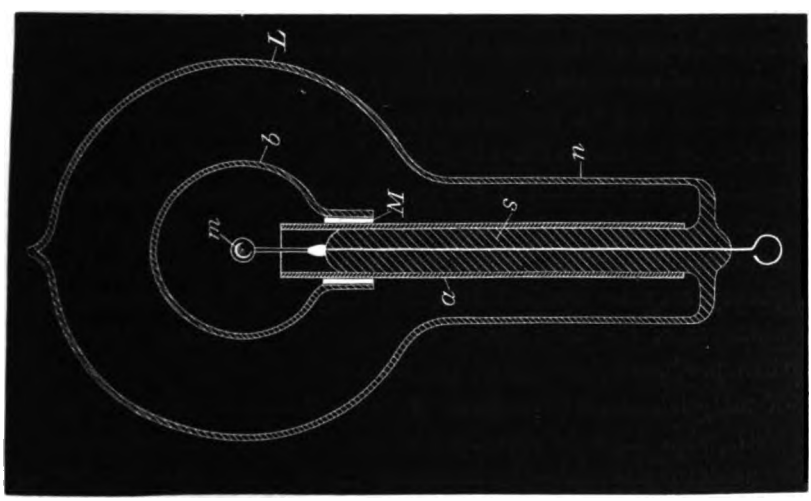

Fig. 28.—Lamp with Auxiliary Bulb for Confining the Action to the Centre.

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use a large bulb. It was found, in the course of experiences with bulbs such as illustrated in Fig. 29, that it was well to make the stem, *s*, near the seal at *e* very thick, and the leading-in wire, *w*, thin, as it occurred sometimes that the stem at *e* was heated and the bulb was cracked. Often the outer globe *L* was exhausted only just enough to allow the discharge to pass through, and the space between the bulbs appeared crimson, producing a curious effect. In some cases, when the exhaustion in globe *L* was very low, and the air good conducting, it was found necessary, in order to bring the button *m* to high incandescence, to place, preferably on the upper part of the neck of the globe, a tinfoil coating which was connected to an insulated body to the ground, or to the other terminal of the coil, as the highly conducting air weakened the effect somewhat, probably by being acted upon inductively from the wire *w* where it entered the bulb at *e*. Another difficulty—which, however, is always present when the refractory button is mounted in a very small bulb—existed in the construction illustrated in Fig. 29, namely, the vacuum in the bulb *b* would be impaired in a comparatively short time.

The chief idea in the two last described constructions was to confine the heat to the central portion of the globe by preventing the exchange of air. An advantage is secured, but owing to the heating of the inside bulb, and slow evaporation of the glass, the vacuum is hard to maintain, even if the construction illustrated in Fig. 28 be chosen, in which both bulbs communicate.

But by far the better way—the ideal way—would be to reach sufficiently high frequencies. The higher the frequency, the slower would be the exchange of the air, and I think that a frequency may be reached at which there would be no exchange whatever of the air molecules around the terminal. We would then produce a flame in which there would be no carrying away of material, and a queer flame it would be, for it would be rigid! With such high frequencies the inertia of the particles would come into play. As the brush, or flame, would gain rigidity in virtue of the inertia of the particles, the exchange of the latter would be prevented. This would necessarily occur, for, the number of the impulses being augmented, the potential energy

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of each would diminish, so that finally only atomic vibrations could be set up, and the motion of translation through measurable space would cease. Thus an ordinary gas burner connected to a source of rapidly alternating potential might have its efficiency augmented to a certain limit, and this for two reasons—because of the additional vibration imparted, and because of a slowing down of the process of carrying off. But the renewal being rendered difficult, and renewal being necessary to maintain the *burner*, a continued increase of the frequency of the impulses, assuming they could be transmitted to and impressed upon the flame, would result in the "extinction" of the latter, meaning by this term only the cessation of the chemical process.

I think, however, that in the case of an electrode immersed in a fluid insulating medium, and surrounded by independent carriers of electric charges, which can be acted upon inductively, a sufficiently high frequency of the impulses would probably result in a gravitation of the gas all around towards the electrode. For this it would be only necessary to assume that the independent bodies are irregularly shaped: they would then turn towards the electrode their side of the greatest electric density, and this would be a position in which the fluid resistance to approach would be smaller than that offered to the receding.

The general opinion, I do not doubt, is that it is out of question to reach any such frequencies as might—assuming some of the views before expressed to be true—produce any of the results which I have pointed out as mere possibilities. This may be so, but in the course of these investigations, from the observation of many phenomena I have gained the conviction that these frequencies would be much lower than one is apt to estimate at first. In a flame we set up light vibrations by causing molecules, or atoms, to collide. But what is the ratio of the frequency of the collisions and that of the vibrations set up? Certainly it must be incomparably smaller than that of the knocks of the bell and the sound vibrations, or that of the discharges and the oscillations of the condenser. We may cause the molecules of the gas to collide by the use of alternate electric impulses of high frequency, and so we may imitate the process in a flame; and from experi-

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ments with frequencies which we are now able to obtain, I think that the result is producible with impulses which are transmissible through a conductor.

In connection with thoughts of a similar nature, it appeared to me of great interest to demonstrate the rigidity of a vibrating gaseous column. Although with such low frequencies as, say 10,000 per second, which I was able to obtain without difficulty from a specially constructed alternator, the task looked discouraging at first, I made a series of experiments. The trials with air at ordinary pressure led to no result, but with air moderately rarefied I obtained what I think to be an unmistakable experimental evidence of the property sought for. As a result of this kind might lead able investigators to conclusions of importance, I will describe one of the experiments performed.

It is well known that when a tube is slightly exhausted the discharge may be passed through it in form of a thin luminous thread. When produced with currents of low freqnency, obtained from a coil operated as usual, this thread is inert. If a magnet be approached to it, the part near the same is attracted or repelled, according to the direction of the lines of force of the magnet. It occurred to me that if such a thread would be produced with currents of very high frequency, it should be more or less rigid, and as it was visible it could be easily studied. Accordingly I prepared a tube about 1 inch in diameter and 1 metre long, with outside coating at each end. The tube was exhausted to a point at which by a little working the thread discharge could be obtained. It must be remarked here that the general aspect of the tube, and the degree of exhaustion, are quite different than when ordinary low-frequency currents are used. As it was found preferable to work with one terminal, the tube prepared was suspended from the end of a wire connected to the terminal, the tinfoil coating being connected to the wire, and to the lower coating sometimes a small insulated plate was attached. When the thread was formed it extended through the upper part of the tube and lost itself in the lower end. If it possessed rigidity it resembled, not exactly an elastic cord stretched tight between two supports, but a cord suspended from a height

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with a small weight attached on the end. When the finger or a magnet was approached to the upper end of the luminous thread, it could be brought locally out of position by electrostatic or magnetic action; and when the disturbing object was very quickly removed, an analogous result was produced, as though a suspended cord would be displaced and quickly released near the point of suspension. In doing this the luminous thread was set in vibration, and two very sharply marked nodes, and a third indistinct one, were formed. The vibration, once set up, continued for fully eight minutes, dying gradually out. The speed of the vibration often varied perceptibly, and it could be observed that the electrostatic attraction of the glass affected the vibrating thread; but it was clear that the electrostatic action was not the cause of the vibration, for the thread was most generally stationary, and could always be set in vibration by passing the finger quickly near the upper part of the tube. With a magnet the thread could be split in two and both parts vibrated. By approaching the hand to the lower coating of the tube, or insulated plate if attached, the vibration was quickened; also, as far as I could see, by raising the potential or frequency. Thus, either increasing the frequency or passing a stronger discharge of the same frequency corresponded to a tightening of the cord. I did not obtain any experimental evidence with condenser discharges. A luminous band excited in a bulb by repeated discharges of a Leyden jar must possess rigidity, and if deformed and suddenly released should vibrate. But probably the amount of vibrating matter is so small that in spite of the extreme speed the inertia cannot prominently assert itself. Besides, the observation in such a case is rendered extremely difficult on account of the fundamental vibration.

The demonstration of the fact—which still needs better experimental confirmation—that a vibrating gaseous column possesses rigidity, might greatly modify the views of thinkers. When with low frequencies and insignificant potentials indications of that property may be noted, how must a gaseous medium behave under the influence of enormous electrostatic stresses which may be active in the interstellar space, and which may

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alternate with inconceivable rapidity? The existence of such an electrostatic, rhythmically throbbing force—of a vibrating electrostatic field—would show a possible way how solids might have formed from the ultra-gaseous uterus, and how transverse and all kinds of vibrations may be transmitted through a gaseous medium filling all space. Then, ether might be a true fluid, devoid of rigidity, and at rest, it being merely necessary as a connecting link to enable interaction. What determines the rigidity of a body? It must be the speed and the amount of moving matter. In a gas the speed may be considerable, but the density is exceedingly small; in a liquid the speed would be likely to be small, though the density may be considerable; and in both cases the inertia resistance offered to displacement is practically *nil*. But place a gaseous (or liquid) column in an intense, rapidly alternating electrostatic field, set the particles vibrating with enormous speeds, then the inertia resistance asserts itself. A body might move with more or less freedom through the vibrating mass, but as a whole it would be rigid.

There is a subject which I must mention in connection with these experiments: it is that of high vacua. This is a subject the study of which is not only interesting, but useful, for it may lead to results of great practical importance. In commercial apparatus, such as incandescent lamps, operated from ordinary systems of distribution, a much higher vacuum than presently obtained would not secure a very great advantage. In such a case the work is performed on the filament and the gas is little concerned; the improvement, therefore, would be but trifling. But when we begin to use very high frequencies and potentials, the action of the gas becomes all-important, and the degree of exhaustion materially modifies the results. As long as ordinary coils, even very large ones, were used, the study of the subject was limited, because just at a point when it became most interesting it had to be interrupted on account of the "non-striking" vacuum being reached. But presently we are able to obtain from a small disruptive discharge coil potentials much higher than even the largest coil was capable of giving, and, what is more, we can make the potential alternate with great rapidity. Both of these results

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enable us now to pass a luminous discharge through most any vacua obtainable, and the field of our investigations is greatly extended. Think we as we may, of all the possible directions to develop a practical illuminant, the line of high vacua seems to be the most promising at present. But to reach extreme vacua the appliances must be much more improved, and ultimate perfection will not be attained until we shall have discarded the mechanical and perfected an *electrical* vacuum pump. Molecules and atoms can be thrown out of a bulb under the action of an enormous potential: *this* will be the principle of the vacuum pump of the future. For the present we must secure the best results we can with mechanical appliances. In this respect it might not be out of the way to say a few words about the method of, and apparatus for, producing excessively high degrees of exhaustion of which I have availed myself in the course of these investigations. It is very probable that other experimenters have used similar arrangements; but as it is possible that there may be an item of interest in their description, a few remarks which will render this investigation more complete might be permitted.

The apparatus is illustrated in a drawing shown in Fig. 30. S represents a Sprengel pump, which has been specially constructed to better suit the work required. The stop-cock which is usually employed has been omitted, and instead of it a hollow stopper, s, has been fitted in the neck of the reservoir, R. This stopper has a small hole, h, through which the mercury descends; the size of the outlet, o, being properly determined with respect to the section of the fall tube, t, which is sealed to the reservoir instead of being connected to it in the usual manner. This arrangement overcomes the imperfections and troubles which often arise from the use of the stop-cock on the reservoir and the connection of the latter with the fall tube.

The pump is connected through a U-shaped tube, t, to a very large reservoir, R_1. Especial care was taken in fitting the grinding surfaces of the stoppers, p and p_1, and both of these and the mercury caps above them were made exceptionally long. After the U-shaped tube was fitted and put in place, it was heated, so

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as to soften and take off the strain resulting from imperfect fitting. The U-shaped tube was provided with a stop-cock, C, and two ground connections, g and g_1—one for a small bulb, b, usually containing caustic potash, and the other for the receiver, r, to be exhausted.

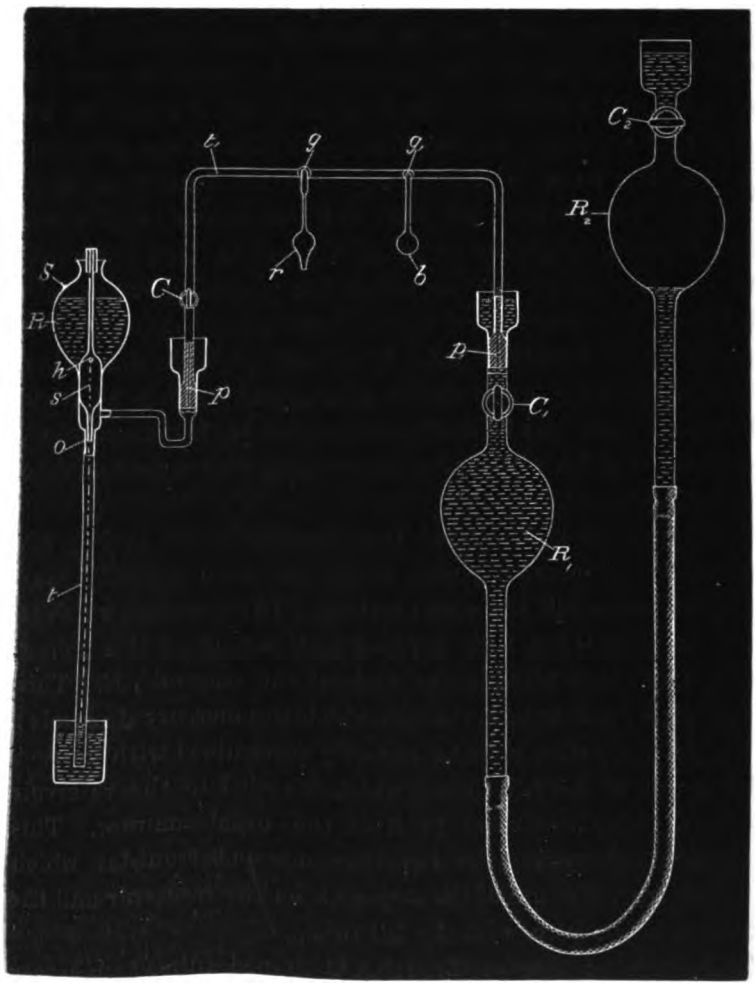

FIG. 80.—Apparatus Used for Obtaining High Degrees of Exhaustion.

The reservoir R_1 was connected by means of a rubber tube to a slightly larger reservoir, R_2, each of the two reservoirs being

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provided with a stop-cock, C_1 and C_2, respectively. The reservoir R_2 could be raised and lowered by a wheel and rack, and the range of its motion was so determined that when it was filled with mercury and the stop-cock C_2 closed, so as to form a Torricellian vacuum in it when raised, it could be lifted so high that the mercury in reservoir R_1 would stand a little above stop-cock C_1; and when this stop-cock was closed and the reservoir R_2 descended, so as to form a Torricellian vacuum in reservoir R_1, it could be lowered so far as to completely empty the latter, the mercury filling the reservoir R_2 up to a little above stop-cock C_2.

The capacity of the pump and of the connections was taken as small as possible relatively to the volume of reservoir R_1, since, of course, the degree of exhaustion depended upon the ratio of these quantities.

With this apparatus I combined the usual means indicated by former experimenters for the production of very high vacua. In most of the experiments it was convenient to use caustic potash. I may venture to say, in regard to its use, that much time is saved and a more perfect action of the pump ensured by fusing and boiling the potash as soon as, or even before, the pump settles down. If this course is not followed, the sticks, as ordinarily employed, may give moisture off at a certain very slow rate, and the pump may work for many hours without reaching a very high vacuum. The potash was heated either by a spirit lamp, or by passing a discharge through it, or by passing a current through a wire contained in it. The advantage in the latter case was that the heating could be more rapidly repeated.

Generally the process of exhaustion was the following:—At the start, the stop-cocks C and C_1 being open, and all other connections closed, the reservoir R_2 was raised so far that the mercury filled the reservoir R_1 and a part of the narrow connecting U-shaped tube. When the pump was set to work, the mercury would, of course, quickly rise in the tube, and reservoir R_2 was lowered, the experimenter keeping the mercury at about the same level. The reservoir R_2 was balanced by a long spring which facilitated the operation, and the friction of the parts was

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generally sufficient to keep it almost in any position. When the Sprengel pump had done its work, the reservoir R_2 was further lowered and the mercury descended in R_1 and filled R_2, whereupon stop-cock C_2 was closed. The air adhering to the walls of R_1 and that absorbed by the mercury was carried off, and to free the mercury of all air the reservoir R_2 was for a long time worked up and down. During this process some air, which would gather below stop-cock C_2, was expelled from R_2 by lowering it far enough and opening the stop-cock, closing the latter again before raising the reservoir. When all the air had been expelled from the mercury, and no air would gather in R_2 when it was lowered, the caustic potash was resorted to. The reservoir R_2 was now again raised until the mercury in R_1 stood above stop-cock C_1. The caustic potash was fused and boiled, and the moisture partly carried off by the pump and partly re-absorbed; and this process of heating and cooling was repeated many times, and each time, upon the moisture being absorbed or carried off, the reservoir R_2 was for a long time raised and lowered. In this manner all the moisture was carried off from the mercury, and both the reservoirs were in proper condition to be used. The reservoir R_2 was then again raised to the top, and the pump was kept working for a long time. When the highest vacuum obtainable with the pump had been reached, the potash bulb was usually wrapped with cotton which was sprinkled with ether so as to keep the potash at a very low temperature, then the reservoir R_2 was lowered, and upon reservoir R_1 being emptied the receiver r was quickly sealed up.

When a new bulb was put on, the mercury was always raised above stop-cock C_1, which was closed, so as to always keep the mercury and both the reservoirs in fine condition, and the mercury was never withdrawn from R_1 except when the pump had reached the highest degree of exhaustion. It is necessary to observe this rule if it is desired to use the apparatus to advantage.

By means of this arrangement I was able to proceed very quickly, and when the apparatus was in perfect order it was possible to reach the phosphorescent stage in a small bulb in less than 15 minutes, which is certainly very quick work for a small

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laboratory arrangement requiring all in all about 100 pounds of mercury. With ordinary small bulbs the ratio of the capacity of the pump, receiver, and connections, and that of reservoir R was about 1-20, and the degrees of exhaustion reached were necessarily very high, though I am unable to make a precise and reliable statement how far the exhaustion was carried.

What impresses the investigator most in the course of these experiences is the behaviour of gases when subjected to great rapidly alternating electrostatic stresses. But he must remain in doubt as to whether the effects observed are due wholly to the molecules, or atoms, of the gas which chemical analysis discloses to us, or whether there enters into play another medium of a gaseous nature, comprising atoms, or molecules, immersed in a fluid pervading the space. Such a medium surely must exist, and I am convinced that, for instance, even if air were absent, the surface and neighbourhood of a body in space would be heated by rapidly alternating the potential of the body; but no such heating of the surface or neighbourhood could occur if all free atoms were removed and only a homogeneous, incompressible, and elastic fluid—such as ether is supposed to be—would remain, for then there would be no impacts, no collisions. In such a case, as far as the body itself is concerned, only frictional losses in the inside could occur.

It is a striking fact that the discharge through a gas is established with ever-increasing freedom as the frequency of the impulses is augmented. It behaves in this respect quite contrarily to a metallic conductor. In the latter the impedance enters prominently into play as the frequency is increased, but the gas acts much like a series of condensers would: the facility with which the discharge passes through seems to depend on the rate of change of potential. If it act so, then in a vacuum tube even of great length, and no matter how strong the current, self-induction could not assert itself to any appreciable degree. We have, then, as far as we can now see, in the gas a conductor which is capable of transmitting electric impulses of any frequency which we may be able to produce. Could the frequency be brought high enough, then a queer system of electric distribution, which

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would be likely to interest gas companies, might be realised: metal pipes filled with gas—the metal being the insulator, the gas the conductor—supplying phosphorescent bulbs, or perhaps devices as yet uninvented. It is certainly possible to take a hollow tore of copper, rarefy the gas in the same, and by passing impulses of sufficiently high frequency through a circuit around it, bring the gas inside to a high degree of incandescence; but as to the nature of the forces there would be considerable uncertainty, for it would be doubtful whether with such impulses the copper tore would act as a static screen. Such paradoxes and apparent impossibilities we encounter at every step in this line of work, and therein lies, to a great extent, the charm of the study.

I have here a short and wide tube which is exhausted to a high degree and covered with a substantial coating of bronze, the coating allowing barely the light to shine through. A metallic clasp, with a hook for suspending the tube, is fastened around the middle portion of the latter, the clasp being in contact with the bronze coating. I now want to light the gas inside by suspending the tube on a wire connected to the coil. Anyone who would try the experiment for the first time, not having any previous experience, would probably take care to be quite alone when making the trial, for fear that he might become the joke of his assistants. Still, the bulb lights in spite of the metal coating, and the light can be distinctly perceived through the latter. A long tube covered with aluminum bronze lights when held in one hand—the other touching the terminal of the coil—quite powerfully. It might be objected that the coatings are not sufficiently conducting; still, even if they were highly resistant, they ought to screen the gas. They certainly screen it perfectly in a condition of rest, but not by far perfectly when the charge is surging in the coating. But the loss of energy which occurs within the tube, notwithstanding the screen, is occasioned principally by the presence of the gas. Were we to take a large hollow metallic sphere and fill it with a perfect incompressible fluid dielectric, there would be no loss inside of the sphere, and consequently the inside might be considered as perfectly screened, though the potential be very rapidly alternating. Even were the sphere

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filled with oil, the loss would be incomparably smaller than when the fluid is replaced by a gas, for in the latter case the force produces displacements: that means impact and collisions in the inside.

No matter what the pressure of the gas may be, it becomes an important factor in the heating of a conductor when the electric density is great and the frequency very high. That in the heating of conductors by lightning discharges air is an element of great importance, is almost as certain as an experimental fact. I may illustrate the action of the air by the following experiment:—I take a short tube which is exhausted to a moderate degree and has a platinum wire running through the middle from one end to the other. I pass a steady or low-frequency current through the wire, and it is heated uniformly in all parts. The heating here is due to conduction, or frictional losses, and the gas around the wire has—as far as we can see—no function to perform. But now let me pass sudden discharges, or a high-frequency current, through the wire. Again the wire is heated, this time principally on the ends and least in the middle portion; and if the frequency of the impulses, or the rate of change, is high enough, the wire might as well be cut in the middle as not, for practically all the heating is due to the rarefied gas. Here the gas might only act as a conductor of no impedance diverting the current from the wire as the impedance of the latter is enormously increased, and merely heating the ends of the wire by reason of their resistance to the passage of the discharge. But it is not at all necessary that the gas in the tube should be conducting; it might be at an extremely low pressure, still the ends of the wire would be heated—as, however, is ascertained by experience—only the two ends would in such case not be electrically connected through the gaseous medium. Now what with these frequencies and potentials occurs in an exhausted tube occurs in the lightning discharges at ordinary pressure. We only need remember one of the facts arrived at in the course of these investigations, namely, that to impulses of very high frequency the gas at ordinary pressure behaves much in the same manner as though it were at moderately low pressure. I think that in

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lightning discharges frequently wires or conducting objects are volatilised merely because air is present, and that, were the conductor immersed in an insulating liquid, it would be safe, for then the energy would have to spend itself somewhere else. From the behaviour of gases to sudden impulses of high potential I am led to conclude that there can be no surer way of diverting a lightning discharge than by affording it a passage through a volume of gas, if such a thing can be done in a practical manner.

There are two more features upon which I think it necessary to dwell in connection with these experiments—the "radiant "state" and the "non-striking vacuum."

Anyone who has studied Crookes's work must have received the impression that the "radiant state" is a property of the gas inseparably connected with an extremely high degree of exhaustion. But it should be remembered that the phenomena observed in an exhausted vessel are limited to the character and capacity of the apparatus which is made use of. I think that in a bulb a molecule, or atom, does not precisely move in a straight line because it meets no obstacle, but because the velocity imparted to it is sufficient to propel it in a sensibly straight line. The mean free path is one thing, but the velocity—the energy associated with the moving body—is another, and under ordinary circumstances I believe that it is a mere question of potential, or speed. A disruptive discharge coil, when the potential is pushed very far, excites phosphorescence and projects shadows, at comparatively low degrees of exhaustion. In a lightning discharge, matter moves in straight lines at ordinary pressure when the mean free path is exceedingly small, and frequently images of wires or other metallic objects have been produced by the particles thrown off in straight lines.

I have prepared a bulb to illustrate by an experiment the correctness of these assertions. In a globe, L (Fig. 31), I have mounted upon a lamp filament, f, a piece of lime, l. The lamp filament is connected with a wire which leads into the bulb, and the general construction of the latter is as indicated in Fig. 19, before described. The bulb being suspended from a wire connected to the terminal of the coil, and the latter being set to

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work, the lime piece l and the projecting parts of the filament f are bombarded. The degree of exhaustion is just such that with the potential the coil is capable of giving phosphorescence of the glass is produced, but disappears as soon as the vacuum is impaired. The lime containing moisture, and moisture being given off as soon as heating occurs, the phosphorescence lasts only for a few moments. When the lime has been sufficiently

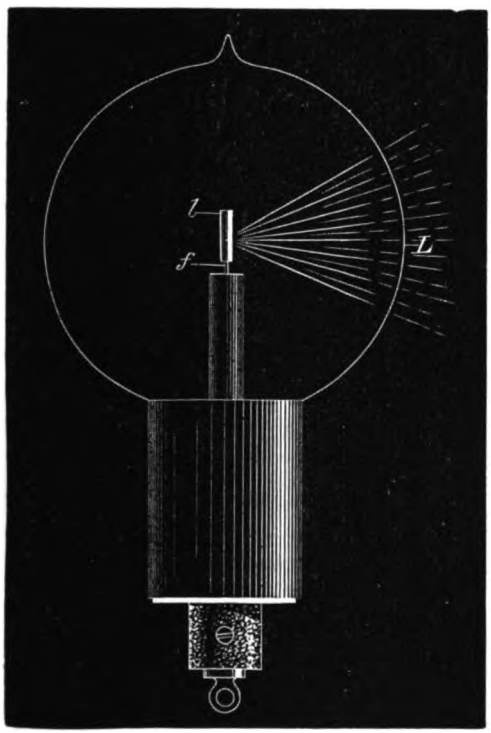

FIG. 31.—Bulb showing Radiant Lime Stream at Low Exhaustion.

heated, enough moisture has been given off to impair materially the vacuum of the bulb. As the bombardment goes on, one point of the lime piece is more heated than other points, and the result is that finally practically all the discharge passes through that point which is intensely heated, and a white stream of lime particles (Fig. 31) then breaks forth from that point. This stream is composed of "radiant" matter, yet the degree of

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exhaustion is low. But the particles move in straight lines because the velocity imparted to them is great, and this is due to three causes—to the great electric density, the high temperature of the small point, and the fact that the particles of the lime are easily torn and thrown off—far more easily than those of carbon. With frequencies such as we are able to obtain, the particles are bodily thrown off and projected to a considerable distance; but with sufficiently high frequencies no such thing would occur: in such case only a stress would spread or a vibration would be propagated through the bulb. It would be out of question to reach any such frequency on the assumption that the atoms move with the speed of light; but I believe that such a thing is impossible: for this an enormous potential would be required. With potentials which we are able to obtain, even with a disruptive discharge coil, the speed must be quite insignificant.

As to the "non-striking vacuum," the point to be noted is that it can occur only with low-frequency impulses, and it is necessitated by the impossibility of carrying off enough energy with such impulses in high vacuum, since the few atoms which are around the terminal, upon coming in contact with the same are repelled and kept at a distance for a comparatively long period of time, and not enough work can be performed as to render the effect perceptible to the eye. If the difference of potential between the terminals is raised, the dielectric breaks down. But with very high frequency impulses there is no necessity for such breaking down, since any amount of work can be performed by continually agitating the atoms in the exhausted vessel, provided the frequency is high enough. It is easy to reach—even with frequencies obtained from an alternator as here used—a stage at which the discharge does not pass between two electrodes in a narrow tube, each of these being connected to one of the terminals of the coil, but it is difficult to reach a point at which a luminous discharge would not occur around each electrode.

A thought which naturally presents itself in connection with high-frequency currents is, to make use of their powerful electrodynamic inductive action to produce light effects in a sealed glass globe. The leading-in wire is one of the defects of the present

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incandescent lamp, and if no other improvement were made, that imperfection at least should be done away with. Following this thought, I have carried on experiments in various directions, of which some were indicated in my former paper. I may here mention one or two more lines of experiment which have been followed up.

Many bulbs were constructed as shown in Fig. 32 and Fig. 33.

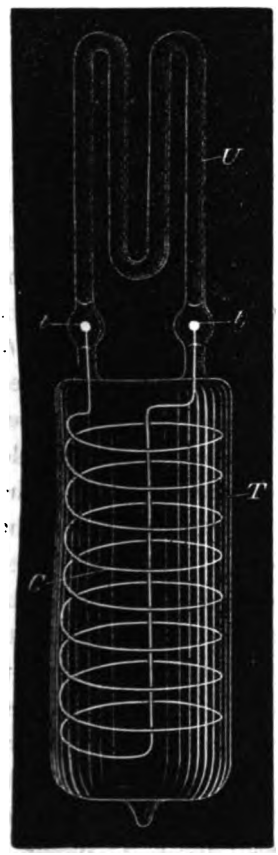

FIG. 32.—Electro-dynamic Induction Tube.

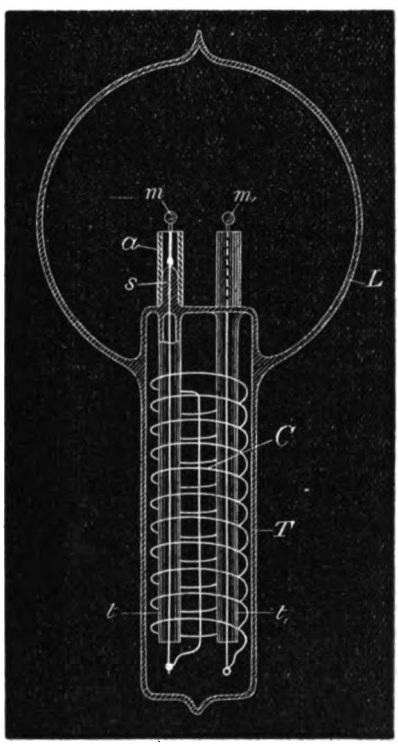

FIG. 33.—Electro-dynamic Induction Lamp.

In Fig. 32 a wide tube, *T*, was sealed to a smaller W-shaped tube, *U*, of phosphorescent glass. In the tube *T* was placed a coil, *C*, of aluminum wire, the ends of which were provided with

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small spheres, t and t_1, of aluminum, and reached into the U tube. The tube T was slipped into a socket containing a primary coil through which usually the discharges of Leyden jars were directed, and the rarefied gas in the small U tube was excited to strong luminosity by the high-tension currents induced in the coil C. When Leyden jar discharges were used to induce currents in the coil C, it was found necessary to pack the tube T tightly with insulating powder, as a discharge would occur frequently between the turns of the coil, especially when the primary was thick and the air gap, through which the jars discharged, large, and no little trouble was experienced in this way.

In Fig. 33 is illustrated another form of the bulb constructed. In this case a tube, T, is sealed to a globe, L. The tube contains a coil, C, the ends of which pass through two small glass tubes, t and t_1, which are sealed to the tube T. Two refractory buttons, m and m_1, are mounted on lamp filaments which are fastened to the ends of the wires passing through the glass tubes t and t_1. Generally in bulbs made on this plan the globe L communicated with the tube T. For this purpose the ends of the small tubes t and t_1 were just a trifle heated in the burner, merely to hold the wires, but not to interfere with the communication. The tube T, with the small tubes, wires through the same, and the refractory buttons m and m_1, was first prepared, and then sealed to globe L, whereupon the coil C was slipped in and the connections made to its ends. The tube was then packed with insulating powder, jamming the latter as tight as possible up to very nearly the end, then it was closed and only a small hole left through which the remainder of the powder was introduced, and finally the end of the tube was closed. Usually in bulbs constructed as shown in Fig. 33 an aluminum tube, a, was fastened to the upper end, s, of each of the tubes t and t_1, in order to protect that end against the heat. The buttons m and m_1 could be brought to any degree of incandescence by passing the discharges of Leyden jars around the coil C. In such bulbs with two buttons a very curious effect is produced by the formation of the shadows of each of the two buttons.

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Another line of experiment, which has been assiduously followed, was to induce by electro-dynamic induction a current or luminous discharge in an exhausted tube or bulb. This matter has received such an able treatment at the hands of Professor J. J. Thomson, that I could add but little to what he has made known, even had I made it the special subject of this lecture. Still, since experiences in this line have gradually led me to the present views and results, a few words must be devoted here to this subject.

It has occurred, no doubt, to many, that as a vacuum tube is made longer the electro-motive force per unit length of the tube, necessary to pass a luminous discharge through the latter, gets continually smaller; therefore, if the exhausted tube be made long enough, even with low frequencies a luminous discharge could be induced in such a tube closed upon itself. Such a tube might be placed around a hall or on a ceiling, and at once a simple appliance capable of giving considerable light would be obtained. But this would be an appliance hard to manufacture and extremely unmanageable. It would not do to make the tube up of small lengths, because there would be with ordinary frequencies considerable loss in the coatings, and besides, if coatings were used, it would be better to supply the current directly to the tube by connecting the coatings to a transformer. But even if all objections of such nature were removed, still, with low frequencies the light conversion itself would be inefficient, as I have before stated. In using extremely high frequencies the length of the secondary—in other words, the size of the vessel—can be reduced as far as desired, and the efficiency of the light conversion is increased, provided that means are invented for efficiently obtaining such high frequencies. Thus one is led, from theoretical and practical considerations, to the use of high frequencies, and this means high electro-motive forces and small currents in the primary. When he works with condenser charges —and these are the only means up to the present known for reaching these extreme frequencies—he gets to electro-motive forces of several thousands of volts per turn of the primary. He cannot multiply the electro-dynamic inductive effect by taking

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more turns in the primary, for he arrives at the conclusion that the best way is to work with one single turn—though he must sometimes depart from this rule—and he must get along with whatever inductive effect he can obtain with one turn. But before he has long experimented with the extreme frequencies required to set up in a small bulb an electro-motive force of several thousands of volts, he realises the great importance of electrostatic effects, and these effects grow relatively to the electro-dynamic in significance as the frequency is increased.

Now, if anything is desirable in this case, it is to increase the frequency, and this would make it still worse for the electro-dynamic effects. On the other hand, it is easy to exalt the electrostatic action as far as one likes by taking more turns on the secondary, or combining self-induction and capacity to raise the potential. It should also be remembered that, in reducing the current to the smallest value and increasing the potential, the electric impulses of high frequency can be more easily transmitted through a conductor.

These and similar thoughts determined me to devote more attention to the electrostatic phenomena, and to endeavour to produce potentials as high as possible, and alternating as fast as they could be made to alternate. I then found that I could excite vacuum tubes at considerable distance from a conductor connected to a properly constructed coil, and that I could, by converting the oscillatory current of a condenser to a higher potential, establish electrostatic alternating fields which acted through the whole extent of a room, lighting up a tube no matter where it was held in space. I thought to recognise that I had made a step in advance, and I have persevered in this line; but I wish to say that I share with all lovers of science and progress the one and only desire—to reach a result of utility to men in any direction to which thought or experiment may lead me. I think that this departure is the right one, for I cannot see, from the observation of the phenomena which manifest themselves as the frequency is increased, what there would remain to act between two circuits conveying, for instance, impulses of several hundred millions per second, except electrostatic forces.

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Even with such trifling frequencies the energy would be practically all potential, and my conviction has grown strong that, to whatever kind of motion light may be due, it is produced by tremendous electrostatic stresses vibrating with extreme rapidity.

Of all these phenomena observed with currents, or electric impulses, of high frequency, the most fascinating for an audience are certainly those which are noted in an electrostatic field acting through considerable distance, and the best an unskilled lecturer can do is to begin and finish with the exhibition of these singular effects. I take a tube in the hand and move it about, and it is lighted wherever I may hold it; throughout space the invisible forces act. But I may take another tube and it might not light, the vacuum being very high. I excite it by means of a disruptive discharge coil, and now it will light in the electrostatic field. I may put it away for a few weeks or months, still it retains the faculty of being excited. What change have I produced in the tube in the act of exciting it? If a motion imparted to the atoms, it is difficult to perceive how it can persist so long without being arrested by frictional losses; and if a strain exerted in the dielectric, such as a simple electrification would produce, it is easy to see how it may persist indefinitely, but very difficult to understand why such a condition should aid the excitation when we have to deal with potentials which are rapidly alternating.

Since I have exhibited these phenomena for the first time, I have obtained some other interesting effects. For instance, I have produced the incandescence of a button, filament, or wire enclosed in a tube. To get to this result it was necessary to economise the energy which is obtained from the field and direct most of it on the small body to be rendered incandescent. At the beginning the task appeared difficult, but the experiences gathered permitted to reach the result easily. In Fig. 34 and Fig. 35 two such tubes are illustrated which are prepared for the occasion. In Fig. 34 a short tube, T_1, sealed to another long tube, T, is provided with a stem, s, with a platinum wire sealed in the latter. A very thin lamp filament, l, is fastened to this wire, and connection to the outside is made through a thin copper wire, w. The tube is provided with outside and inside coatings, C and C_1 respectively,

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and is filled as far as the coatings reach with conducting, and the space above with insulating powder. These coatings are merely

FIG. 34.—Tube with Filament rendered Incandescent in an Electrostatic Field.

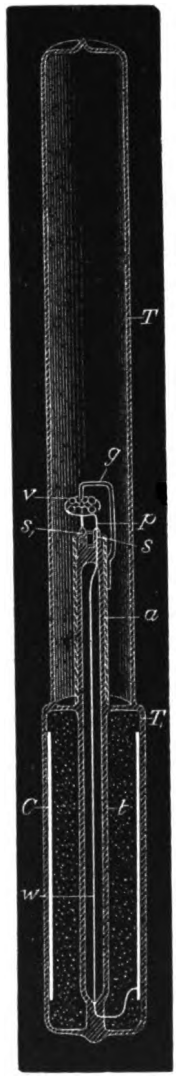

FIG. 35.—Crookes Experiment in Electrostatic Field.

used to enable to perform two experiments with the tube—namely, to produce the effect desired either by a direct connection of the body of the experimenter or of another body to the wire *w*, or by acting inductively through the glass. The stem *s* is provided with

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an aluminum tube, a, for purposes before explained, and only a small part of the filament reaches out of this tube. By holding the tube T_1 anywhere in the electrostatic field the filament is rendered incandescent.

A more interesting piece of apparatus is illustrated in Fig. 35. The construction is the same as before, only instead of the lamp filament a small platinum wire, p, sealed in a stem, s, and bent above it in a circle, is connected to the copper wire w, which is joined to an inside coating, C. A small stem, s_1, is provided with a needle, on the point of which is arranged to rotate very freely a very light fan of mica, v. To prevent the fan from falling out, a thin stem of glass, g, is bent properly and fastened to the aluminum tube. When the glass tube is held anywhere in the electrostatic field the platinum wire becomes incandescent, and the mica vanes are rotated very fast.

Intense phosphorescence may be excited in a bulb by merely connecting it to a plate within the field, and the plate need not be any larger than an ordinary lamp shade. The phosphorescence excited with these currents is incomparably more powerful than with ordinary apparatus. A small phosphorescent bulb, when attached to a wire connected to a coil, emits sufficient light to allow reading ordinary print at a distance of 5 to 6 paces. It was of interest to see how some of the phosphorescent bulbs of Professor Crookes would behave with these currents, and he has had the kindness to lend me a few for the occasion. The effects produced are magnificent, especially by the sulphide of calcium and sulphide of zinc. From the disruptive discharge coil they glow intensely merely by holding them in the hand and connecting the body to the terminal of the coil.

To whatever results investigations of this kind may lead, their chief interest lies for the present in the possibilities they offer for the production of an efficient illuminating device. In no branch of electric industry is an advance more desired than in the manufacture of light. Every thinker, when considering the barbarous methods employed, the deplorable losses incurred in our best systems of light production, must have asked himself, What is likely to be the light of the future? Is it to be an

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incandescent solid, as in the present lamp, or an incandescent gas, or a phosphorescent body, or something like a burner, but incomparably more efficient?

There is little chance to perfect a gas burner; not perhaps, because human ingenuity has been bent upon that problem for centuries, without a radical departure having been made—though this argument is not devoid of force—but because in a burner the higher vibrations can never be reached except by passing through all the low ones. For how is a flame produced unless by a fall of lifted weights? Such process cannot be maintained without renewal, and renewal is repeated passing from low to high vibrations. One way only seems to be open to improve a burner, and that is by trying to reach higher degrees of incandescence. Higher incandescence is equivalent to a quicker vibration: that means more light from the same material; and that, again, means more economy. In this direction some improvements have been made, but the progress is hampered by many limitations. Discarding, then, the burner, there remains the three ways first mentioned, which are essentially electrical.

Suppose the light of the immediate future to be a solid rendered incandescent by electricity. Would it not seem that it is better to employ a small button than a frail filament? From many considerations it certainly must be concluded that a button is capable of a higher economy, assuming, of course, the difficulties connected with the operation of such a lamp to be effectively overcome. But to light such a lamp we require a high potential; and to get this economically we must use high frequencies.

Such considerations apply even more to the production of light by the incandescence of a gas, or by phosphorescence. In all cases we require high frequencies and high potentials. These thoughts occurred to me a long time ago.

Incidentally we gain, by the use of very high frequencies, many advantages, such as a higher economy in the light production, the possibility of working with one lead, the possibility of doing away with the leading-in wire, &c.

The question is, How far can we go with frequencies? Ordinary conductors rapidly lose the facility of transmitting electric

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impulses when the frequency is greatly increased. Assume the means for the production of impulses of very great frequency brought to the utmost perfection, everyone will naturally ask how to transmit them when the necessity arises. In transmitting such impulses through conductors we must remember that we have to deal with *pressure* and *flow*, in the ordinary interpretation of these terms. Let the pressure increase to an enormous value, and let the flow correspondingly diminish, then such impulses—variations merely of pressure, as it were—can no doubt be transmitted through a wire even if their frequency be many hundreds of millions per second. It would, of course, be out of question to transmit such impulses through a wire immersed in a gaseous medium, even if the wire were provided with a thick and excellent insulation, for most of the energy would be lost in molecular bombardment and consequent heating. The end of the wire connected to the source would be heated, and the remote end would receive but a trifling part of the energy supplied. The prime necessity, then, if such electric impulses are to be used, is to find means to reduce as much as possible the dissipation.

The first thought is, employ the thinnest possible wire surrounded by the thickest practicable insulation. The next thought is, to employ electrostatic screens. The insulation of the wire may be covered with a thin conducting coating, and the latter connected to the ground. But this would not do, as then all the energy would pass through the conducting coating to the ground, and nothing would get to the end of the wire. If a ground connection is made, it can only be made through a conductor offering an enormous impedance, or through a condenser of extremely small capacity. This, however, does not do away with other difficulties.

If the wave length of the impulses is much smaller than the length of the wire, then corresponding short waves will be set up in the conducting coating, and it will be more or less the same as though the coating were directly connected to earth. It is therefore necessary to cut up the coating in sections much shorter than the wave length. Such an arrangement does not still afford a perfect screen, but it is ten thousand times better

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than none. I think it preferable to cut up the conducting coating in small sections, even if the current-waves be much longer than the coating.

If a wire were provided with a perfect electrostatic screen, it would be the same as though all objects were removed from it at infinite distance. The capacity would then be reduced to the capacity of the wire itself, which would be very small. It would then be possible to send over the wire current-vibrations of very high frequencies at enormous distances without affecting greatly the character of the vibrations. A perfect screen is of course out of question, but I believe that with a screen such as I have just described telephony could be rendered practicable across the Atlantic. According to my ideas, the gutta-percha-covered wire should be provided with a thin conducting coating sub-divided in sections. On the top of this should be again placed a layer of gutta-percha and other insulation, and on the top of the whole the armour. But such cables will not be constructed, for ere long intelligence—transmitted without wires—will throb through the earth like a pulse through a living organism. The wonder is that, with the present state of knowledge and the experiences gained, no attempt is being made to disturb the electrostatic or magnetic condition of the earth, and transmit, if nothing else, intelligence.

It has been my chief aim in presenting these results to point out phenomena or features of novelty, and to advance ideas which I am hopeful will serve as starting-points of new departures. It has been my chief desire this evening to entertain you with some novel experiments. Your applause, so frequently and generously accorded, has told me that I have succeeded.

In conclusion, let me thank you most heartily for your kindness and attention, and assure you that the honour I have had in addressing such a distinguished audience, the pleasure I have had in presenting these results to a gathering of so many able men—and among them also some of those in whose work for many years past I have found enlightenment and constant pleasure—I shall never forget.

The PRESIDENT: It is my most pleasing duty to propose a very

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hearty vote of thanks to our lecturer, who has entertained us, it is true, for two hours; but we would willingly wait for another hour's similar entertainment. I venture to suggest that those of us who have time at our disposal should regard this as but Act I., and persuade the lecturer to continue—after an interval for refreshment, of course—to give us Act II. No words of mine, obviously, are necessary to make you appreciate the magnificent results you have seen. To have shared with Professor Galileo Ferraris the invention of the principle of the rotary magnetic field, and its application to practical alternate-current motors on a large scale, would have been quite sufficient to have made our distinguished lecturer to-night honoured among us; but in these magnificent experiments we have seen on electrostatic electric lamps he has really surpassed himself, and I therefore propose a hearty vote of thanks for the delightful lecture we have had this evening.

As we are guests here to-night, we must not separate without offering our thanks to the managers of the Royal Institution for having so kindly given us the use of this theatre.

One other vote of thanks we must not forget—to Mr. Siemens, for having placed the resources of his firm at the disposal of Mr. Tesla for preparing these experiments.

The several resolutions were carried by acclamation.

The meeting then adjourned.

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The Two Hundred and Thirtieth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 11th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on January 28th, 1892, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Donations to the Library were announced as having been received since the last meeting from Mr. John Aylmer, Professor A. B. W. Kennedy, and Mr. Stuart A. Russell, Members; and Mr. J. Louis Balbi, Associate; to whom the thanks of the meeting were duly accorded.

The PRESIDENT: Mr. Siemens, I am sorry to say, like our Secretary, is suffering from illness, and unable to come to the meeting this evening; but Mr. G. P. Simpson will officiate in his place, and I now ask him to read Mr. Siemens's paper.

The following paper was then read :—

SOME EXPERIMENTAL INVESTIGATIONS OF ALTERNATE CURRENTS.

By ALEXANDER SIEMENS, Vice-President.

Most of the papers read before this Institution on the subject of alternate electric currents, and on apparatus employed for utilising them, have dealt principally with the theoretical side of the question, and undoubtedly have been extremely useful to constructors of alternate-current apparatus. Nevertheless, certain gaps in our knowledge obtruded themselves in practice, and the investigations and experiments which are described in the present communication were undertaken, not so much to verify any particular theory, as to increase our knowledge of useful facts.

For convenience of reference, these experiments are not described in chronological order, but they have been grouped

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together according to the subjects which they are intended to elucidate. Foremost among these is the loss of energy through the heating of iron by induction where alternate electric currents are employed—a subject which engaged the attention of Prof. Ewing and of Dr. J. Hopkinson six years ago, whose researches pointed out the direction in which further experiments should be made. It is well known that this loss is caused by the so-called Foucault currents and by hysteresis. The effect of the former is counteracted by laminating the core of a magnet either by building it up with wires of small diameter or with thin discs, according to the direction of the lines of induction.

A short consideration will show that the heat produced by Foucault currents can be calculated beforehand for a core of known construction and dimensions.

Take the case of an iron wire, 1 cm. long, subjected to an induction parallel to its axis of B lines per 1 square cm. of its section.

Let r be the radius of the wire, and ρ the radius of a ring, of the width $d\rho$, concentric with the wire. In this ring currents will be produced in the same manner as in the secondary circuit of a transformer.

The maximum induction passing through this ring is $N = \rho^2 \pi . B$. Hence the electro-motive force generated is $E = 4 n N \times 10^{-8} = 4 n \rho^2 \pi B 10^{-8}$ volts.

If c is the conductivity of the iron employed, the resistance of the ring is $R = \frac{1}{c} \times \frac{2\pi\rho}{d\rho}$; consequently the waste of energy in this ring, expressed in watts, is

$$d W = \frac{E^2}{R} = 8 c \pi n^2 B^2 \rho^3 d\rho . 10^{-16}.$$

The total energy wasted in 1 cm. length of the wire is therefore

$$W = 2 c . \pi . n^2 . B^2 . r^4 . 10^{-16} \text{ watts.}$$

From this formula a table has been calculated to indicate the waste of energy by Foucault currents in 1 cwt. of iron of the conductivity $c = 0{\cdot}102 \times 10^6$, when an alternate current of the frequency $n = 100 \sim$ per second was employed.

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Induction. B	Diameter of Wire. ½ mm.	1 mm.	2 mm.	3 mm.
1,000	0·8 watts	3·3 watts	13·3 watts	29·9 watts.
2,000	3·3 „	13·3 „	53·3 „	119·9 „
3,000	7·4 „	29·9 „	119·9 „	269·8 „
4,000	13·4 „	53·3 „	213·3 „	479·8 „
5,000	20·8 „	83·3 „	333·2 „	749·7 „
6,000	29·9 „	119·9 „	479·8 „	1,079·4 „

In order to determine the total loss of energy caused by the heating of the iron, experiments were made with a specially constructed "cable transformer."

This mode of constructing transformers has been foreshadowed by Dr. Werner von Siemens, who proposed in his patent No. 42 of 1886 to surround the primary and secondary circuits of a transformer with iron wire, but his experiments gave no satisfactory results.

The subject was recently taken up again, and the present form of these transformers was suggested by Mr. Dieselhorst and Dr. Baur.

The principal advantage which is claimed for this form of transformer is that it can be made by machinery, in the same way as a submarine cable is made, the employment of manual labour in putting it together being entirely avoided.

A special machine has been designed and erected for completing such transformers in one operation, after the iron core has been prepared on an ordinary rope stranding machine.

Besides offering these facilities for manufacture, the peculiar shape of the cable transformer lends itself to a variety of useful applications.

For instance, at the Crystal Palace Exhibition one of these transformers connects an alternate-current machine to a high-voltage transformer some distance away, raising the potential of the current at the same time from 80 volts to 2,500 volts.

The transformer used for the experiments has a core of 900 soft iron wires, each 1 mm. in diameter by 6 metres long, twisted up in the form of a rope, and surrounded by two windings of copper wire, properly insulated, the one for high voltage and the other for low voltage.

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From the fact that the diameter of the transformer is very small compared to its length, it follows that the magnetic field, and consequently the induction in the iron core, is uniformly distributed, with the exception of a slight drop at either end.

The mean induction produced by passing an alternate current through the low-voltage circuit was measured by the difference of potential obtained in the high-voltage circuit while no current was passing through the latter.

The temperature of the iron core was ascertained by measuring the increase in the electrical resistance of one of its wires, which had been insulated from the rest by a cotton covering.

By actual comparison it was found that one of the wires near the periphery gave, within the limits of errors of observation, the same results as the central wire, so that the exact position of this "test wire" has no influence on the result; the heating of the cotton insulation can be neglected, as its mass is so very small compared with that of the iron.

Separate tests were made to determine the specific heat of the iron, which was found to be = 0·112, and the temperature coefficient for the electrical resistance of the test wire, which was equal to 0·0054.

The current was kept on for a short time only, as it was desired in the first instance to determine the rate at which energy is converted into heat in such a transformer, and the rise in temperature after keeping the current on for s seconds can be calculated from the formula,

$$t_s = \frac{R_s - R_0}{\cdot 0054 \,.\, R_0}$$

R_0 = electrical resistance before starting the current; R_s = electrical resistance after keeping the current on for s seconds.

During the time that the current passes, the test-wire will lose a certain amount of heat by radiation and convection, although it is surrounded by all the other iron wires, which are heated in a similar manner. This loss can, however, easily be allowed for by plotting a "cooling curve" after the passage of the current has been stopped, with times as abscissæ and temperatures as ordinates.

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In this way the rise of temperature of the iron core per second was observed with an alternate current of 100 $\frown\!\!\!\!\smile$ per second, varying in strength so as to produce a different induction for each observation.

Considering that the loss of energy caused by the heating of the iron is proportionate to its mass, to its specific heat, and to its rise of temperature per second, it was possible to plot a curve in which the maximum number of C.G.S. lines per square cm. are the abscissæ, and the losses of energy are represented as ordinates calculated for the mass of 1 cwt. of soft iron wires 1 mm. in diameter.

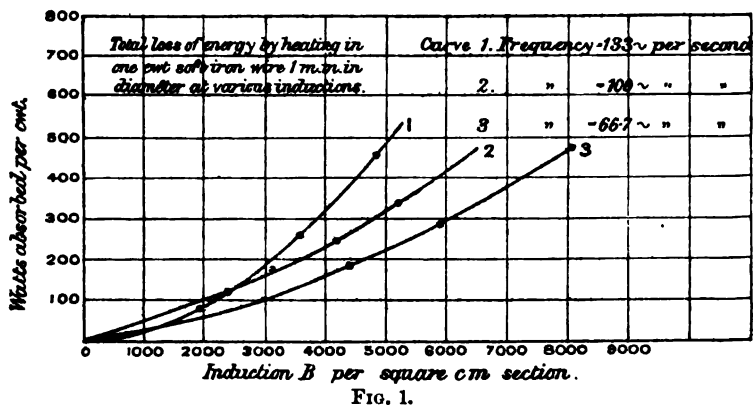

FIG. 1.

It should be added that the abscissæ of Curve 2, Diagram I., which embodies the results obtained, have been calculated from the measured volts, instead of from the calculated mean volts, which in sinoidal waves are = 0·9 measured volts.

This was done as Curve 2 was to be used for designing transformers where the measured volts only are used for calculation.

As this curve gives the losses caused by hysteresis and by Foucault currents, while the formula given above enables us to calculate the losses caused by the Foucault currents alone, we can determine the loss by hysteresis alone by a simple subtraction.

All these results, it must be remembered, refer to 1 cwt. of soft iron, subdivided into wires of 1 mm. diameter, on which

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an alternate current is acting of a frequency of 100 ∩∪ per second, the type of apparatus used being a cable transformer.

The following table shows the results :—

Induction. B =	Curve 2.	Losses of Energy. Foucault Currents.	Hysteresis.
1,000	43·2 watts	3·3 watts	39·9 watts.
2,000	96·2 „	13·3 „	82·9 „
3,000	158·0 „	29·9 „	128·1 „
4,000	231·2 „	53·3 „	177·9 „
5,000	309·5 „	83·3 „	226·2 „
6,000	390·1 „	119·9 „	270·2 „

The losses caused by hysteresis are independent of the dimensions of the iron; this table enables us, therefore, together with the formula for the losses caused by Foucault currents, to predetermine the rate of the loss of energy in iron wire of any weight or dimensions when acted upon by an alternate current of a frequency equal to 100 ∩∪ per second.

Similar experiments were made with alternate currents of a frequency of 66·6 ∩∪ per second, and of a frequency of 133·3 ∩∪ per second, and the curves embodying the results are also shown on Diagram I.

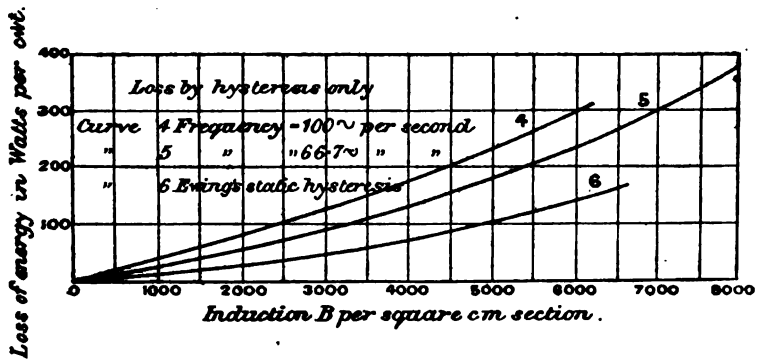

Fig. 2.

It should be observed that the currents of 66·6 and of 100 ∩∪ were obtained from the same alternate-current machine (type W_1), which has a high self-induction in the armature, and of which it has been ascertained experimentally that the waves of its current are sinoidal.

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The currents of 133 $\frown\!\!\!\!\smile$ per second were produced by another type of machine (W_{II}) with a low self-induction and a very narrow field.

On Diagram II. the losses by hysteresis alone have been shown, derived from the curves of Diagram I. by deducting the losses caused by Foucault currents according to calculation. By way of comparison, Professor Ewing's curve of losses caused by static hysteresis has been added. In this table the results obtained with alternate currents of 133·3 $\frown\!\!\!\!\smile$ per second have been been left out of consideration, on account of their being made with a different machine, as just explained.

The next point to be investigated is the change which an alteration of the frequency of the current will involve in the case of a transformer designed for a given voltage in its secondary circuit. If it is borne in mind that this voltage, apart from the constant factors, depends on the product n N (n denoting the frequency, and N the maximum number of lines of induction), it follows at once that in order to produce the same E.M.F. with a lower frequency the number of lines of induction has to be increased in proportion, and *vice versa*. In other words, if the same E.M.F. is to be produced in the secondary circuit of a given transformer, the product n N must be constant. This result can at once be applied to the formula, which gives the losses of energy caused by Foucault currents—

$$W = 2\,c\,\pi\,n^2\,B_{\max}^2\,r^4\,10^{-16};$$

and as $N = r^2 . \pi . B$, the formula can be expressed—

$$W = \frac{2\,c}{\pi}(n\,N)^2\,10^{-16};$$

or, in other words, these losses are constant for the same transformer, whatever the frequency may be, as long as the E.M.F. of the secondary circuit remains the same.

A comparison of the curves for a frequency of 66·7 and of 100 $\frown\!\!\!\!\smile$ per second on Diagram I. shows that the losses of energy for the same induction are practically proportional to the frequencies. The same curves show that this loss increases more rapidly than the number of lines of induction.

From this consideration it follows that a transformer which

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has been designed to produce a certain E.M.F. in its secondary circuit with a given frequency cannot produce the same E.M.F. with a lower frequency without over-heating, while it remains cooler when it is worked with a higher frequency. In other words, transformers built for low frequency require much material.

An experiment was made to ascertain the final temperatures of a 50-H.P. transformer worked with different frequencies, and after 10 hours' working the maximum temperature was found to be—For a frequency of 100 ∿ per second, 53° C. in the core, and 46° C. on the outside; for a frequency of 66 ∿ per second, 69° C. in the core, and 57° C. on the outside—a result which appears to bear out the above consideration.

What frequency is most advantageous can, however, not be settled by taking into account nothing but the heating of the transformers: it is quite evident that the construction of suitable generating apparatus plays as important a part; and the problem resolves itself into the commercial one, Which combination of apparatus can be constructed most cheaply?

The question, What induction should be settled upon in designing a transformer? is rather a complex one, and it can only be solved by taking into consideration—

1st. The amount of material, iron and copper.

2nd. The drop of potential difference in the secondary circuit from no load to full load.

3rd. The efficiency of the apparatus.

4th. The heating of the apparatus.

In order to facilitate the investigations a series of transformers is compared which all have the same length of iron core, and the same number of turns of copper wire of the same diameter.

It is assumed that one of these transformers is designed for an induction B = 5,000, and that its output is such that the weights of the iron and of the copper used in its construction are equal to each other and to 1 cwt.; this may be called the normal transformer.

If, now, another transformer of the same series, but with a lower induction, is to produce the same voltage, the section of the

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iron has to be increased proportionately: for instance, in the case of B = 2,500 it has to be doubled.

The weight of the iron employed varies, in fact, inversely as the induction, and their relation can be represented by a rectangular hyperbola (Curve 7).

As we have assumed that the thickness of the copper wire and the number of its turns are to be the same in all these transformers, the weight of the copper will vary directly with the length of wire wound on the iron core, and this can be determined if the relationship between the length of periphery and the section of the iron core is known.

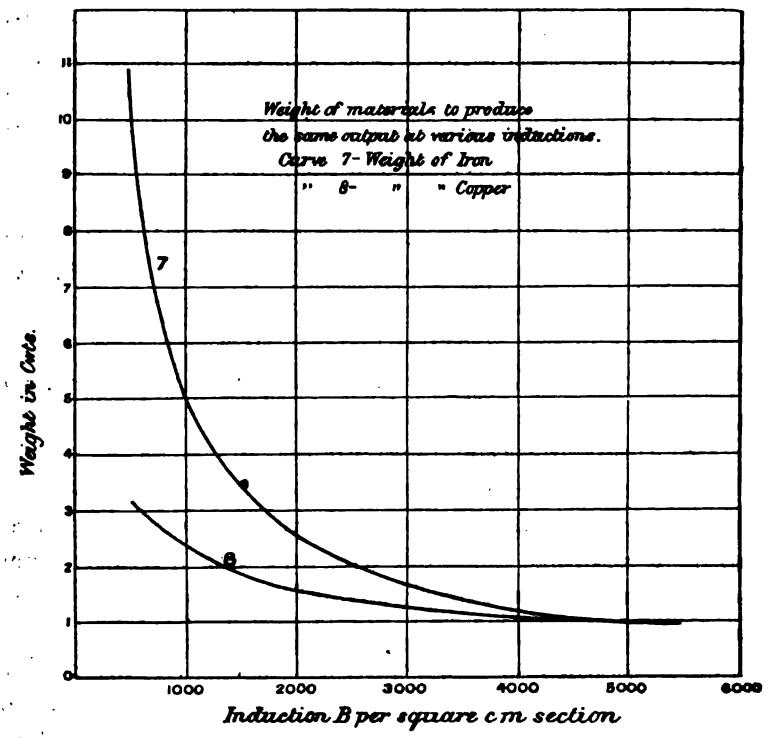

FIG. 8.

In the case of the sections of the cores being circles or squares or rectangles, with the same ratio between their length and width, the length of periphery varies as the square root of

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the section. As transformers are generally designed with cores of this kind, it may be accepted that the periphery of the iron core, and, consequently, the weight of copper to be employed, varies as the square root of the section of the iron core.

The weight of copper in the series of transformers under contemplation can therefore be represented by a curve (8) the ordinates of which are the square root of the ordinates of the curve giving the weight of iron for the corresponding inductions. From these two curves it appears that the weight of the iron increases very rapidly with decreasing induction, while the weight of copper increases at a slower rate.

As materials are important items in the cost of a transformer, the consideration of the first point shows the desirability of employing a strong induction. A similar conclusion will be drawn from the investigation of the relation between the induction and the drop of difference of potential in the secondary circuit of the series of transformers. As they are all supposed to give the same current, the drop from no load to full load will be in proportion to the resistance of the circuit. This resistance is in proportion to the weight of copper employed, as all the transformers are assumed to be wound with wire of the same diameter. The curve (8) giving the weight of copper at different inductions will therefore also indicate, in comparison with the normal transformer, the variation in the drop of potential between no load and full load under the same circumstances.

In order to determine the variation of the efficiency of the transformers under investigation, it is necessary to treat separately the losses in the iron and in the copper. The former can be found by the aid of the curves on Diagram I., in combination with the curve giving the weight of iron necessary at the various inductions.

An example will best illustrate how this loss is ascertained in a case where the total mass of the iron is worked at the same induction, produced by a current of a frequency equal to 100 $\frown\!\!\!\smile$ per second, the iron mass being composed of 1 mm. wires. The loss in the normal transformer working with an induction $B = 5{,}000$, and with a weight of iron = 1 cwt., is 309·6 watts,

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according to Diagram I. For another transformer working with an induction B = 2,500, the weight of iron, as shown by the curve (7), has to be increased to 2 cwt.; at the same time the loss per cwt. is reduced to 126·8 watts, or the total loss is reduced to 253·6 watts.

In a similar manner the losses for the different inductions have been calculated, and the results are embodied in a curve (9) which indicates that the loss of energy in the cores of transformers increases with the strength of induction, at first rapidly, but after the induction has reached a value of B = 1,000 the increase is very slow. As far as the loss of energy in the iron goes, it is, therefore, of not much consequence what induction between 1,000 and 5,000 lines per square cm. is used. The influence of the strength of induction on the loss of energy in the copper can be shown in the simplest manner by comparing this loss in the normal transformers designed for 5,000 lines with the loss in the other transformers of the series.

There are evidently three possible cases for the normal transformer: the loss in the copper may be equal to the loss in the iron, or it may be greater, or it may be less. For each of these cases a curve can be plotted showing the sum of the losses in the iron and in the copper at the corresponding induction.

If we take, for instance, the case that the losses in copper and in iron are equal, the total loss in the transformer having an induction B = 5,000, is 2 × 309·6 watts.

For the transformer having an induction B = 2,500 the loss in the iron is given in our Curve 9, and the loss in the copper is in proportion to its weight, which varies in accordance with Curve 8, or, in this particular case, the loss is = $\sqrt{2}$ × 309·6 watts. In this way Curve 11 has been plotted for the case of the losses being equal, Curve 10 represents the case where the loss in the copper is equal to two-thirds of the loss in the iron, and Curve 12 where the loss in the copper is 50 per cent. greater than the loss in the iron.

Taking these curves to represent generally the variation of the total loss at the various inductions, it appears that, when keeping the loss in copper small in comparison with the loss in

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iron, it is quite possible to obtain about the same efficiency with any induction between 2,000 and 5,000 lines per square cm., but if a lower induction is used the efficiency, even on full load, decreases very rapidly.

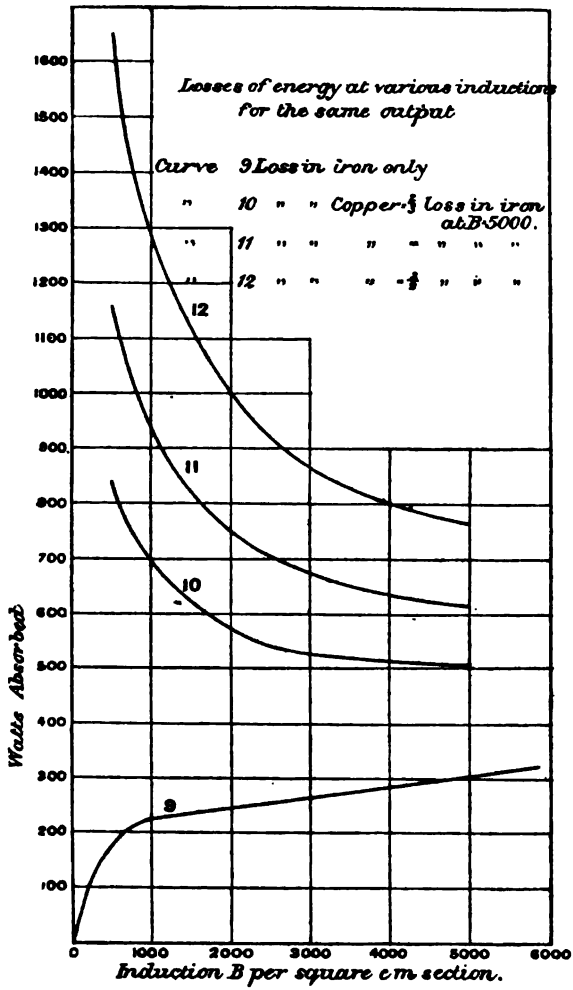

FIG. 4.

From the consideration of efficiency it is, therefore, desirable to work the transformer with a high induction, and it is only the question of the heating of the apparatus which imposes restric-

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tions on the use of high inductions, to avoid the destruction of the insulating material. In other words, the problem was to find the maximum internal temperature of a body of known dimensions, when the rate at which its mass is heated has been determined.

In order to obtain some data towards the solution of this problem, a solenoid was constructed by winding 18 lbs. of insulated iron wire, 2·4 mm. in diameter, on a brass tube 300 mm. long, 8 mm. internal diameter, and provided with flanges at the ends each 100 mm. diameter.

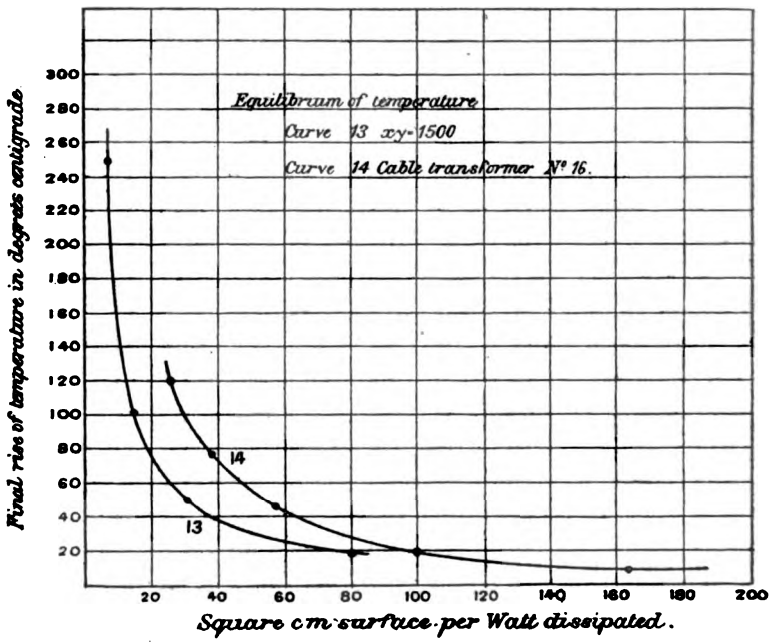

FIG. 5.

The internal temperature was measured by means of a thermometer just fitting the brass tube, and due care was taken to keep the temperature of the surrounding air constant during the experiments.

A constant direct current was sent through the coil, and a reading of the thermometer was taken after it had ceased rising. The rate at which energy was supplied to the coil was determined by the product of the square of the current into the

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resistance of the coil, measured when the equilibrium of temperature had been attained.

By varying the current the internal temperature could be varied, and the results were plotted in Curve 13, the abscissæ of which represent the cooling surface in square centimetres per watt dissipated, and the ordinates indicate the excess of the internal temperature of the coil over the temperature of the surrounding air in degrees centigrade, after the equilibrium between the supply and the dissipation of energy had been established in each case.

This curve is a rectangular hyperbola of the equation, $x\,y = 1{,}500$, and it shows that if the temperature of such a coil is to be kept at 100° C. the cooling surface should be 15 square cm. for each watt converted into heat in the coil.

It is self-evident that this curve (13) relates only to the coil by the aid of which it was constructed, and that it can only serve as a guide under strictly similar circumstances. By way of comparison a set of experiments were made on the same lines with a cable transformer, hung up in air, 6 metres long and 9·5 cm. in diameter, the internal temperature of which was determined by means of a test wire, as described above.

This curve (14) shows that for an internal temperature of 100° C. a cooling surface of about 28 square cm. has to be provided per watt of energy converted into heat in the transformer.

Before it is possible to draw general conclusions as to the connection between the internal temperature of transformers and their cooling surface, it is obviously necessary to make a good many more experiments in the direction indicated by the two curves just described.

Last year Dr. Fleming read a paper before this Institution, "On some Effects of Alternating-Current Flow in Circuits "having Capacity and Self-Induction" (*Journal of the Institution of Electrical Engineers*, vol. xx., No. 94, p. 374); it is, therefore, not necessary to repeat here the results of some experiments which were brought to the notice of the Institution at that time.

A very important factor in the construction of alternate-

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current apparatus is the striking distance of these currents in various materials, as this determines, in the case of high-voltage currents, the thickness of the insulating material with which the conductors have to be surrounded, the ohmic resistance being no guide.

The first set of these experiments referred to the striking distance in air. In all the experiments made the frequency of the alternate current was 100 ∿ per second, except where another frequency is specially mentioned, and the voltage was produced by means of an alternate-current machine and a transformer, and it was measured by one of Sir William Thomson's static voltmeters.

The electrodes were re-polished each time a spark had passed between them; one of them was fixed horizontally during the experiments, while the other could be advanced towards it from above by means of a micrometer screw which allowed of reading to a hundredth of a millemetre.

The experiments were made in the following manner:—After connecting the electrodes of the spark micrometer to the terminals of the transformer, a certain voltage was put on and accurately measured; the distance between the electrodes was then slowly diminished until sparking occurred. Each experiment was repeated at least three times. Various electrodes were experimented with, the first pair being two plane parallel surfaces. In this case the fixed horizontal electrode consisted of a polished brass disc about 100 mm. in diameter, while the movable electrode was also formed by a brass disc, but only 37 mm. in diameter. To prevent the action of sharp edges, the edges of the discs were rounded off. The temperature of the surrounding air during these experiments was 14·75° C.

The results obtained are as follows:—

Difference of Potential.	Striking Distance.	Difference of Potential.	Striking Distance.
2,000 volts ...	0·67 mm.	10,000 volts ...	4·80 mm.
4,000 „ ...	1·59 „	12,000 „ ...	6·46 „
6,000 „ ...	2·53 „	15,000 „ ...	10·23 „
8,000 „ ...	3·60 „		

The last reading, giving the striking distance for 15,000 volts,

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cannot be correct, as the sparks started from the upper edge of the smaller disc, and not from the nearest points between the two discs, the smaller disc being placed above the fixed one.

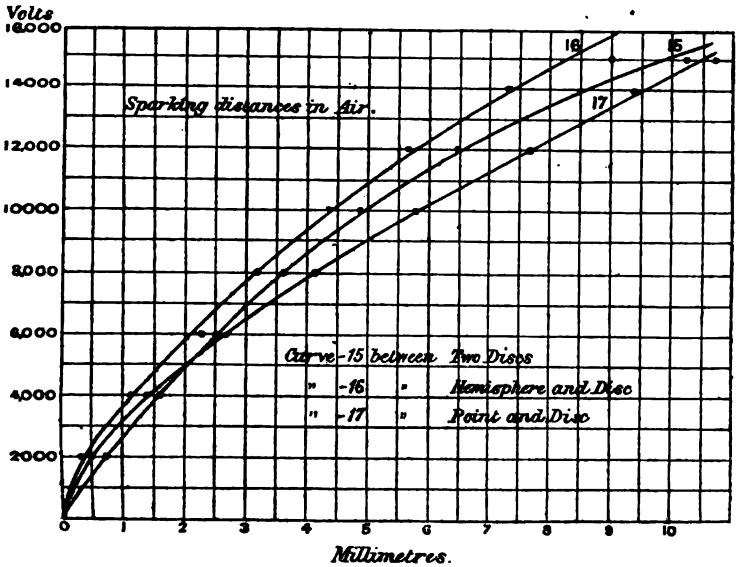

FIG. 6.

Curve 15 is the graphic representation of the above table.

For the next series the arrangements were the same as before, with the exception that the smaller movable disc was replaced by a half-sphere of 10 mm. diameter. The temperature of the air during these experiments was from 16° to 17° C., and its humidity 80 per cent.

Difference of Potential.	Striking Distance.	Difference of Potential.	Striking Distance.
2,000 volts ...	0·45 mm.	10,000 volts ...	4·37 mm.
4,000 „ ...	1·40 „	12,000 „ ...	5·65 „
6,000 „ ...	2·30 „	14,000 „ ...	7·32 „
8,000 „ ...	3·25 „	15,000 „ ...	9·02 „

These results are plotted in Curve 16.

After this the movable electrode of the spark micrometer was replaced by a steel point of an angle of 60°, its section

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being an equilateral triangle of 5 mm. sides. This point was also re-polished after each spark; the spark, however, did not always start from the point. During these experiments the temperature of the air was 12° C., and its humidity about 50 per cent.

Difference of Potential.	Striking Distance.	Difference of Potential.	Striking Distance.
2,000 volts ...	0·40 mm.	10,000 volts ...	5·78 mm.
4,000 „ ...	1·26 „	12,000 „ ...	7·60 „
6,000 „ ...	2·66 „	14,000 „ ...	2·37 „
8,000 „ ...	4·08 „	15,000 „ ...	10·70 „

Curve 17 embodies this table.

After this the influence of an alteration in the frequency was tried by lowering this to 80 ∿ per second, and repeating some of the first experiments with disc electrodes. The striking distances for 4,000 volts and for 6,000 volts under these circumstances were found to be 1·47 mm. and 2·30 mm., or only slightly different from those produced by a frequency of 100 ∿ per second. From theoretical considerations it might have been expected that the distances would vary directly as the frequencies.

In order to investigate the influence of capacity, one or more coils of gutta-percha-covered wire were connected to the circuit of the transformer in parallel with the spark micrometer. The first experiments were made with a frequency of 100 ∿ per second, the two disc electrodes, and a capacity of 0·113 microfarads. The temperature of the air was 16·5° C., and its humidity 79 per cent.

Potential Difference.	Sparking Distance. With Capacity.	Without Capacity.
4,000 volts ...	1·53 mm. ...	1·59 mm.
6,000 „ ...	2·21 „ ...	2·53 „
10,000 „ ...	4·17 „ ...	4·50 „

A further experiment under similar conditions, but with a capacity of 0·28 microfarads in circuit, showed that the sparking distance at 10,000 volts was further reduced to 3·94 mm.

These tests were repeated with other electrodes, viz., the spherical surface and the disc.

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At a difference of potential of 10,000 volts the sparking distance turned out to be—

1. With 0·14 M.F. capacity in circuit ... 4·08 mm.
2. Without any „ „ ... 4·50 „
3. With 0·14 M.F. „ „ ... 4·06 „

After that the steel point and the disc were used as electrodes, a capacity of 0·14 M.F. being connected to the circuit, and the results were—

Difference of Potential.	Sparking Distance. Without.	With Capacity.
4,000 volts ...	1·26 mm. ...	1·14 mm.
10,000 „ ...	5·78 „ ...	4·83 „

All these experiments point to the fact that the sparking distances in air are diminished to a slight extent when capacity comes into play.

With a view of ascertaining how the voltage is propagated along a circuit, the position of the micrometer relatively to the cable was varied.

The experiments detailed above were made with the spark micrometer connected to the beginning of the cable; they were afterwards repeated after inserting the spark micrometer in the middle of the cable, and again after it had been connected to the end of the cable. In all three positions the same striking distance was observed under similar circumstances, which tends to prove that the voltage is propagated uniformly over the whole length of conductor, even if there is capacity in some parts of the circuit.

As stated above, the voltage experimented with was measured by means of Sir William Thomson's static voltmeters; but it is undoubtedly the highest ordinate of the wave which causes the spark. This "real" voltage can be found, if sine waves are assumed, by multiplying the measured volts by $\sqrt{2}$, and these new figures should be applicable to constant continuous currents.

In this way the results of Curve 15 were corrected, and compared with some results published by Warren de la Rue in *Nature* on the 12th September, 1878. Assuming that the E.M.F. of his

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cells was equal to 1·03 volts, and reducing his results to volts and millimetres, they compare as follows :—

Difference of Potential.	Striking Distance. Warren de la Rue.	Siemens.
2,000 volts ...	0·45 mm. ...	0·25 mm.
4,000 „ ...	0·95 „ ...	0·80 „
6,000 „ ...	1·55 „ ...	1·45 „
8,000 „ ...	2·15 „ ...	2·15 „
10,000 „ ...	2·85 „ ...	2·85 „

On referring to the other curves published at the same time, it will be seen that Warren de la Rue found the sparking distances between two plain surfaces and between two spherical surfaces not to differ very much from each other. It may, therefore, be allowable to compare the mean values of these two curves of his with Curve 16 for the sparking distance between a hemisphere and a plain surface after the values of the volts have been multiplied with $\sqrt{2}$.

Difference of Potential.	Striking Distance. Warren de la Rue.	Siemens.
2,000 volts ...	0·36 mm. ...	0·25 mm.
4,000 „ ...	0·87 „ ...	0·80 „
6,000 „ ...	1·50 „ ...	1·45 „
8,000 „ ...	2·10 „ ...	2·16 „
10,000 „ ...	2·75 „ ...	2·85 „

Both these tables show that the results are practically identical, and consequently it may be concluded that the secondary current of the transformer used for the experiments is really a sine function.

Some further experiments were made with the same alternate-current machine and transformer, and a frequency of 100 $\sim$ per second, to determine the striking distance in various insulating materials. The differences of potential were again measured by Sir William Thomson's static voltmeter, or, in the case of the higher ones, by his volt balance.

One series of experiments, represented by Curve 18, was made by connecting the terminals of the transformer to the inner and outer conductor of concentric cables the insulation of which

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consisted of impregnated fibrous material. The difference of potential was then gradually and slowly raised until the insulation gave way.

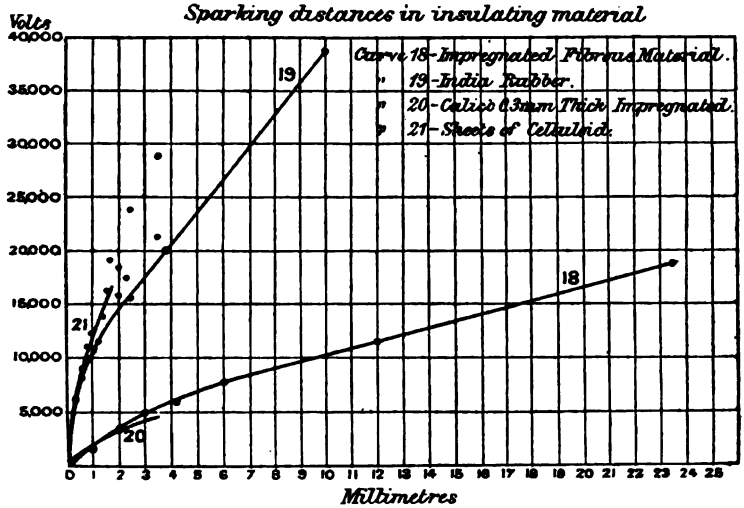

FIG. 7.

Curve 18 really consists of two parts, the lower of which was obtained by breaking 35 short samples of cable, made with various thicknesses of the insulating material; the upper part, above 7,000 volts, was determined by breaking actual cables, not less than 50 yards in length, specially manufactured for these experiments.

The next series of experiments dealt with india-rubber insulation, but results proved to be extremely irregular, although a curve (19) has been plotted which gives the minimum voltage at which sparking occurred at the various thicknesses.

The following table gives the details of the samples tested, and under what voltage they broke down:—

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No.		Thickness of India-Rubber.			
1.	40 in. of 880 H wire ...	3·5 mm.,	broke with	21,500	volts.
2.	10 yds. of experimental core,	3·8 mm.	„	20,000	„
3.	1½ „ „ „	2·5 mm.	„	16,000	„
4.	1 „ concentric cable,	3·5 mm.	„	28,000	„
5.	90 „ No. 504 core ...	1·2 mm.	„	12,000	„
6.	46 „ „ 505 „ ...	2·0 mm.	„	16,500	„
7.	50 „ „ 507 „ ...	1·8 mm.	„	18,200	„
8.	48 „ „ 508 „ ...	2 3 mm.	„	16,800	„
9.	5 „ „ 200 H wire	2·3 mm.	„	28,000	„
10.	1 „ concentric cable	2·0 mm.	„	17,500	„
11.	India-rubber sheet, ...	0·5 mm.	„	7,500	„
12.	„ „ ...	1·0 mm.	„	10,500	„
13.	Experimental piece of core,	10·0 mm.	„	38,000	„

The concentric cables were tested by connecting the transformer to the two conductors; the sheets were placed between the disc and the spherical electrode. The core, when in short lengths, was covered with tinfoil; when long, it was immersed in water; and the terminals of the transformer were connected, the one to the insulated conductor, the other to the water or the tinfoil respectively.

Another set of tests were made by placing calico on the disc of the spark micrometer, and by connecting the other pole to a copper brush, such as are used in dynamo machines, resting on the calico.

Several samples of calico were tried—

(*a*) Thin calico, 0·12 mm. thick, not impregnated.

(*b*) Silesia calico, 0·15 mm. thick, not impregnated.

(*c*) Thick calico, 0·30 mm. thick, not impregnated.

(*d*) Thick calico, 0·30 mm. thick, impregnated.

All four samples required the same lowest difference of potential—about 700 volts—for a breakdown. Different spots of the same sheet behaved very differently, some not breaking with even 1,000 volts; but at least one spot could always be found which broke down with 700 volts.

Curve 20 gives the results obtained with several layers of the impregnated thick calico. The layers were tightly pressed

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together before being tested, and the points of the curve give the minimum value of the voltage which broke them down.

The last curve (21) records the sparking distance through celluloid, which was tested in sheets placed between the two discs of the spark micrometer.

A good many of the high-tension experiments were made with a specially constructed powerful transformer, capable of giving at its secondary terminals a current of 2 amperes under a pressure of about 50,000 volts. This is now on view at the Crystal Palace Electrical Exhibition, and various experiments are shown to exhibit its qualities. Unfortunately, it is too heavy to be shown in this Institution.

In conclusion, it should be mentioned that all the experiments described in this communication were carried out at Woolwich, at the works of Messrs. Siemens Brothers & Co., mostly under the immediate care of Dr. Baur, who was freely assisted by the heads of the various departments whenever he encountered difficulties in the course of his work.

Mr. PREECE: When reference was made two or three times this evening to the fact that I read a paper *last year*, I became for the first time conscious that since that paper was read so many striking phenomena have been brought before us that I had almost forgotten many points involved in that paper, and I wanted before the discussion commenced not only to refresh my own memory, but also to point out to you two or three additions I have made. But first I want to express my great regret that the paper in its complete state has not been already in the hands of every member of this Institution. This is due, unfortunately, to a circumstance which we regret very much indeed —the illness of Mr. Webb. Had he been well enough, I have no doubt we should all have had copies, but perhaps before we next meet you all may have them. I have added several tests that have been kindly given to me, so as to make the paper more complete; and I have added a section that I think has a very important bearing on the whole subject, and I will, with your permission, read it to you. It bears on the durability of india-rubber. Mr. Preece

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Mr. Preece.

Now I want to lay great stress on the effect of age on india-rubber. It is all very well for us to see pretty figures like this on the board showing how much a specially prepared rubber will resist high pressures under fresh and peculiar circumstances; but we want to know what will be the effect of high pressures when they have been acting on conductors for more than twelve months. Now, on the next occasion when we meet here, I hope to have displayed on the table samples of india-rubber in all stages of manufacture, both for wire and for other purposes, which have been subjected to this particular test—a test which I have introduced into our specifications for india-rubber wire, and which I hope to have printed with the paper. On that occasion Mr. Roberts, who has had more experience in testing than most people, will himself explain how this test is conducted, and show results. Another point to which I should like to refer is this—that although I was very kindly assisted in the preparation of my paper by all the manufacturers with whom I placed myself in communication, there were one or two, unfortunately, with whom I had not communicated, viz.: Messrs. Hooper & Co., the Telegraph Manufacturing Company of Helsby, and the Okonite Company of Manchester. Naturally you may assume the result. Letters, rather severe in tone, implied neglect. I hope I have satisfied everybody that it was only an offence of omission and not of commission. This pretty book is, I think, to a certain extent the result of my omission. It is a book—a price-list—issued by a company with which I had had no acquaintance; it is called the "International Okonite Company, Limited." But the result of the omission has been, in the first place, to have myself supplied with several samples of this wire, which have given very high figures and very good results; but more, it has had the result of bringing out a price-book that in method and order and classification is worthy the study of every manufacturer present. It has also taught me a fact; and in giving you this fact, although the clock has struck half-past nine, I am sure you will excuse me for one moment in being a little frivolous. Now there is a story told about Artemus Ward that, in exhibiting one of his panoramas, there was a terrific blotch on it, and he said one day: "Ladies and

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" gentlemen, that blotch is a cow. For ten years I have shown that Mr. Preece.
" panorama, and I did not know what that blotch was; but this
" morning the artist told me it was a cow, and I am bound now to
" tell you that that is a cow." Now the reference is this: What is the meaning of "okonite"? I never knew until to-day what "okonite" meant. I do not know now what the substance okonite is. The samples shown to me were vulcanised india-rubber, and very good vulcanised india-rubber too. Okonite has yet to appear upon this hemisphere. But this was how the matter was explained to me: "Nearly all good insulators end in 'onite.'
" You only have to put 'O K'—'all correct'—before it, and there
" you have 'okonite.'"

Another regret I have to admit to you, and that is, I have not been able in my paper to make any reference to oil as an insulator. We heard a very high testimonial paid to oil by Tesla the other day. We know that oil has been used much in this country, and it is now in the hands of Messrs. Johnson & Phillips, and especially in the hands of Mr. Phillips, who has promised that I should have all the particulars relating to oil, but unfortunately he also is ill. But I want to mention another point in connection with oil as an insulator, and it is the fact that the use of oil as an insulator is, I believe, the invention of Professor Hughes. In the year 1858, before many of you were born, Professor Hughes read a paper at the Society of Arts on, I think it was called a viscous insulator—[Prof. HUGHES: A semi-fluid insulator]—a semi-fluid insulator. He showed that if you took a submarine cable and coated it first with a thin layer of gutta-percha, and put a loose gutta-percha pipe outside it, and filled the interstice with the semi-fluid material, you had an insulator which, by its mobility, recovered itself. He stuck a knife through, there was dead earth for a moment, but in another moment the insulation was restored. This first oil insulation is, I am afraid, forgotten. It was brought out again by Mr. David Brooks about ten years ago, and for quite eight years we had a line of pipes all the way from Vauxhall to Clapham Junction where the wires were insulated with oil. I think oil is destined to perform a very important function in future. Another point. At the next

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Mr. Preece.

meeting Major Cardew will probably commence the discussion with a description of a new method he has devised, and a paper upon which he read before the Royal Society a fortnight ago, showing how with a differential method and with a quadrant electrometer he is able to measure insulators far beyond anything on that board, showing visibly before us how you can measure 250,000 megohms with quite as much ease as we measure an ohm or half an ohm. Now I asked Mr. Simpson not to rub out those figures he had on the board. I am particularly interested in those figures, because those figures of De la Rue were the results of experiments he did for me in his laboratory in the year 1878, to determine the striking distance across air, with a view to forming lightning protectors, so that we may ensure that the surfaces of the lightning protector should be as near as possible without being struck by working currents—that is, that the distance between the earth plate and the line plate should be somewhere about the distance across which 700 volts would strike. The figures given by Mr. Siemens in his paper are the exact figures we found then. They are the figures that I have worked upon ever since, and the reason why I did not want them rubbed out was this—because, if you will remember, that in the earlier and smaller voltages there was always a considerable difference between the striking distance for direct currents and the striking distance for alternating currents. In the higher E.M.F. they equalised exactly; but when you come to 4,000 volts and under, then you found, at 2,000 volts, while the striking distance by De la Rue was 0·45, by Siemens it was 0·36; and so throughout till you get to 6,000 volts, the alternating currents always being smaller than the continuous. Now the reason why I have taken in my paper, and also why the Board of Trade has taken, a lower figure—that is, a striking distance of 2,000 volts across the tenth of an inch—is because we have to anticipate what may take place when cracks and fissures occur in insulating media, and where you have not to deal with a bright surface cleaned between each expe t; you have to deal with the dirty and damp surfaces. engineer will take a factor

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of safety of 1 in 4 or 1 in 5, and so I select 500 volts per millimetre; and that is why I in my paper, and Major Cardew in the Board of Trade rules, have taken 2,000 volts per 1-10th inch as the minimum. In this proportion of striking distance I have never yet found two men agree. Mr. Ferranti brought down the striking distance with a transformer of high frequency as low as 620 volts per millimetre. What Mr. Tesla has done I do not know. I am very sorry he is not here, as we might have got from him some interesting facts bearing on this subject. Mr. Preece.

The discussion on Mr. Preece's paper and Mr. Siemens's paper was postponed until the next meeting.

The PRESIDENT announced that, as the result of the ballot for new members, the following candidates were elected:—

Foreign Member:

William Stanley, jun.

Associates:

John William Forster Ashwin.
William Joseph Bamber.
Lieut. W. P. Brett, R.E.
George Henry Cottam.
J. A. Dawson.
Maynard Dodd.
Claud Edward G. Gilbert.
John Derby Hyde.
James Hardie McLean.
G. A. Nussbaum.
Thomas Henry Padgham.
James Edward Pearce.
Maurice Pitman.
Thomas Henry Smith.
Paul Winsloe-Phillipps.

Students:

Percival C. Austwick.
Walter James Bache.
Alfred H Bayes.
William W. Borman.
Robert Frederick Botting.
Frank C. Briggs.
Henry Ridley Burnett.
H. J. Cash.
Herbert George Crabb.
V. J. Delebecque.
John Fowler.
Oliver Freeman.
Arnold Hartley.
Walter Hawkings.
Fred. N. Haward.
Leopold William Heath.
John George Maxwell Hilton.
F. O. Hunt.
Alfred Claude Ivimy.
John Acheson Traill Marsh.

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Students (continued):

Wyatt Meacher.
David King Morris.
John Turner Morris.
Augustin Gifford Palgrave.
George Dudley Aspinall Parr.
S. G. Payne.
C. E. S. Phillips.
Roland Sydney Portheim.
David T. Powell.
George Henry Victor Roller.
William Henry Slater.
Charles Neame Spencer.
Straun W. G. Tamplin.
Arthur E. Tessier.
Ernest Helm Walters.
Robert A. West.

The meeting then adjourned.

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The Two Hundred and Thirty-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 18th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 11th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Messrs. G. L. Addenbrooke and Mr. T. Cushing were appointed scrutineers of the ballot for the election of new members.

The PRESIDENT: As Mr. Siemens's paper really divides itself into two distinct portions—one portion on transformers, and another on the sparking distance through different dielectrics—it has been thought by the Council that perhaps, on the whole, it would be better to take the discussion on Mr. Preece's and Mr. Siemens's papers separately. Therefore we will commence this evening with the discussion on Mr. Preece's paper. Major Cardew has kindly arranged some experiments in connection with the subject dealt with in that paper, and therefore, with your permission, he will now commence the discussion by showing you these experiments. The President.

Major CARDEW: I will first explain the intention of the apparatus I have brought here. The apparatus is arranged for the purpose of giving measures of very high insulation resistance. I do not pretend that the method is completely worked out yet, but as far as we have got it is extremely sensitive, and appears to be a satisfactory method; and it very easy to take a comparison between two insulating substances in a few minutes by this method which might take days by any other. The principle is this: You have an ordinary electrometer, with the quadrants connected in the ordinary way. The opposite quadrants are connected together. It is not necessary to draw more than Major Cardew.

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Major Cardew.

two—the two oppositely charged quadrants—because the two alternate ones are connected; the needle is connected to earth. The quadrants are connected to a service of electric potential. The whole system is kept as nearly absolutely insulated as possible. It is very easy to secure an insulation exceeding a million megohms. The limit of sensibility of the system is reached when the resistance to be balanced is of the same order as the insulation of the measuring apparatus. The electrometer we have there is, as you see, one of White's standard pattern, and far more complicated than is required for this system: you merely want the quadrants and the needle; and the suspension of the needle is not required to be at all delicate. Now, when you have a system perfectly insulated such as that, the position of the earth, with regard to the potential of the whole system, will depend upon the last point you touch in connecting up practically, and if the insulation is good the potential will remain as last touched. Now, if you connect on to one side the resistance which you wish to measure, on to the other side a resistance or a material which offers a resistance varying as its length, and if you make an earth contact on to that, which you shift along, you have a balance as soon as the resistances to earth on each side are equal, and the needle remains at zero, indicating an equal potential from the earth on the two sides. We find that according to the order of resistance we are measuring different materials may be conveniently used. We have some silk there which has approximately a resistance, so long as the air is fairly dry, of 250,000 megohms per inch; and we have some material, which is partly silk, which is about one-hundredth part of that resistance. It is quite applicable to much lower resistances in principle.

I will just show a few experiments now. Here is the insulated battery, which is a kind of Volta pile. It has 400 copper-zinc couples, highly insulated, you see, by long ebonite legs, standing upon sealing-wax, which has an enormously high insulating power. The two poles of this are connected to the quadrants of the electrometer. Now the whole system is fairly well insulated, as is shown by the spot remaining near the

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proper zero. If I touch the middle of that battery, I equalise the potentials of the two poles. You see there is a slight motion, which shows that the insulation is not quite equal on the two sides. The sensibility of the instrument can be tested by then touching the battery at one cell from the centre, and observing the difference of reading. This is generally from 12 to 20 scale divisions, thus showing that a variation of 1 volt in 180 is distinctly readable, or that the accuracy you can obtain from this is at any rate equal to 1 in 180. Now here we have a sample of an insulated cable. The ends have been carefully paraffined and a nail driven into the core, to obviate so far as possible leakage from the end. That is connected to one side. Of course, when it is attached, the light, you see, goes off, indicating earth on that side. Now I will try to balance the leakage against this material, which is partly silk, and has about 10,000 megohms per inch. But I do not pretend that this is absolutely constant, for it depends upon the humidity of the air to some extent, and for absolute measurement you want to standardise it. Perhaps you notice the earth is now transferred to the other side. Now you may at any time, of course, force earth on to the middle of the battery by touching that. We have secured an earth in this room. Although we cannot secure any power, they do provide an earth. We discovered a water pipe hidden behind a curtain, so we have got an earth. Now we are pretty near the balance. If I touch this, just to show you—touch it here—you see now it is travelling this way, indicating that there is a greater leakage on this side than on the other side. That, roughly, is about the resistance of that sample. We will put on another sample of cable, just to show the difference. I am forcing the earth on the middle now. Now you see it is going over to that side. Now we are getting near the balance. There is the balance—about 3 or 4 inches, as against 20 feet on the other. Of course even such a small piece of cable as that takes time to electrify, as is very easily shown by this method. Now you have got it perfectly steady. If I go out further, it will begin to travel to the left. See! it is travelling off to the left; and if I go closer, it will travel to the right. It is very applicable to measuring such things as the insulation of ordinary insulators, or

Major Cardew.

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Major Cardew.

anything of that sort which you want to get quickly. I will just show a piece of india-rubber. There is a specimen of india-rubber of definite thickness. I put it on an earth connection, and put that insulated piece of brass on to it and balance the leakage through the rubber.

I propose to further improve the insulation of the apparatus by having a specially made transformer, with a secondary suspended entirely clear by a silk thread, and in that way get a very high insulation for the apparatus; the electrometer being also suspended, probably. Now that piece of india-rubber will go on creeping, indicating electrification, for some time: it is gradually creeping to the right. That is to say, the insulation of the rubber improves up to a certain time; it takes perhaps five minutes to thoroughly electrify a little piece like that, or perhaps a minute ought to electrify a piece of that thickness. We may take that to be a balance. We will try with a piece of a different kind of india-rubber. This is not quite so good as the other.

Professor PERRY: Surely, if you need a minute's electrification, you will have very different results if you use alternating currents, as you propose?

Major CARDEW: Yes; but we shall get what we want to get with the india-rubber if used with alternating currents. An alternating current is, however, inapplicable with any considerable capacity. The sensitiveness of this arrangement to capacity is extraordinary. If you put your hand anywhere near it, it appreciably affects it. So long as you are using a continuous source, it does not vitiate your measurements if you allow time; but with alternating currents you could not, of course, distinguish between the two.

Now there is another use for this arrangement which I have also proposed, and I hope to get people interested to give it a trial, and that is in connection with supply at high pressure. Taking the same arrangement of electrometer, but without a mirror—you have a null method, and you only want to read approximate deflections—an indicating needle will be good enough. Connect the quadrants to the two sides of the circuit, and the needle to earth. If the potential from earth is the same

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on each side, the needle remains in the middle. That is the case so long as the leakage current on the two sides is equal, whatever the insulation may be: that is to say, if that side has an insulation of 1 megohm, and that side also has an insulation of 1 megohm, this needle will remain in the middle; but if that be a megohm, and that half a megohm, the needle will deflect to the side which has more earth. Therefore this arrangement indicates whether the leakage is equal on both sides of your circuit, whether the current be alternating or continuous. But if you add to that a very high resistance—say 100,000 ohms—connected to earth at one end, and the other end connected to a kind of three-way switch, which is also connected to your two quadrants, and if you at any time switch this artificial leak first on one side and then on the other, and note the effect produced, a very approximate estimate of the insulation on each side of the circuit can be obtained. The electrometer should always be in connection with the mains, so that the people in charge at the station can observe any alteration in reading which indicates the occurrence of some fault, and on which side it has occurred. The artificial leak may be about 100,000 ohms resistance; it can be made in a very simple form with a piece of graphite. I have to-day tried a piece giving over this resistance, with 2,000 volts on it, without any sensible heat at all. It appears to do perfectly well. If they then switch on momentarily this into connection with that, and observe the effect, they get equations sufficient not only to determine the insulation of each main, but a check equation as well. I find that answers perfectly satisfactorily. Major Cardew.

I have also tried another suggestion of mine, which is a sort of double gold-leaf electroscope—one electroscope on one wire, and another on the other. The gold leaves will diverge equally so long as the leakage is equal on the two sides. If any inequality appears, the leaky side collapses more or less. By this you can easily test exactly what your insulation is, and of course without consuming any power whatever.

Professor PERRY: Of course you are assuming that the leakage to earth of positive and negative mains is the same?

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Major CARDEW: So long as the needle is in the middle.

Professor PERRY: But at your machine?

Major CARDEW: Oh! it takes machine and all, of course. This is a case of where the resultant fault is it includes the machine.

Mr. Roberts.

Mr. M. F. ROBERTS: The estimation of the durability, or probable durability, of india-rubber when used for electric light cables is of so much importance that Mr. Preece has suggested it might be interesting if I exhibited apparatus which can be used for the purpose, and which will give very reliable approximate

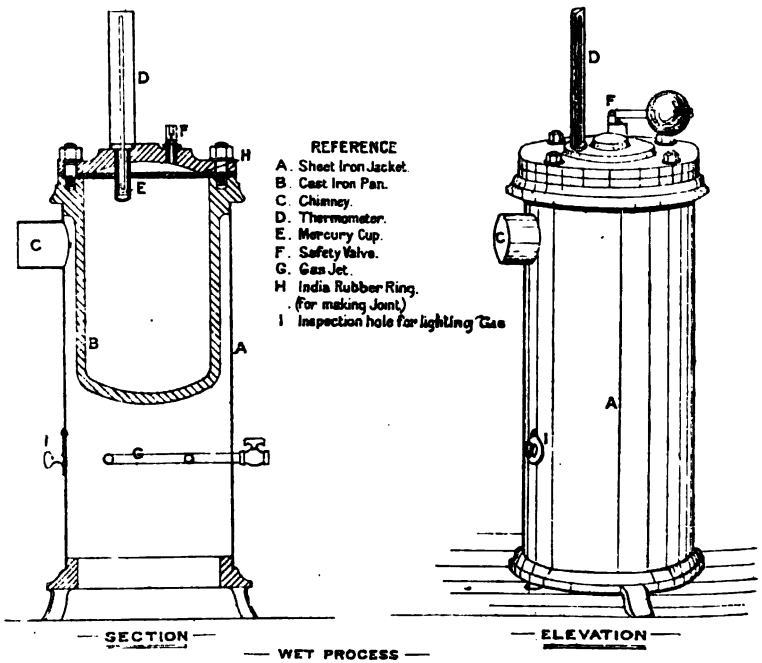

FIG. 1.

results without any chemical test. It is generally supposed—at least, I have heard many electricians express the opinion—that rubber used for insulating electric light leads must consist of pure Para rubber and sulphur only. But the result of various analyses I have made shows that, even when pure rubber is used, French chalk is frequently added; and in some of the samples I

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am exhibiting to-night I have found pitch, asphalte, silica, oils, and resins, and in other cases what I think is probably recovered rubber—that is, old rubber used again. To make a chemical analysis of every sample of cable before use would be practically impossible, and the test adopted by the Post Office, as Mr. Preece explained, is to subject samples taken from the cables to a moist heat of 320° Fah. for three hours, and to a dry heat of 270° Fah. for one hour. The apparatus we use for the wet test consists simply of a dentist's vulcanising apparatus. I have the exact apparatus here (Fig. 1). It consists of a cast-iron cylinder which has been proved

Mr. Roberts.

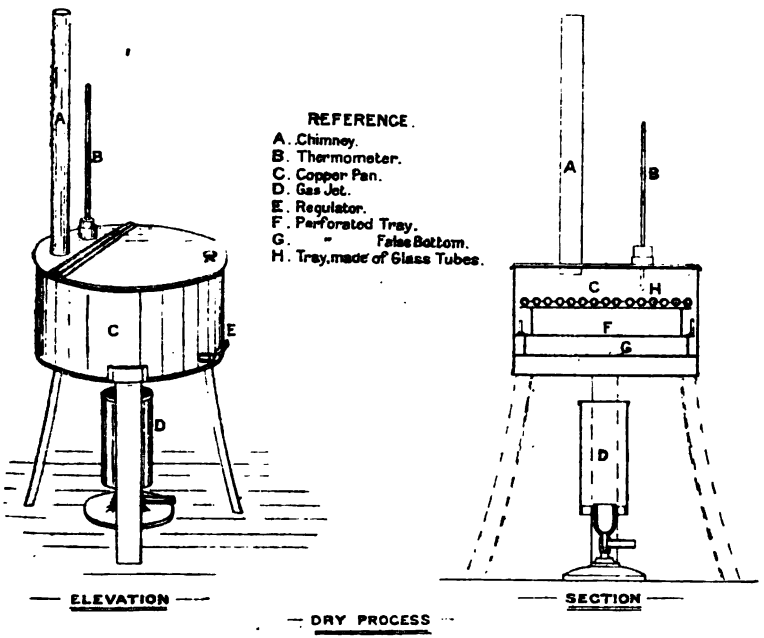

FIG. 2.

by hydraulic pressure up to 200 lbs. per square inch. This cylinder has an ordinary gas burner underneath. The cover is supplied with a safety valve, a mercury cup in which the bulb of a thermometer is placed, an india-rubber joint ring, and suitable bolts, or, rather, studs and nuts. To make the test, the cylinder is half filled with water, the samples are suspended above the water, the gas is lighted, and it is only necessary to adjust the safety

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Mr Roberts

valve to blow off at a pressure of 90 lbs., and to regulate the gas to keep the valve just blowing to ensure the required temperature of 320° Fah. being maintained. The thermometer shows if the safety valve is in order or not. The whole cost of the apparatus is but £3 or £4, and the test can be readily made by an unskilled man. The dry test is made in an ordinary copper drying oven with a false bottom, between the gas burner and the bottom of the oven itself (Fig. 2). In this test, if it is noticed at any time that the temperature is rising rather too rapidly, the attendant simply takes the plug out of the tube below the false bottom, cold air enters, and the temperature is at once lowered. To make a comparative test of two or more samples it is desirable to repeat the tests with the same specimens, and to observe which samples fail first. Our experience of rubber tested with this apparatus shows that qualities which stand the test can be relied on to last 15 years at least, and I have here samples supplied 15 years ago which stood the test, and which are still in excellent condition. I have here also some samples of cables which were submitted with competitive tenders some five years or so ago. Some of these samples failed in the test, and the present miserable condition of the untested length will show how unreliable the cables would have proved if they had been used. I had rather a difficulty in getting samples of rubber in sufficient number to pass round; but you see here some samples of rubber, some of which failed, whilst others stood the test. These are samples which failed. They were never brought into use, and were only manufactured a year ago: the rubber is perfectly useless. I have also brought with me a few samples of rubber, taken off very fine wire, to show that, no matter how thin the covering may be, it will, if of good quality, successfully stand these tests. The beauty of the apparatus is that it can be obtained at very little expense, and it can be used by an unskilled person. Everyone who is interested in purchasing electric light cables can obtain the apparatus for both tests, and so far as my experience goes, extending over 15 years, the results obtained with india-rubber are perfectly reliable.

Mr. Gray

Mr. WILLIAM GRAY: Unfortunately, I have not had the revised proof of Mr. Preece's paper long enough in my hands to look into

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it very closely. The subject is one of great interest to electrical engineers, and perhaps more especially to cable manufacturers, and we are very much indebted to Mr. Preece for drawing further attention to this matter of specification; but he has, I think, misunderstood the question at issue, and has rather assumed that electrical engineers have not already given due consideration to the points he now raises. He says that for submarine cables, and for telephonic and telegraphic underground cables, the size of conductor and the thickness of coat remain constant, so that it is perfectly permissible to specify that the insulation resistance shall be x megohms. I do not follow him in this, because with submarine and telephonic cables the ratios are often very different, and I do not think that such a comparison would be an accurate one for all cables. Mr. Gray.

Now electrical engineers generally, although much indebted to telegraph engineers for their research into the various qualities of insulating materials, have recognised for a long time that there are many mechanical considerations which must be dealt with which are of far greater importance for electric lighting than the insulation resistance alone, and to these considerations they have directed their attention. Mr. Preece, as he informs us, has only recently turned his attention to this matter, and I think, perhaps on many points, has not had the opportunity of examining closely what electrical engineers have meant in drawing up the present specifications for electric light cables. The general practice undoubtedly has been to start with the basis of a minimum thickness necessary for mechanical reasons, and then further to increase this as the diameter of the cable is increased, quite apart from any question of insulation resistance alone, this latter test being only used to show whether the mechanical application during manufacture of the rubber or other material has been properly carried out. This insulation has also been taken as a very handy mode of classification of the various cables, and we find in many price lists of electric light cables the number of megohms quoted is merely a minimum, most of the cables exceeding this figure. When we get to large cables the thickness of rubber covering is increased, but with a falling ratio, thus giving, when the same

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Mr. Gray. quality of rubber is used, a less insulation. This paper may be of great value in pointing out the necessity of careful tests on all cables. Mr. Preece has drawn attention to the fact that there should be a standard time of electrification. This is most important if any comparison is to be made at all. I believe some makers take an average of five minutes' readings, whereas others take one minute. One minute, being the most common, should, I think, be the standard adopted.

Mr. Preece also deals with the question of the voltage of the battery employed in these tests. Well, undoubtedly I think that, with ordinary measuring appliances (not, perhaps, the very sensitive apparatus which Major Cardew has shown us), where we have to deal with high resistances, it is necessary to use high battery power; and it is proved by practice that, though faults are developed and broken down, the cable itself is not permanently impaired: therefore it is advisable to use this high power, since faults should not exist in the cables, and if they do exist, they should be broken down and removed. Mr. Preece then goes on to fix the minimum thickness. I cannot say what this minimum thickness should be; it must, of course, depend on the manufacturer's appliances, the quality of the material used, and upon the weight of the cable and the conductor. We cannot, for the sake of theory, put it at 40 mils! I know of many wires covered with a less thickness which are thoroughly reliable, with no apparent defect in them whatever. It is obvious that this minimum thickness, whatever it may be, does not apply to conductors of widely different sizes. For example, a No. 16 wire should not be covered to the same thickness which would be necessary for a strand of 91/16, or some such large strand. For with this latter, even if the minimum thickness could be applied evenly and perfectly, if drawn underground, it is certain that damage would occur with such a thin coat of dielectric round so large a conductor.

This basis of 1 millimetre is, I take it, fixed as being the sparking distance through air. This sparking distance offers, so far as I am aware, no comparison whatever to the sparking distance through a dielectric. In air this distance may vary

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as we know from Messrs. Siemens, Crookes, and De la Rue's careful tests, from 24,000 to 80,000 volts per inch, according to the conditions of tests. Why, therefore, is 24,000 volts taken as the basis? This was only obtained under certain conditions; when the atmospheric humidity changes, and the shape of the electrodes is changed, the results obtained would be quite different. But, apart from this, why is air taken at all? except for the reason that the insulating material might crack, and that the spark might leap across the air in the crack. If this be the reason, then we should use an insulation that does not crack; and it is obviously unfair to put all insulators on the same footing, as, if the insulation does crack, the fact of the thickness being equal to sparking distance through air is quite insufficient, as the moisture in the crack will form a conducting medium for the discharge. I do not quite understand from this paper, when dealing with specific insulation, if we are to take it that a cable having, say, a specific insulation of 1·5 is only one-tenth part as good as another which has an insulation of 15. I think that Mr. Preece will agree with me that this is not the proper interpretation of this specification. Then what is? Durability is in no way dependent on specific insulation; that, I think, is generally conceded. I have had a number of cables tested, one of which gave an insulation equivalent to about 9·2 specific units, and after nearly five years, testing every fortnight or so, it still gave 9·2. Another core tested started at about 0·6 unit, and for this period of nearly five years remained at 0·6, thus showing that is possible to get both high and low insulation combined with durability. Another core —I will take a medium one—started at about 4 units, and came down to 0·4. This question of specific insulation has therefore, I take it, a good deal to do with the manufacture, as it is possible to manufacture a cable giving a high specific insulation that is not necessarily a durable one, or the manufacturer may prefer to make a cable with a low specific insulation, such as may be maintained for a number of years. Mr. Gray.

Further, this specific insulation has apparently nothing to do with the sparking distance; materials giving a low

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Mr. Gray. specific insulation may very often be found to resist in a higher degree the breaking-down strain than other materials giving higher specific insulation. The matter of classification I will not enter into at length; the difficulty is that there are so many specifications that the cable manufacturer is forced to vary his own according to the demands of the consulting engineer. I should be very glad, and no doubt other makers would also, if consulting engineers would combine, and come to some definite conclusion as to what they really do want. This would perhaps facilitate the matter of classification, which I agree with Mr. Preece appears to be a complicated thing at present.

The question of durability I would like to just touch upon. I am not aware that even with the apparatus this gentleman has been good enough to show us, and with Mr. Preece's specification here, we do get the results we want.

Mr. Preece says, "Of course there are many tricks in the trade "which will enable a maker to dodge any arrangement." No doubt this is quite true: there are numerous tricks in the trade; and I believe it would be advisable not to attempt to specify in the matter too closely, but rather to trust to the value the makers place on their reputation than to try to force them all down into common lines—that is to say, to attempt to make them use a material which many of them do not want to use. To show what I mean, I have made an experiment, since Mr. Preece added the paragraph on durability, upon a mixture similar to a class largely used in America at one time, and which has in it a very small percentage of new rubber, being made up of old goloshes, broken-up buffers, &c., &c., and really this resists the proposed durability tests much better than many other mixtures of higher quality.

Mr. PREECE: You had better send me some of it.

Mr. GRAY: I have some here I can give you. I do not see, however, why, if Mr. Preece must specify, he should specify a test only for india-rubber. In testing an impregnated fibre cable, are we to depend for insulation on the lead covering remaining waterproof? the impregnated fibre being only used to prevent contact. If so, should he not specify how this lead is to be

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applied?—pure or alloyed, hot or cold, and so on. If we are to depend upon the impregnated fibre alone for insulation, how will he specify the class of fibre to be used, the temperature and time of impregnation, and the oil or other material with which the fibre is impregnated? because all these produce important effects. When, for example, a fibrous material is employed, its mechanical properties might be entirely destroyed if the impregnation is carried out for too great a length of time at a high temperature. This, however, Messrs. Fowler-Waring may wish to speak upon. Messrs. Siemens, of course, believe their cable to be thoroughly reliable, and no doubt the Fowler-Waring Company think the same of their manufacture. We think we can make that class of cable also, but have not the same confidence in it. In addition, no specification is given for bitumen cables. What should this be? Mr. Gray.

In the matter of specification, it appears to me, we should thoroughly test the cables, and trust a good deal to the manufacturer for durability; and we must be content for the present to accept a different specification from every different maker, or lose the result of his experience.

Professor GEORGE FORBES: I wish to congratulate the author on the addition he made to his paper last evening which deals with the point which most of us consider to be the most important point of the whole, and which really had not been touched upon in the rest of the paper—that is to say, the question of durability. I must say that it is somewhat startling to hear from Mr. Gray that the test of durability given by the author is really comparatively valueless. I do not suppose, however, it is to be considered completely valueless, because those india-rubbers which do not stand the test may be discarded as useless; and if we are able to discard a certain number, then something is gained, even though others which pass may be not perfectly right. At any rate, this test is worth our study, although it may not be a final or complete test; but it is a step in the right direction, and we do want some means of getting at the durability of the material. The point to which my attention is naturally directed most is the question of the specification which Professor Forbes.

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Professor Forbes.

Mr. Preece has given us. I may say I do not think that this kind of specification helps us very much more, or that we will get the article we want any more certainly if we use the specification than if we act upon the lines that we have been going on in the past. There is really no point which is not specified in the ordinary specification except the point of stating the specific insulation of the material—a term open to grave objection. As a matter of fact, we do always specify the specific insulation. When we are told that a cable is 5,000 megohms a mile, or whatever it is, and that the ratio of the diameter of the cable to the core is $1\frac{1}{2}$, the specific insulation is involved in that. The specific insulation is simply proportional to the resistance in megohms per mile when the ratio of diameters is fixed; so that really what we have been using—the resistance in megohms per mile, with a given ratio of cable to core—is as definite an indication of what Mr. Preece calls the specific insulation of the material as anything that we might use. It is perfectly true putting it in the form Mr. Preece has suggested is worthy of consideration. It is more easy to compare the material with regard to this quality if we do compare it in some such way. But there is absolutely nothing more in this specification than what we have always been specifying. Now during the last few months we have been, in this part of the world and others, suffering from a very obnoxious malady, and the influenza has been prevalent; and the consequence of it is that we have had prescriptions as remedies, and preventives of the influenza have been published in our newspapers, both in the daily papers and the medical journals, and doctors have very wisely told us that this practice is by no means a good thing. There may be many cases where the specific recommended does some good, but there are many cases where it is doing no good, and the patient, by not calling in advice, is neglecting his rising temperature, and so forth, and actually doing himself harm by trusting to these prescriptions; and really more harm is being done on the whole by having these prescriptions suggested to those ignorant of medical science, because they consider they are capable of being their own doctors. I venture to think very

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much the same sort of injury will be done to electric light business by the publication of such a specification as this. I am quite sure there are many people who feel themselves incapable of drawing up a specification who will now say: "I am perfectly "able to draw up a specification for electric light cables. If I "comply with this specification, I know I have got the right thing: "Mr. Preece has told us so." And the opinion of the electrician to the Post Office, whether it be on the subject of electricity or any other subject you care to mention, is very deservedly regarded with great veneration. Now I may say I feel sure that this will be a tendency of this specification, and I think it is right that a protest should be made, to prevent people from thinking that when they specify in accordance with this specification they will get what they want. There is no test which will give us an indication of the quality of the rubber. Even the durabiilty test—which may be imperfect, but may have a certain value—is not included in this specification, which has been circulated throughout the technical Press, and there is no test to give us any idea of the quality of the india-rubber. To say that the specific insulation is any test whatever of its qualities for electric light cables is, of course, found to be erroneous. An uneducated person wishing to get an electric light cable of vulcanised rubber, we will say, and applying to several makers, finding out that the specific insulation of one is much greater than the other, and no greater price charged for it, of course goes for the higher: "it must "be the best," he says. But we know that it is perfectly possible to make extreme high specific insulation with rubber waste of the lowest quality. The highness of that specific insulation is no criterion of the merit of the cable; whereas, with this published specification going the round of the Press, many of those asking for electric light cables will imagine that it is. Mr. Preece will say, perhaps—this is the way in which he might argue—that it does not matter one bit about the quality of the india-rubber; for if the person ordering a cable uses this specification, then if he gets the worst rubber in the world it may crack as much as it please, he will get

Professor Forbes.

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Professor Forbes.

between the wire and his fingers a longer distance than the sparking distance in the air, and therefore will have prevented danger and provided against the cable breaking down. Now, again, on this point I join issue that the thickness of the dielectric is not a preventive of anything happening—danger to life or anything—when the insulation cracks. We want insulation, as Mr. Gray justly remarks, which will not crack, and that is what we are determined to have. And the thing we want about specifications is to find that the material will not crack. The thickness of the dielectric which we want in a cable is the thickness which will render it suitable for the mechanical strains which it will have to go through; that is the only question of thickness in the great majority of cables that I am aware we have to consider. However, I have no doubt that Mr. Preece will give us some further information about whether he considers that the thickness of the cable should be regulated by the sparking distance in air. I see no reason whatever; and at present, for my own part, my feeling is that we should gain nothing by specifying specific resistance of the material; and in no other point does this specification vary from the specification we have been in the habit of using; and all we can do at present, where it is impossible to make a thorough analysis of the materials, is to specify ourselves that the cables which are being supplied by the maker we employ are good, and to feel, as we do with most of our cable makers, that they are desirous of producing a good article which shall have a good reputation and last long. At the present moment I am afraid there is no other test which we can put it to which will get over the difficulty of settling between bad and good material. Of course these remarks chiefly apply to vulcanised india-rubber. Other questions are well deserving of discussion: lead cables and jute fibres impregnated are a very important class of cable, and ought to be discussed. Some years ago I drew attention to some failures in such lead-covered cables, and I have never regretted that I did so. When I spoke of those defects which had made themselves apparent, it distressed the minds of some makers of those cables, I know. I am sorry it did so. It dis-

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tressed the mind of so distinguished a man as Dr. Werner von Siemens, but it had a beneficial effect. It had two effects: one is that the prices of those cables immediately increased very largely; but at the same time such defects in those cables, I am glad to be able to say, have not been apparent since; and I am sure that those cables are doing excellent service now. Professor Forbes.

Mr. CROMPTON: I agree with the last speakers in that I cannot see the advantages of the specification proposed by Mr. Preece. I believe that such a specification may be a very dangerous weapon in the hands of partially instructed electrical engineers, and, unfortunately, it is just such men who would use it most. Mr. Crompton.

I think we have every reason to be proud of the high state of perfection to which the manufacture of rubber-covered cables has reached in England. Without mentioning names, there are several makers whose own specification can be safely accepted, and whose excellence of manufacture may be depended upon. I should much prefer, if I wanted a thoroughly good insulated cable, that the last speaker—Mr. Gray—for instance, should draw up his own specification, and that I should accept it, and rely on the reputation of his firm for his working to it, than, on the other hand, that I should draw up a specification for him to accept. I am certain that in the first case I should get a thoroughly good article, whereas in the second case my ignorance of the niceties of the manufacture would only make me liable to insert matters in my specification which would hinder Mr. Gray, and prevent him from supplying me with as cheap and efficient cable as he would have done if he been allowed to work out the matter in his own way.

As regards the important question of the thickness of the insulation which is required, I have thought out this matter carefully. One of the Board of Trade regulations as to the thickness of insulation required on overhead wires has been found difficult, if not impossible, to carry out when the insulated wires are of very small size. The insulated wire for a primary conductor to carry very small currents up to 2,000 volts is a case in point. The insulated conductors supplied to me for this purpose by the Silvertown Company is what they call their L quality, and is an

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Mr. Crompton.

exceedingly perfect sample of manufacture. The conductor is 7 No. 21 gauge stranded wire insulated with their highest quality of rubber up to a thickness of 34 mils. Outside this there is a braiding for mechanical protection. The insulation resistance is excessively high, exceeding on test 5,000 megohms per mile. The total insulation resistance of the whole of the circuits when in position has always been in excess of the Board of Trade requirements, but according to the Board of Trade rule the thickness of the dielectric is insufficient—it should have been 100 mils. In order to show that there was no advantage in the extra thickness of insulation, I had three samples of conductors prepared by the Silvertown Company, one with the thickness above described, another with 50 mils, and a third with 100 mils of dielectric, and these samples were submitted to a series of tests in the presence of Major Cardew at our Chelmsford works. It was then shown that the thinner insulation was in every respect practically equal to the thicker insulation, the increase in resistance in megohms per mile being only as 5,000 megohms to 8,000 megohms. In some respects, however, the 33-mil sample showed itself actually superior to the thickest sample. For instance, the thicker dielectric is much more easily ruptured on the outside of the bend of a kink than the thin one is. Again, when dealing with wind pressures or strains produced by accumulation of snow and the like, the thinner insulation shows to considerable advantage. But, after all, the main determining question is the cost, that of the thick insulation being so high as to be practically prohibitive of the distribution of small currents of high tension. The tables which I attach show how very considerably this increased thickness of insulation affects not only the cost of the cable itself, but also the supports and other material, and labour required for the erection of the line. It is easy to show, in the lighting of a small country town or village having its distribution system spread over a widely extended area, that this demand for increased and, as I assert, unnecessary thickness of dielectric may increase the total cost of the distribution system by 40 per cent.; and we know now, from what Major Cardew has stated in official correspondence, and which is corroborated by Mr.

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Preece in his paper to-day, that the sole object of naming the minimum thickness on small wires was on account of the supposed relation of the length of the air spark to the thickness of the dielectric. Mr. Crompton

I do not think my language is too strong when I say it was a monstrous idea for the Board of Trade to saddle the development of electric distribution with such a regulation, and on such highly insufficient grounds, and I therefore thank Mr. Preece for bringing the matter now prominently before you in order that it may receive adequate discussion. If the result of this discussion is to show that, after all, the air spark is the governing factor in the thickness, why should this thickness be that of the dielectric? why should not the total thickness of the dielectric added to that of its protection be taken, as it is really this total thickness which governs the distance between the workman's hand and the bare conductor? But, after all, is it not far safer to adopt measures to ensure against cracks than to encourage a thickness of cheaper insulation, which is likely to conduce to the formation of cracks? I think most manufacturers of high-class dielectrics will support me when I state that all materials with which rubber is loaded in order to cheapen it have the result of decreasing its mechanical strength as well as its tendency to chemically decompose.

I unhesitatingly, therefore, myself declare in favour of spending our money on the smaller thickness of the highest class rubber insulation, instead of on the increased thickness of necessarily inferior insulation proposed by Mr. Preece, and which I understand is approved of by Major Cardew.

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Mr. Crompton.

Table I.—Overhead Conductors.

SHOWING WEIGHTS OF CONDUCTORS, SUPPORTING WIRES, ALSO STRAINS DUE TO WEIGHTS AND WIND ON ONE SET OF CONDUCTORS—*i.e.*, PAIR OF PRIMARIES AND PAIR OF SECONDARIES. ONE SPAN = 1-36TH OF A MILE.

	Silvertown L Quality, Ordinary Thickness Dielectric.	Silvertown L, Board of Trade Thickness Dielectric.
Weight of steel suspending wire ... ... 3/16, in lbs.	5·4	7·4
Primary conductor ... 7/21, „	9·2	14·3
Secondary „ ... 7/18, „	16·5	16·5
Wind allowance for suspending wire ... ... in lbs.	87	100
Wind allowance for primary... „	153	248
„ „ „ secondary „	234	234
Total combined strains... ... „	505	610
Approximate breaking strain	3,000	4,000
Factor of safety	6	6
STRAINS ON POLES.		
Due to weight and wind on line, in lbs.	1,010	1,334
Bending moment at 21 feet above ground ... ... „	21,700	28,700
Bending moment due to wind pressure on pole itself ... „	4,000	4,800
Total strains on pole	25,700	33,500

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Mr. Crompton.

Table II.—Overhead Conductors.

SHOWING INCREASE IN COST OF MATERIAL AND LABOUR ON ONE SET OF CONDUCTORS, ENTAILED BY THE INCREASE OF THICKNESS IN DIELECTRIC REQUIRED BY BOARD OF TRADE RULES; EVERYTHING ELSE IN ACCORDANCE WITH THESE RULES. ONE MILE = 36 SPANS.

	Silvertown L Quality, 5,000 Megohms, Ordinary Thickness Dielectric.			Silvertown L Quality, 7,000 Megohms, Board of Trade Thickness of Dielectric.		
	£	s.	d.	£	s.	d.
2 miles primary conductor, 7/21	66	8	0	140	6	0
„ secondary „ 7/18	138	8	0	138	8	0
„ steel supporting wire ...	6	6	10	8	9	9
Tags	2	10	0	3	0	0
Tops, weather protectors, &c. ...	11	10	0	13	0	0
Insulators	4	10	0	6	6	0
Poles, 30, 5 in. at top...	16	10	0	21	15	0
„ 6, 6 in. „	4	7	0	—		
„ 6, 6 in. iron at top... ...	—			17	5	0
Sockets for poles...	26	14	0	29	14	0
Concrete at bases of poles	11	0	0	12	0	0
Earth plates, sundries	6	10	0	7	0	0
	£294	13	10	397	3	9
Or, as	100	0	0	136	0	0

I am indebted to Mr. A. H. Pott, the Engineer in charge of our Chelmsford installation, for the preparation of the figures in the above tables.

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Mr. Raworth.

Mr. J. RAWORTH: I was under the impression, when Mr. Preece closed the reading of his paper last year, that he had brought before the Society the most important paper we had ever had the pleasure of listening to. That impression has now solidified into absolute certainty. We have had the opportunity, resulting from that paper, of learning some most remarkable facts. Mr. Gray has informed us that the figures of high insulations placed at the head of the Silvertown lists must not be taken as indicating the quality of india-rubber with which the cables are insulated. This information gave me a severe shock. That very amiable and assiduous traveller—Mr. Lessels, I think—who represents Mr. Gray's firm, has had numerous conversations with me upon the question of insulation. On one occasion I asked him why they were in the habit of specifying 5,000 megohms per mile for a wire one-tenth inch in diameter, and exactly the same insulation per mile for a wire an inch in diameter. He said: "Oh! the reason is this: Our customers like "to know exactly what the amount of leakage on their system is." I told him that I for one should not care to have an exact knowledge of such an infinitesimal quantity. It does not seem to me to be necessary to spend large sums on insulation for the sole purpose of simplifying our calculations—in order, for instance, that we may know that if we have put down 200 miles of various sizes, we have an insulation resistance of 1-200th part of 5,000 megohms. Nevertheless, in using such cable I always had a pleasant feeling that if I had 5,000 megohms insulation I had a quality of rubber corresponding thereto. Now we hear from Mr. Gray that ground-up goloshes will produce a cable that will satisfy the Post Office.

Mr. GRAY: The Post Office test.

Mr. RAWORTH: If I were satisfied that a wire had passed the Post Office test, I should go comfortably to bed, in the belief that if there were 2,000 volts on the wire they would be there in the morning, with no interruption of the current.

Now the reason I have for believing Mr. Preece's paper to be the most important that has ever been brought before the Society is this: Our members are fond of reading papers on

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dynamos and steam engines, which buzz round and make sparks; but nobody before ever thought it necessary to read a paper on underground conductors, of which nobody thinks after they have paid the bill. But if we consider the amount of money spent on underground conductors, as compared with the sums spent on engines and dynamos, we shall at once see that Mr. Preece's paper was most opportune. We learn from the balance-sheet just issued that the Westminster Company have spent about £130,000 on underground conductors and conduits—a sum of money equal to the total cost of their plant, buildings, and the land upon which they stand. There can be no doubt, therefore, that, as electrical engineers have been in the habit of specifying very excessive insulation, there must have been put down in London at the very least £100,000 worth of material which might be better employed as money in the pockets of the shareholders. Mr. Preece has called attention to these facts, and whether his specification or his method of testing be exactly right or not is a matter of small moment. Mr. Raworth

Perhaps Mr. Gray will excuse me for giving you an illustration from the Silvertown price list, which shows that manufacturers of good cables do not always give good advice. Last year there was brought before our notice a new make of cable (Class Y), which was intended to meet a demand for a low-tension wire. The list stated that it was suitable for 500 or 600 volts, and then follows this clause: "The insulation used is of the best "quality vulcanised rubber, of sufficient thickness to give the "maximum flexibility combined with that of mechanical strength. "No attempt is made, as with our ordinary list, to keep the same "ratio of diameter of conductor to outside diameter of insulation, "and consequently the insulation resistance of most of the "cables varies with the size, but in no case will it be found to be "less than 2,000 megohms per statute mile when tested in water "at 60° Fah." The main point I wish to call your attention to is, first, that up to last year the Silvertown Company had been making their cables of all sizes of the same insulation per mile.

Mr. GRAY: That is for small wire. You will see there is a reason for that—a very good reason. After you get beyond what

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Mr. Raworth. you may call the working thickness, then you do not try to keep the same ratio.

Mr. RAWORTH: You do in the ordinary list.

Mr. GRAY: Because they are all smaller wires.

Mr. RAWORTH: But I see the insulation thickness is from 3-10ths up to something like 5-10ths of what is stated as "best "quality of vulcanised india-rubber." A friend of mine wrote to the Silvertown Company to ask what class of wire they would recommend for 100 volts of cable, and they recommended this cable, which has thicker insulation than the same firm has supplied under three years' guarantee for the City of London 2,000-volt primary circuit, and also very much thicker than a similar class of cables by Siemens Bros., which cables, when tested with 6,000 volts alternating after 24 hours' immersion in water, did not break down or show any signs of it.

It is evident, therefore, that the electrical engineer who has no better source of advice than the cable manufacturer must be in a most profound muddle. I hope Mr. Gray will forgive me for using the above illustration, but it so happens that no other trade list is so specific on the point I wish to prove. I daresay Mr. Gray will explain that I am quite wrong, and that there are two qualities of best india-rubber or two methods of measuring a quarter of an inch.

Mr. Preece was so good as to draw our attention in the course of his paper to a matter to which I have given great attention in the course of my connection with the electric lighting industry, and that is the denomination of the sizes of cables. Some years ago, when I had the honour of being connected with Messrs. Siemens Brothers, I found that their denominating numbers, arrived at by measuring the number of yards to the tenth of an ohm, were also capable of other interpretations. When I went to the Brush Company, I adopted the same idea in a simpler form, and began to number our conductors by the number of 1,000ths of a square inch in the section; thus 7/16 became No. 23, and 19/16 became 66, and so on. In each case the label put on the coil gives you the number of 1,000ths of square inch in the section of copper, therefore it follows that

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it gives you the number of amperes the cable will carry on the usual base of 1,000 amperes per square inch. By multiplying by 4 we get the number of yards to the tenth of an ohm, and the weight in pounds per 1,000 feet; and if you wish to know the loss of E.M.F. with the normal current, it is $2\frac{1}{2}$ volts for every 100 yards you have in circuit. For the use of my assistant engineer I worked out some time ago a very short mnemonic formula, which I call the rule of two, applicable to the distribution of electricity by alternating currents, viz.—that, when using 2,000 volts on a 2,000-yard circuit, with 2 per cent. loss, you require 2 lbs. of copper for every 2 lamps. Mr. Raworth.

Mr. A. SIEMENS: I wish to say a few words about Mr. Preece's paper, and I will say it very briefly, as time is late. The formula Mr. Preece gives for specific insulation can be made very easy by simplifying it in making the large diameter for the whole wire exactly 10 times the diameter of the conductor: then the logarithm $\frac{D}{d}$ becomes 1; and if you manufacture a wire of these dimensions, you get the specific inductive resistance of the insulating material employed by multiplying the resistance of 1 knot of the wire at 75° Fah. with $\frac{4{\cdot}39}{10{,}000}$. Mr. Preece makes fun of the classification of the wires; but the difficulty is that the electrical engineers specify so many different wires, and want you to keep in stock so many different wires, that you must needs make some use of letters, and have a lot of them. Then Mr. Preece says, with regard to testing: "There is a widespread impression that "insulation varies inversely with the potential of the battery "employed. This is a mistaken notion." With regard to this, some tests have been made at our works, and the following table gives the results:— Mr. Siemens.

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Mr Siemens.

Description.	No. of Leclanché Cells.	Insulation after One Minute's Electrification.
		Megohms.
Gutta-Percha Core	100	3,468
	200	3,366
	500	3,267
	700	3,117
	1,000	3,086
	1,200	3,021
High Insulating India Rubber Core	100	8,054
	200	7,916
	500	7,470
	700	7,268
	1,000	6,705
	1,200	6,245
Ordinary India-Rubber Core ...	100	513
	200	462
	500	419
	700	355
Lead Cable, C Impregnation ...	100	2,813
	200	2,674
	500	2,541
	700	2,224
	1,000	2,057
	1,200	1,798
Lead Cable, A Impregnation ...	100	2,602
	200	2,492
	500	2,425
	700	2,345
	1,000	2,274
	1,200	2,212

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Mr. Siemens.

You will see that in all cases, with very different insulating materials, the insulation resistance came down with the increase of differenee of potential. Then, with regard to the duration of time, it will interest you to hear that some cable which we had at Frankfort, and worked there every day for a few hours with 20,000 volts, had, before leaving, 1,204 megohms at 75° Fah., and when returned to our works the insulation was 1,200 megohms, so practically it was exactly the same. A second coil was 1,053 megohms before, and after it came back 1,009, so practically it was not altered at all.

Professor PERRY: Had it been in use, or only on show?

Mr. SIEMENS: Every day for a few hours it burned a number of lights at the Marine Exhibition at Frankfort; it was buried underground and used. Then, with regard to the duration of lead cables, I would like to confirm Professor Forbes. They seem to last exceedingly well if well protected against injury. We have now laid about 16 miles for the London Electric Supply Company, and it has been in use for two years without the slightest mishap. I think in one case some other people were digging up the street and damaged a joint box; but that has nothing to do with the insulation of the cable, and that is the only thing that has happened.

The PRESIDENT: The scrutineers report that all the following candidates have been elected:—

Foreign Members:

Professor H. Hering. | August Heilborn.

Members:

Paul Bedford Elwell. | Francis Teague.

Associates:

C. P. Buckell. | Thomas Joseph Clarke.
Thomas Clark. | George Neill.

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Students:

G. H. Baillie.
Leonard Barlow.
Cuthbert W. Barnes.
Percy Harry Budgen.
William Ranson Cooper, M.A.
Charles V. Drysdale.
George Harold Dutton.
Wilfred H. Everett.
Henry C. D. Fearon.
Edwin Stanley Franklin.
Bernard Booth Granger.
Julian Halford.
William Thomas Hodgson.
Hugo W. Keiffenheim.
Percy W. Paget.
M. S. Poore.
Arthur Henry Roe.
H. Sydney Smith.
Alan Smout.
Arthur Stanley Tugwell.
Claude Henry Chaloner Woodhouse.
William Walker.

The PRESIDENT: The discussion on Mr. Preece's paper, and also on Mr. Siemens's, will be continued this day week.

The meeting then adjourned.

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The Two Hundred and Thirty-second Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 25th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 18th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Messrs. Vose and Fairfax were appointed scrutineers of the ballot.

The PRESIDENT: We will now resume the discussion on Mr. Preece's paper.

Mr. STUART A. RUSSELL: In discussing the question of how best to specify for an electric light cable, Mr. Preece has neglected altogether the mechanical properties and durability of the dielectric, and has thus put himself in such a position that it is really impossible for him to come to any conclusion which is at all reliable. He has stated, without taking into consideration the effect of these qualities, that a certain thickness and specific insulation should be specified for all conductors; but, Sir, I would compare this method of procedure with what is done with, say, engines or dynamos. What would a dynamo maker say if we drew up a specification in which we stated that certain proportions should exist between the weights of iron and of copper in the machine, or between, say, the number of ampere-turns on the armature and the dimensions of the polar air gap, and at the same time we said this should apply to all types and sizes of machines? I think a manufacturer would probably be somewhat surprised at receiving such a specification; and although he might make such a machine as a manufacturer, he would certainly decline all responsibility for its behaviour. We find, however, that Mr. Preece is really attempting to Mr. Russell.

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Mr. Russell. treat the subject of cables in much the same fashion; but I think our best plan will be to do with them as we do with engines and dynamos, and not attempt to enter into details, but go to a maker who has a reputation to lose, tell him what it is we want the cable to do, and leave the details of manufacture to him. There are a great many points in the paper on which one might speak, but there is not time to take them all, and I would therefore propose to confine myself more especially to the question of thickness required at high pressures. But before starting on that I should like to say that I endorse entirely the remarks of several speakers at the last meeting who pointed out that specific insulation was by no means a reliable guide to the quality of the material. Various facts were brought forward at the last meeting, and I have prepared, as an additional aid in support of this contention, a table of tests on rubber cables, taken from a number of tests made on experimental

TESTS ON CABLES INSULATED WITH VULCANISED RUBBER. TESTED IN WATER AT 60° FAH. AFTER 24 HOURS' IMMERSION AND ONE MINUTE'S ELECTRIFICATION. TESTING E.M.F., BETWEEN 500 AND 600 VOLTS.

INITIAL RESISTANCE.		RESISTANCE AFTER 4½ YEARS.	
Megohms per Mile.	Specific Insulation in Proposed Units.	Megohms per Mile.	Specific Insulation in Proposed Units.
5,500	5·58	1,750	1·78
3,800	3·86	340	0·35
611	0·62	630	0·64
1,180	1·20	1,200	1·22
1,500	1·52	1,530	1·55
14,500	14·72	14,600	14·82
660	0·67	1,300	1·32

rubber cores which have been going on for several years; and although I will not read out the whole of this table, I will just

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mention three cases. First, taking a high specific insulation, a cable having an initial insulation of 14·72, in the units proposed by Mr. Preece, after 4½ years gives 14·82; another, giving —to take a low figure—0·62 initial insulation, gives after 4½ years 0·64; thus showing that both very high and very low specific insulations may exist with dielectrics that remain unchanged for years. Mr. Preece has suggested that our salvation lies in striking the happy mean and sticking close to the normal, although I am sorry to say he has not told us what that normal is. But I have here an initial insulation of 3·86, which, I suppose, must be somewhere about the normal, and after 4½ years this has fallen to 0·35, or less than one-tenth of the initial insulation. Mr. Russell.

With these few remarks, I would now pass on to the question of the thickness, which is especially interesting to me, because I have had occasion to make a good many tests on that subject; and here I think Mr. Preece takes up an altogether untenable position when he tells us that the thickness of the dielectric should be settled primarily by the striking distance in air. He entirely neglects the fact that materials differ very much in quality, and that they all differ from air so far as disruptive strain is concerned; and it seems to me very unfair that good quality materials which do not crack should be handicapped by an inordinate thickness being required, simply because there are some materials which happen to possess this failing of cracking in rather a marked degree. But if we suppose it necessary to take these cracks into consideration, I then say that the thickness proposed by Mr. Preece is altogether insufficient, because the pressure will not then be maintained between two electrodes surrounded by air, but between two electrodes connected by a damp and dirty surface, and it is well known that under such conditions a spark will pass over many times the distance it would pass in air. With regard to the difference between the various dielectric materials and air, the paper read by Mr. Siemens the week before last affords us a very excellent proof of how great that difference is, and how wrong it is to treat them all on a similar basis; for, if we look at

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Mr. Russell. the curves on Diagram No. 7, we see how vastly superior vulcanised india-rubber is to impregnated fibre, and both of them to air. Mr. Preece has also quoted the Board of Trade rules in support of his suggestion as to the thicknesses to be used. Now, Sir, it is well known to all of us who are practically working at this subject that the Board of Trade rules have been drawn up with an entire neglect of the quality of the material. They specify a particular thickness: there is not a word in the rules which defines the quality; and, in fact, the result of the rules has been to induce people to look round for a cheap and nasty material which they can afford to put on to the thickness required by the Board of Trade without incurring very serious expense; and I believe this method of specifying for a particular thickness, and neglecting all other qualities, has the effect of absolutely undoing the intention of the rules, because it induces people to use a cheaper material which is more liable to crack, and when these cracks appear, the safety one expects to be gained by using great thicknesses entirely disappears. I should like to further emphasise this difference between air and other materials by quoting the figures of some tests I have made on rubber-covered wires of various qualities. I may say that the sizes were chosen so that the experiments should be made on very thin coverings, because it was considered that any difficulty of manufacture would be considerably enhanced with a thin covering, and whatever pressure these thin-covered wires might stand might very well be taken as a safe standard for all other wires. In the first place, however, I will mention the tests with two qualities of sheets, several sheets being tried in each case, and the average taken. The thickness in the first case was 20 mils (0·020 in.), specific insulation in Preece units about 10, and the sheets broke down at 11,600 volts. By a strange coincidence the other set of sheets, whose thickness was 21 mils, and specific insulation about 13, broke down at exactly the same pressure, taking the average of all the tests in each case. Tests were also made on three qualities of wire which I will call *a*, *b*, and *c*; *a* and *b* being ordinary stock wires used, I believe, for wiring fittings where the tubes are often so small that wires with a respectable

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diameter cannot be got in, whilst *c* was a length of experimental core specially made for this test. In all cases the conductor was 28 mils in diameter, and was covered with one layer of pure and one layer of vulcanised rubber to a thickness of 28 mils. A coil of each wire was cut into a number of equal lengths, each length was immersed in water, connected with a transformer, and the pressure gradually raised until the dielectric gave way, the time being probably two to three minutes from the moment of starting until it broke down. Of the wire *a*, whose specific insulation was about 1·25, 11 lengths were tested: one broke down at 4,500 volts, one at 5,300 volts, and nine between 4,800 and 5,000 volts, the mean being 4,910 volts. Of the *b* quality of wire, having a specific insulation of about 2·5, 10 lengths were tested: one broke at 6,100, one at 6,200, one at 6,650, and seven between 6,300 and 6,550 volts, the mean being 6,420 volts. The third wire, *c*, was of much higher specific insulation, and shows very clearly that specific insulation is no guide to the resistance of the material to disruptive strain. The specific insulation was 13, and of five lengths tested one broke down at 5,020, and one at 5,530 volts, the mean of the five tests being 5,266 volts. Now, Sir, it appears to me that the considerable regularity of the results obtained with each of these wires shows very distinctly that even with such a small thickness as 28 mils—very much below that proposed by Mr. Preece as his minimum—it is possible to get by careful manufacture very even results. Another point that has been raised is the effect of time in tests of this kind. Now another coil of wire *a*—specific insulation 1·25, thickness of dielectric 28 mils—was immersed in water, and a pressure of 4,000 volts was maintained on it until a fault occurred; when the fault occurred it was cut out, and the remainder of the wire again started on test. By this means a series of breaks was got, occurring after the following intervals of time:—The first occurred at 13 mins. after the pressure was applied, the second at 14 mins., the third at 19 mins., the fourth at 47 mins., the fifth at 1 hr. 12 mins., the sixth at 1 hr. 34 mins., the seventh at 2 hrs. 11 mins., the eighth at 3 hrs. 28 mins., the ninth at 7 hrs. 40 mins., and the tenth at 8 hrs. 53 mins. The remainder

Mr Russell.

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Mr. Russell. of the wire was then subjected to a gradually increasing pressure until it broke down, which occurred at 5,750 volts, or nearly 10 per cent. higher than the maximum pressure required in the first set of tests on the same quality wire. This clearly shows that pressures not much below the breakdown pressure have a very marked effect on the dielectric if applied continuously for hours, and that when working near the breaking limit a deterioration does take place through the gradual breaking down of weak places in the dielectric. In this case the pressure used was 90 per cent. of the lowest breaking-down pressure required in the first set of tests; but other tests made in the course of ordinary work—testing cables before their being supplied to customers—show that when a voltage considerably below the breaking-down pressure is used there is no bad effect on the dielectric. For instance, pressures of 20 per cent. of the breaking pressure, so far as my experience goes, will do no harm, and pressures of 50 per cent. of the breaking pressure may be continued on the wire for a few hours without doing any damage if the cable is originally sound. I may quote a case which occurred in the ordinary course of testing a cable, in which there was an incipient fault, although it was so small the usual insulation test did not show it. A drum of cable, testing at about 6,000 megohms per mile before the high-pressure test was applied, after two or three hours at 5,000 volts gave 1,300 megohms. The pressure was applied again for about an hour, and the insulation resistance fell to 300; whilst a third application of the high pressure broke the dielectric down altogether. The fault was then cut out and the cable put into the tank with the fault clear of the water, and it then gave exactly the same insulation resistance as when it was tested before the application of the high pressure.

This is, I believe, the very best test which can possibly be applied to cables which are to resist high pressures—namely, to subject a cable to, say, three times, or thereabouts, the pressure that is to be used, and to compare the insulation resistance as measured before and after the test to see whether there is any change in it; as, if the resistance is unaltered, the cable may then be passed as sound, and equal to withstanding the pressure for which it has been ordered.

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The PRESIDENT: We have another paper to discuss, and, as we have a paper promised for next time, we are anxious, if possible, to finish the discussion on both, this evening. Mr. Crompton has been good enough to bring some specimens of cable, and perhaps he would like to speak upon them. The President.

Mr. CROMPTON: I only put those in; I do not desire to speak in reference to them.

Mr. M. F. ROBERTS: I just wish to explain that the samples of—I do not like to say of india-rubber, but suppose I must—the samples of india-rubber which Mr. Gray manufactured out of old goloshes and so on, have not met the test I explained here last week. I have here the sample Mr. Gray gave me, and that it has not stood the test is self-evident. [*Sample passed round meeting.*] The point, however, is that nobody could possibly mistake this sample for india-rubber; it looks more like concrete than anything else, and has a disgusting smell. It altogether fails to pass the dry test; but even if this had not been the case, it would not be fair to say the test is unreliable for rubber, and in proof to produce a sample which no electrical engineer could possibly mistake for rubber. Mr. Roberts.

Mr. W. GRAY: That sample had already been tested. If it is subjected to two continuous tests, it may not survive them. Of course in such a test the question of time is of great importance. What I say is, that it is possible to make out of old goloshes and similar material an apparently good cable, which would pass Mr. Preece's test as specified. I do not say that it would stand several repeated tests, or continual application of heat; as I have already stated, such material is not durable, and Mr. Preece's test, as it stands, is not therefore a test of durability. Mr. Gray.

Mr. SWINBURNE: The sparking distance, taken by itself, has, I think, little to do with the questions before the meeting. The pressure is only one of the factors which determine the sparking distance in air; the form of the electrodes is the other. For instance, if two infinite discs 1 centimetre apart spark under 10,000 volts, the rate of fall of potential in the air is 1,000 volts per millimetre at the instant of rupture. If two points are substituted for the discs, the fall of potential is no longer uniform, Mr. Swinburne.

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Mr. Swinburne. so that near the points themselves it is much greater than 1,000 volts per millimetre. The breakdown thus begins at the points, and extends. The air is, in fact, torn. With long sparking distances the small discs commonly used are little better than points, the fall of potential being much more rapid at the edges than elsewhere.

The breaking down of a cable has, of course, nothing to do with the insulation in megohm miles, but a disruptive discharge in india-rubber probably resembles the same phenomenon in air. The question to consider is, then, not the average fall of potential taken across the whole of the covering, but the maximum rate of fall, which is at the surface of the wire. To secure a certain thickness of dielectric per 1,000 volts at the parts where the electrical stress is greatest thus demands a thicker coating for a small than a large wire. A particle of conducting material in the coating may start a tear in the coating, just as a sharp point will start a spark from an electrical machine. A point does not discharge an electric machine because it presents a large surface, but because it produces a distribution of potential such that the stress just round it is greater than the air will stand.

What we want to know is the disruptive strength of materials—that is to say, the volts per centimetre needed to break it down, the stress being uniform. The insulation should be such that at all parts of the coating the stress is less than this, a factor of safety being of course taken.

If a crack exists in a cable, the sparking distance is unimportant. What has to be considered is the creeping distance, which, as Mr. Stuart Russell has pointed out, is enormously greater.

Mr. Esson. Mr. W. B. Esson: Referring to the specification in Mr. Preece's paper, he states as the 6th condition that "the weight "of copper used is to be such that the loss of volts between the "distributing switch and the furthest lamp when full load is on "shall not exceed 1 per cent." I take "full load" to mean all the lamps, and think the figure of 1 per cent. much too low. It ought to be, as a usual thing, 2 per cent., and for the reason that rarely in any building are all the lamps lighted at the same time. In an ordinary house, generally not more than half of them are

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alight, so 1 per cent. ought not to be a fixed thing for every installation, but there ought to be taken into account the particular circumstances regulating the number of lamps simultaneously alight in the building to be wired. With an allowance for a private house of 2 per cent. when all the lamps are on, there is always less than 1 per cent. drop in actual working. Another point is that Mr. Preece's clause 6 is not quite complete. Though the size of the cable is limited in one direction by specifying the number of volts loss which must occur between the distributing switch and the furthest lamp, it wants limiting in another direction by specifying the current-density which must not be exceeded in the shortest circuits. If you have a very short circuit it is possible to lose only 1 per cent. and yet dangerously heat the conductors, so there ought to be added to the clause something like this: The voltage loss shall not exceed 1 per cent (or 2 per cent., as the case may be), and in no conductor shall the current-density exceed 1,000 amperes per square inch. It is a very important point to specify the maximum current-density, as well as the loss of volts allowable. Mr. Esson

I might mention that cable makers are well aware that conductors of large diameter insulated to the same thickness with material of the same quality have a less insulation resistance than small cables. This is well shown by a manufacturer's list I have in my hand, which gives the specifications of all sizes of cables from 7/22 to 37/15, the former having a dielectric resistance of 6,000 megohms, and the latter 1,000 megohms per statute mile. Not only are the complete data of the cables given, but there is a drawing to scale as well, showing for all sizes the diameter of the copper, the thicknesses of the pure rubber tape, of the separator, of the vulcanised rubber jacket, and of the outside braiding and compounding. It would be advantageous if all makers of cables followed this example.

Mr. J. P. HOOPER: I would beg to state that, so far as my experience goes, the specific resistance quoted by Mr. Preece in his paper for Hooper's core is too high. I believe it is not more than two-thirds of the figure given in the paper, if as much; and it is only fair to manufacturers to have this corrected. On the Mr. Hooper.

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Mr Hooper

other hand, a higher insulation can be obtained by increasing the proportion of sulphur; but whether this is safe or otherwise remains to be proved by experience.

As far as durability is concerned, Hooper's core will not stand for a series of years alternately wet and dry in the tropics. After a few years shore ends of cable between high and low water have fallen in insulation. Whether this applies to the insulation being used for electric light wires at the present time is also a question of experience.

We have never varied in our manufacture: as the cores were made in 1866 so they are made now; and referring to a remark in the book of the late Willoughby Smith, where it is stated that he was not aware of what had become of the Persian Gulf cable jettisoned in the Channel in 1868, I am glad to be able to state that it is working satisfactorily at the present day.

Mr. Mavor.

Mr. A. E. MAVOR: I am sorry I had not the opportunity of attending any of the former meetings when this subject was discussed. I missed Mr. Preece's paper and the discussion. However, I have carefully read the paper, and those gentlemen who discussed it last week have been kind enough to tell me the substance of what they said. I think, in so far as Mr. Preece's paper related to the necessity of manufacturers clearly describing what they manufacture, it has done good, and will do more good. The numerous classes of conductors described by various symbols in the price lists of the manufacturers are, as a rule, confusing and unnecessary. The requirements of the trade in this department are well understood, and I believe that manufacturers should firmly specify the conductors most suitable for the various classes of work. It is not sufficient to state the number of megohms and the nature of the external covering, but the maximum potential at which the conductors may be used should be stated, and the nature of the installations for which they are specially adapted. The durability of the various insulations is perhaps the most important part of this question. The cable insulated with old goloshes, and referred to by Mr. Gray as being capable of passing the Post Office tests, has little interest for us, as it admittedly does not possess even sufficient durability to

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stand a repetition of the tests. It is not the testing, but the subsequent history of the cable, which is most important. Neither the Silvertown Company nor any of us ever made cables only for the purpose of testing; the cables are made to be used, and not merely for the purpose of passing a test. Mr Mavor.

Mr. STUART RUSSELL: I may say that our cables are the result of experience gained by testing for many years.

Mr. MAVOR: Probably; I should say certainly. But you do not make cables merely to pass a test.

The PRESIDENT: There seems to be a little confusion arising from a misunderstanding. I may point out that Mr. Preece has added a good deal about durability since the paper was first read.

Mr. MAVOR: I understand Mr. Gray has every faith, and all manufacturers have the strongest faith in their own manufactures. That is quite my position. Most of us have a strong belief in Para rubber, but few of us would place much reliance on rubber compounds. The more prominent the question of durability becomes, the less we shall hear of those remarkable mixtures. Mr. Gray does not believe it possible to make a durable cable on the principle which has been adopted by Messrs. Siemens and the Fowler-Waring Company for their lead-covered cables; but he will come to believe it: he will be obliged to.

Mr. ALFRED WHALLEY [*communicated*]: The determination of specific insulation in terms of Mr. Preece's convenient unit is dependent upon the accuracy of the values given to D and *d*. D is easily measured. As regards *d*, on page 609, vol. xx., Mr. Preece defines this as the "inner diameter" of the "insulating "sheath;" while in the table of tests on page 616 it is called the "diameter of the conductor," and measured as the outside diameter in the case of stranded conductors. If all the stranded conductors used for electric lighting were made up of the same number of wires and insulated with the same material, it would be a simple matter to determine the proper percentage reduction of the outside diameter of a strand needed to obtain *d* as the *virtual inner diameter* of the insulating sheath. Should this reduction be a maximum of 5 per cent. for a given material covering, a Mr. Whalley.

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Mr. Whalley.

stranded conductor formed of but a few wires, the error introduced by neglecting it and taking d as the full outside diameter of the copper strand would make the true specific insulation of the material to be from 13¼ per cent. to 4½ per cent. *below* the calculated value when the ratio of D over d, thus measured, has a range of 1·4 to 3. Insulating materials which entirely fill the grooves on the outside of a stranded conductor would thus have a considerable advantage given them, when being compared with insulating materials which do not fill the grooves.

It will help to avoid misunderstandings if specifications drawn up on the lines suggested by Mr. Preece contain a clause stating clearly how d shall be reckoned, having regard both to the numbers of wires forming the stranded conductors, and to the insulating materials which may be used.

The PRESIDENT: Mr. Preece is unable to be present this evening, but will communicate his reply in writing.

Mr. Preece.

Mr. W. H. PREECE [*communicated*]: I fail to read anything in this discussion to which I have to reply, for the speakers have so thoroughly answered each other. Frequent reference is made to an imaginary specification contained in my paper. The paragraphs numbered from 1 to 10 at pages 618 and 619 are merely principles underlying two or three specifications. They are not intended to be a specification *per se*.

It is clearly forgotten by the speakers that the Board of Trade rule for aerial conductors, printed at page 613, was recommended by the Council of this Institution itself to the Board of Trade, and was in consequence adopted by that Department. It was arrived at after considerable discussion and inquiry by a Committee of the Council. Mr. Crompton was a member of this Committee. It admits a thoroughly practical and useful standard, from which the thickness of the insulating medium can be determined, and it is the only standard that has as yet been suggested for high-pressure feeders and mains.

The PRESIDENT: I will now call on Mr. Siemens to answer some questions asked when his paper was read, and not replied to at the time owing to his then being away through ill health.

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Mr. A. SIEMENS: The first question was by Mr. Swinburne, who asked how long the E.M.F. was on the different cables before the breakdown occurred. All the experiments were started with a potential difference of about half the potential difference with which it was expected that the cable would break down, and then the potential difference was raised step by step, the steps being large at first, and becoming smaller as the limit was approached. Then, also, the raising of the voltage from one step to another was not accomplished at a jump, but it was accomplished in about two to five minutes. Each step was left on at least a quarter of an hour, sometimes for several hours, and even sometimes for days, but never less than a quarter of an hour. Mr. Siemens.

Then there was some question about the sparking distance in air—how long it took before the spark passed. The paper describes exactly how the experiments were made. They were all repeated several times, and the several sparking distances did not vary more than 1 per cent. for each voltage. In this case it seems that the time that the voltage is left on has absolutely no influence on the sparking distance—I mean in the cables; as some previous speakers have already remarked, the sparking would occur after a certain length of time. But the sparking in air would either occur at once or not at all.

The vulcanised sheet rubber used in the experiments was exactly the same as that used for the insulation of the cables.

Mr. Trotter asked how the thickness of the insulation of the concentric cables was determined. It was measured—and the result was also checked by calculation—from the outer diameter of the inside conductor to the inside diameter of the outer conductor.

Mr. Raworth asked whether the india-rubber of 10 mm. thickness which gave way at 38,000 volts was of the special quality mentioned in Mr. Preece's paper. It was not; it was of ordinary quality. The whole of the experiments brought one fact out very clearly, and that was, that the way the cables are manufactured has more to do with the resistance against sparking through than the quality of the rubber.

My attention has been called to the fact that an equation for determining the influence of Foucault currents was brought before

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Mr. Siemens.

the British Association some years ago by Mr. Swinburne. I was not aware of this when I wrote the paper; but I think somebody else will show that this formula is altogether wrong, so that it is not very important who has first brought it forward.

Professor Perry.

Professor PERRY: The paper Mr. Siemens has read on the heating of the iron of the transformer is, in my opinion, one of the most valuable we have had. He wants to know the heat lost in the iron, and he actually measures it as the heat lost in the iron itself, and not in the usual other places. Now, usually, the information that is not given us by the readers of papers on this subject—the information which is left out, not wilfully, but through ignorance of its importance—is often far more important than the information that is furnished, and hence the results that have been obtained are in most cases quite misleading. Sometimes, for example, an experimenter has found great losses in a transformer, but he does not tell us that the primary of his particular transformer was two coils in parallel, or that the secondary of his transformer was two coils in parallel. In all probability the manufacturer has taken great care that those two coils are equal in resistance. The 2 per cent. difference in the number of turns in the two coils, innocently neglected by the winding boy, would lead to loss by idle currents inside the transformer, not showing themselves in any way outside, of much greater amount than what is due to hysteresis and eddy-currents put together. Some readers of papers, because they have experimented on transformers with cores of finely divided iron, put down all the loss obtained by them to *hysteresis*—an overburdened word—and take no account, for example, of eddy-currents in the copper conductor. Sometimes, too, the number of ampere-turns in the unloaded transformer—the magneto-motive force—is taken from the primary current. Now the men who measure their magneto-motive force in this way would not take the ampere-turns from the primary current if the transformer were loaded, because that would be too absurd; and yet they do not notice that every so-called unloaded transformer or choking coil is really a loaded transformer, because there are always eddy-currents, and the eddy-current circuits may be replaced in calculation by a secondary

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circuit closed on itself. Working in this wrong way, some experimenters have obtained the actual shape of the hysteresis cyclic curve of magnetisation of the iron, and they do not see that really all their experimental results are completely accounted for by assuming constant permeability in the iron with a little saturation, and eddy-currents, with no hysteresis whatsoever. The problem attacked by Mr. Siemens seems to me the most important one in transformers. How much loss is there in the iron core over and above the loss due to eddy-currents? When you have found this difference—call it hysteresis if you please—all the problems in a transformer with many coils—finding the induction, the secondary currents, finding the secondary voltages, and so on—are easily solved, on the assumption that the permeability is constant. The numerical calculation of the most complex case is quite an easy thing: many coils with magnetic leakage, choking coils beyond the actual coils of the transformer, condensers in series and in parallel, and arranged with choking coils in all sorts of ways,—such numerical calculation is quite easy. The formulæ are exceedingly simple. Now I have taken very different cases indeed—exceedingly different cyclic laws of magnetisation. [*Drawing on the blackboard*] That is constant permeability. This, again, is constant permeability with a little saturation—no loop at all. Here is the ordinary well-known hysteresis loop, obtained from the slowly performed cyclical magnetisation of Professor Ewing and Dr. Hopkinson. I have taken these more complicated cases of the ordinary great loop festooned with smaller loops,—I have taken the most complicated loop that is possible to occur in the magnetisation of iron,—and in all cases I find that, to four significant figures, the calculation of everything that is to be calculated about a transformer practically comes to the same; it might be a little different in the end figure, perhaps, but practically you get the same answer as when you have constant permeability, with one exception, and that is in the calculation of the primary current when there is no load, or nearly no load, on the transformer. Professor Perry.

But the point before us now—and a most important point it really is—is, what is the primary current when there is no load on

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Professor Perry.

the transformer or choking coil?—what is the loss in the iron? And to answer this we require experiments such as Mr. Siemens has made. Every other experimenter's results which I have seen can be explained without suggesting hysteresis. With regard to his method of measuring, the value of Mr. Siemens's results, and of the whole thing, depends on the accuracy of his measurements. He makes his measurements by the rise of temperature in his iron test wire. Of course the accuracy of this measurement of temperature depends upon many considerations. In this measurement by rise of temperature the time must be short, the specific heat of his iron must be accurately known, the measurement of temperature by the resistance method must not only be very accurate as to amount, but must be known immediately. His correction for cooling is, I think, wrong, and I hope that his correction was a small one, for the way in which the temperature rises in a mass of iron like that is a very complicated affair indeed. But if the outside test wire gave practically the same results as his inside test wire—and I understand that this was the case—then practically no corrections need be made. I have not had very much time, I am sorry to say, to consider the paper; and even if it had contained all the details which I think that such a scientific paper ought to contain, I should hardly have been able to criticise them. What I want really to urge upon Mr. Siemens is that he should add to the paper all the details; the result is of such great importance that, in my opinion, he is called upon to add to that paper all the details that are necessary to allow us to criticise carefully. We will assume, of course, that Mr. Siemens has satisfied himself of the accuracy of all his measurements; but we, the readers of the paper, must also be satisfied. Passing over the difficulties that I have spoken of, if we consider, for example, the temperatures he measured, we shall see how delicate his measurements were. It is in the low powers—the small number of watts per cwt. of iron—that I am most interested, because it is only there that I feel a difficulty in accounting for the loss by eddy-currents merely. Let us take, for example, 25 watts per cwt. In his 1-mm. wire, by means of a very long calculation I find that this means one degree rise of temperatur

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in a thousand seconds. The resistance of that wire, I take it, was 0·37 ohm, so that the change of resistance in a thousand seconds was 0·002 ohm. Now I do not like, without being able to criticise this paper more fully, to base very general conclusions on the results of such delicate measurements as this in a wire which is being curiously heated. A thermo-electric E.M.F. of very small amount, with such small test currents as Mr. Siemens was using, might easily produce apparent change of resistance, and therefore apparent loss, of far greater amounts than what have been observed. For example, I should like to know, in the addition to the paper, if Mr. Siemens reversed his currents in making his tests of resistance. Professor Perry.

Mr. Siemens calculated the eddy-current loss and subtracted that from the total loss, and puts down the remaining loss to hysteresis. By his method there is no trouble with eddy-currents elsewhere than in the iron. The current-heat generated in the conductors causes no trouble, and there is no trouble with inaccuracy in the use of power meters. His calculation of eddy-currents, if his mere constants are correct, seems to me right. It is a method I have been using, and I put the results in a slightly different way. If the induction follows the law, $B = a \sin kt + b \sin rkt +$ &c., the average power in watts wasted in eddy-currents per cubic centimetre of iron, taking the specific resistance of iron as 10^4, is $P = 6{\cdot}25 \times 10^{-13} k^2 R^2 (a^2 + b^2 r^2 +$ &c.), if R is the radius in centimetres of the iron wire used. But, of course, in applying a formula of this kind, we have to remember that the induction per square centimetre is not constant everywhere. I do not mean merely at the ends in this cable transformer. Of course there is a little cross magnetisation there; but in the middle section, in any kind of transformer indeed, there is more induction near the skin of the mass and the skin of each wire. Of course, if there is any want of uniformity in the induction, one gets a greater answer for eddy-currents than this formula or that of Mr. Siemens will give. Again, in a loaded transformer, besides the ordinary 10 per cent. drop in eddy-current loss from what there is at no load, one always expects that there must be some-

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Professor Perry.

thing like 50 per cent. less on account of change of temperature, and consequent change of resistance. I will assume, again, that Mr. Siemens has taken account in his calculation of the change of temperature in calculating the eddy-current loss. Again, it is to be remembered that the law of voltage is not a true sine law, and the law of current is very different indeed. There is a frequency in the voltage of the machine due to the number of turns the armature makes in a second, as well as the usual frequency. Thus the expression, $V = 2{,}828 \sin 600\, t$, is usually taken as expressing the law when the frequency is about 95 and the effective voltage is 2,000. But the law may really be $V = 100 \sin 20\, t + 2{,}823 \sin 600\, t + 200 \sin 1{,}800\, t$, and the effective voltage would be exactly the same. But the inductions in the two cases are $I \propto 4{\cdot}711 \sin 600\, t$, and $I \propto 5 \sin 20\, t + 4{\cdot}705 \sin 600\, t + 0{\cdot}111 \sin 1{,}800\, t$. Now I do not say that the eddy-current loss in the two cases will differ, but our consideration of the whole problem of hysteresis is greatly affected by this magnification in the induction of a seemingly unimportant term in the voltage. Put briefly, what I mean is this: When an experimenter tells us that he keeps his volts constant, of course he means that he keeps his *effective* volts constant; and I have given a case likely to occur in practice, in which a term which was only of the importance of about 1-800th of the important term in the effective voltage becomes more important than the usual important term in the induction, its importance being magnified more than 800 times. Different methods of keeping the effective volts constant will have very different effects, depending upon the different self-inductions of machines, and hence it is nearly impossible for an outsider to criticise the results brought before him, or to co-ordinate one experimenter's results with another's. In fact, the machinery set somehow in motion by a current in an unloaded transformer is very complicated. Let the dynamo have a simple harmonic law of E.M.F., it can be shown that even when no hysteresis is assumed the current will possess large higher harmonics, and of course the induction must possess higher harmonics. The manufacture of these ripples by the iron, the energy wasted in manufacturing

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them, may be called hysteresis if one pleases, but it is not the hysteresis of slow cycles—it is quite a different thing—and will be of a very different kind if the dynamo does not follow a simple harmonic law of E.M.F., and it will greatly depend upon the self-induction of the dynamo. Professor Perry.

Dr. J. A. FLEMING: I will not spend any part of my allotted time in complimenting Mr. Siemens on his paper, but we must all be agreed that he has brought before us a practical subject, treated in a very practical manner. Dr. Fleming.

My first remark refers to a slight slip in the method of calculating the eddy-current loss in round wires. If ρ is the radius of an elementary cylinder taken in the unit length of the round wire of radius r, and if b is the induction parallel to the axis of the cylinder, then the electro-motive force acting round this circuit is $e = \pi \rho^2 \frac{db}{dt}$; if $b = \mathrm{B} \sin pt$, as usual—B being the maximum value of the induction—then $e = \pi \rho^2 \mathrm{B} p \cos pt$. The resistance, R, of the elementary cylinder is $\mathrm{R} = \frac{2\pi\rho}{c\,\delta\,\rho}$, where c is the conductivity of the iron; and hence the power lost in the elementary cylinder is, in watts, equal to

$$\delta w = \frac{e^2}{\mathrm{R}} = 2\pi^3 c \mathrm{B}^2 n^2 \rho^3 \delta \rho \cos pt \, 10^{-16}.$$

Hence we have, on integrating from $\rho = 0$ to $\rho = r$, and taking the average value of $\cos^2 pt$, the expression,

$$\mathrm{W} = \frac{\pi^3 c \mathrm{B}^2 n^2 r^4}{4} 10^{-16}.$$

The loss in watts per cubic centimetre is therefore equal to

$$\mathrm{Y} = \frac{c\pi^2}{16} d^2 \mathrm{B}^2 n^2 10^{-16},$$

where d is the diameter of the wire in centimetres.

When this last (and, as I believe, correct) expression is used for the calculation of the eddy-current loss in round wires of 1 millimetre in diameter, and for a frequency of 100, Mr. Siemens's numbers are found to be too small by about 15 per cent.

It has been pointed out lately by Mr. Steinmetz that the hysteresis loss in iron is expressible by a law of the following

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Mr. Fleming.

kind: $H = \eta n B^{1\cdot6} 10^{-7}$, where H is the loss in watts per cubic centimetre for a frequency n and a maximum induction B, and η is a constant varying from 0·002 to 0·004.

The loss in a Siemens cable transformer, as given by Mr. Siemens's results, is, however, more nearly given by the rule,

$$T = \left(\frac{d\,n\,B}{4 \times 10^6}\right)^2 + \frac{0{\cdot}625\,n\,B}{107},$$

where T is the total loss in watts per cubic centimetre, d is the diameter of the wires, B the maximum induction during the phase. The first term is the eddy-current loss, and the last the hysteresis loss. We must bear in mind that, owing to the great decrease in the conductivity of iron with rise of temperature, a transformer core will waste less energy when hot than when cold. If the eddy-current loss forms a large fraction of the total loss, this change will make a marked impression upon the total loss.

These experiments of Mr. Siemens seem to show that the hysteresis loss increases with the frequency. There is room for more investigation on this point, and particularly in respect of that question on which authorities are still divided, viz., whether the total iron losses do or do not decrease as the secondary load increases.

Knowing, however, the very great difficulty of calorimeter experiments, I join Professor Perry in the wish that we had had in the paper more details of the exact measurements of the quantities, for in determining very small rates or losses of heat exceedingly small errors sometimes have a great effect upon the final result.

Mr. Evershed.

Mr. S. EVERSHED: I must echo the wish that we had the actual observations taken during the tests, or at least a sufficient number of them to judge of the accuracy of the results. Mr. Siemens has told us in a paragraph of his paper that it is his intention to add to our store of useful knowledge. On the whole, he has certainly done so. As already pointed out by Dr. Fleming, Mr. Siemens has fallen into the error of calculating the eddy-current loss from the *mean* rate of change of induction in the iron core. But I doubt if Dr. Fleming's explanation was very clear. If he had pointed to the formulæ in Mr. Siemens's paper where we have the element of the power spent is equal to the square of an E.M.F.

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divided by a resistance—as, of course, all power is—and told us that if we are to get the result in *watts* we must have the E.M.F. in *effective* volts and the resistance in ohms, he need not have said more, and we should not have had occasion for all those bewildering sines and integrals on the blackboard. I think the best thing to do in discussing this subject is to supplement the paper with facts, and some of that useful information the absence of which Professor Perry deplored, but did not attempt to supply. Mr. Vignoles and I have been engaged for the last two years in investigating the subject of hysteresis in iron, with a special view of the wants of commercial men in designing transformers, and for that reason we have kept in sight all along what the actual losses were in real transformers. What we wanted to aim at was to get at the loss—not, perhaps, within 1 per cent., but sufficiently near to enable people to calculate losses beforehand. There is one thing, already referred to, in Mr. Siemens's paper—I rather suspect it may be another error—I wanted to ask whether he measured the specific resistance in the iron in his transformers. Mr. Evershed.

Mr. Siemens: Yes, it was measured.

Mr. Evershed: It is given as 9·8 at 0° C., and that is only true for pure iron. As a matter of fact, transformers are built of commercial iron, and not usually run at 0° C.—not in England, at any rate. It is very easy to correct the figures Mr. Siemens gives by taking the iron at a more ordinary specific resistance—say from 11 to 15 microhms per cubic centimetre. Mr. Siemens may be perfectly happy with regard to the error of 21 per cent. in his table due to his having taken the mean E.M.F., for in all probability the error in the value of ρ entirely cancels the first mistake, leaving the figures in the table substantially correct for the particular sample of iron used. I am glad to see that engineers have at last adopted Mr. Swinburne's method of calculating eddy-currents. Mr. Swinburne told us years ago that it was easy enough to calculate eddy-currents, and was laughed at; but we have come round to his method at last, and Mr. Siemens uses the formula given originally by Mr. Swinburne. Mr. Siemens's method of experimenting is somewhat similar to that adopted by Mr. Vignoles and myself; but

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Mr. Eversheds.

instead of measuring the temperature, as he does, by means of the change of resistance of iron wire, which it seemed clear to us was an unsafe and needlessly complex method, we used a clinical thermometer buried in the iron core. A good clinical thermometer is one of the most delicate and accurate measuring instruments we have, and can be read easily to 0·05° C. Ten or twelve readings of the thermometer taken during the heating give us a very accurate value for the total power wasted in the core, and calculation enables us to deduct the eddy-current loss, and in that way we get a very close approximation of the actual loss due to hysteresis alone. Mr. Siemens does not apparently make any allowance for the heating of a cable transformer by means of its own exciting current. This source of error is easily eliminated by maintaining a direct current, equal to the exciting current, during the time the cooling curve observations are being made.

Mr. Swinburne and Mr. Siemens give a formula by which eddy-currents in round iron wires may be calculated; perhaps it may be useful if I give here a formula by which they can be calculated in thin iron plates. The loss in 1 cubic centimetre of thin iron plates, say up to 40 mils thick, is equal to the square of the rate of change of induction-density into the square of the thickness of each lamina, divided by 16 times the specific resistance of the iron, or, in symbols: loss in watts $= \frac{\left(\frac{d\,B}{d\,t}\right)^2 t^2}{16\,\rho}$ per cubic centimetre —a very simple formula. The only thing you do not know accurately in this formula is the E.M.F., viz., $\frac{d\,B}{d\,t}$ —that is to say, the rate of change of induction of the iron. In a very thin lamina B may be uniform throughout the section. I leave the form taken by $\frac{d\,B}{d\,t}$ a blank, in order that anybody working it out may take it as a sine curve, or a zig-zag, or any other form they please. If the induction changes according to a sine law, we have $\frac{d\,B}{d\,t} = \frac{4{\cdot}4\ (\text{maximum B}) \times (\text{frequency})}{10^8}$ in *effective* volts per square centimetre of iron.

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Mr. SWINBURNE: I have not so much to say upon Mr. Siemens's paper as on the discussion. Beginning with Professor Perry: I think Professor Perry is very much to be congratulated. He has shown that the calculation of transformers is quite simple, and does not really depend on the magnetisation curve of the iron. We practical people have been trying to get that into the academical mind for years and years. Professor Perry then proceeded to integrate a series incorrectly.* Referring to Mr. Siemens's paper, I think that method of finding the power from the rise of temperature very inaccurate. As Mr. Evershed says, I was laughed at when I said it was quite easy to calculate eddy-currents. No doubt I shall be laughed at now when I say alternating power can be measured with a wattmeter. But it is perfectly accurate; it is infinitely more accurate than these round-about ways. Professor Fleming referred to the eddy-currents being reduced by rise of temperature. I think we ought to mention that that has been pointed out, I think, but am not quite sure, by Mr. Joyce or Mr. Cuthbert Hall in the *Electrical Review* some time ago. But, at any rate, in any decently designed transformer eddy-currents are so small that they cannot possibly produce the result frequently attributed to them. The only experiments showing decrease of iron loss with increased load are Mr. Mordey's and Professor Ayrton's. I think Professor Ayrton has now admitted that Blakesley's method of testing transformers is not accurate, and that that would account for his errors in making the efficiency come out too high at full load. Mr. Mordey's tests therefore stand alone, and their accuracy has been repeatedly questioned. As to the measurement of hysteresis under practical circumstances, and not with a ballistic galvanometer, I may point to another British Association paper of mine in which I measured a number of samples of iron with a wattmeter, so that I got it under a frequency of 100, and found the result agree very nearly with Ewing's. It varied about 5 per cent., but of course I had not the same qualities of iron.

Mr. Swinburne.

* *Note added later.*—Professor Perry's integration was quite correct; I have therefore cut out the proof of its inaccuracy.

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The President. The PRESIDENT: I think perhaps we had better adjourn the discussion, as two or three gentlemen wish to speak, and Mr. Siemens will have to reply. It will be necessary to conclude the dicsussion in time, however, to allow Professor Hughes to read a paper he has promised on "Oil as an insulator," because I understand he wishes to arrange some experiments. The discussion is now adjourned to March the 10th.

The following new candidates were declared elected:—

Associates:

James O. Callender.
Henry Clavell Crews.
James Edmund Edgcome.
Albert Griffiths.
W. J. Payton.

Students:

Harold William Clayden.
Harold Sparke Distin.
William Finlay, jun.
Albert Charles Ford.
John Pullen Phibbs.

The meeting then adjourned.

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JOURNAL

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The Two Hundred and Thirty-third Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 10th, 1892—Mr. ALEXANDER SIEMENS, Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on February 25th, 1892, were read and approved.

The CHAIRMAN: I think, gentlemen, as a sort of irregular proceeding, we may congratulate our Secretary on his having sufficiently recovered from his recent illness to be able to be among us again.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Alfred Inigo Baron. | A. M. Taylor.

From the class of Students to that of Associates—

Gustavus Frederick Möller.

Mr. Hall and Mr. Estler were appointed scrutineers of the ballot for new members.

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The CHAIRMAN: As some special arrangements have been kindly made by Mr. Wimshurst for experiments in illustration of Professor Hughes's paper, we will take that first. I therefore call upon Professor Hughes.

The following paper was then read:—

OIL AS AN INSULATOR.

By Professor D. E. HUGHES, F.R.S., Past-President.

At our last meeting, Mr. Preece kindly gave me credit for being the first to propose the use of oil as an insulator for wires conveying an electric current; and in accordance with a suggestion on his part, I should like to state upon what grounds I can fairly be considered to be the first to urge upon the electrical world the use of hydrocarbon oils, such as petroleum and rosin oil, for this purpose.

In 1858 universal regret was felt on the failure of the insulation of the first Atlantic Cable. From the first successful laying of this cable the insulation gradually became worse, until in a few days all signals failed. The cause of this was supposed to be due to minute flaws in the gutta-percha during its manufacture, which became worse by submersion, or that lightning, or the intense currents then used for the White-house induction coil, punctured the cable at several points.

It appeared to me (from some old experiments of mine) that what we needed was some form of insulation that possessed self-restoring powers, so that, if punctured by lightning, or our ordinary working currents, the puncture should be closed by some simpler process than having to take up a portion of the cable.

I thought that Nature showed us the way in which she restores punctures and mechanical injuries to all living objects. This is done by a flow of liquid sap to plants, or blood in animals; for if we make an incision in the bark of a tree, the sap flows out and hardens in contact with air; if we cut our fingers, blood flows out and heals the wound. Therefore I thought

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that if I could enclose in a cable an insulating self-restoring medium, the cable would not become dead at the first, or even after innumerable punctures. To carry out this object it seemed to me that a thick insulating oil, enclosed between the wire and its outer skin, would perfectly fulfil the conditions required.

Faraday had shown some years previously that oil of turpentine had a high insulating property, and this gave me great hopes that I should find, by experimenting with numerous samples of oil, the kind and quality desired for my purpose. Therefore I at once commenced a long series of experiments in order to find the best kind of oil and the most suitable form of cable to carry out this idea.

Knowing that I could not fairly test these oils, or a short length of cable, by our ordinary voltaic currents, I had recourse to the very high potential currents given by the ordinary frictional static electric machine. The method used was this: I charged a battery of Leyden jars to a known degree, which was indicated upon the Leyden jars by a Peltier electrometer; these jars, when charged, were put in communication with the short piece of the cable to be tested, the outside of which was coated with tinfoil, or placed in water connected with earth, and exterior of the Leyden jars. By this means, if the insulation of the few inches of cable was bad, the Leyden jars would instantly be discharged through the defective insulation; but if the insulation was comparatively perfect, the time of discharge of the electrometer was a correct measure of its insulating properties. Of course this method required that the Leyden jars should be perfectly insulated and hold their full charge for at least one hour when not discharging through a defective cable.

On making preliminary experiments on several samples of gutta-percha and india-rubber coated wires, I found a marked difference in each variety; but as these differences were of a constant character for each sample, it was easy to tell which of these had the highest insulating property.

These experiments showed me that if I wished to arrive at the true value of an insulating oil, I must make the experiments

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by a method by which the result should be entirely free from any interior or exterior coating. For this I took two small flat circular discs, 1 inch in diameter, which could be immersed in oil, and by an insulated adjustable screw I could compare the striking distance of these discs in air, compared with the same when immersed in oil, and could also observe the time required for a complete discharge of the Leyden jars through this medium.

On testing numerous samples of different oils, I found not only a great difference in the species of oil, but also a great difference in different samples of the same oil; consequently a table giving the results on different oils might be misleading, as a sample of superior quality of a certain oil (although inferior in many other samples) might give higher results than a badly selected sample of a really superior oil.

The only difficulty as regards rosin oil is one of manufacture, or, rather, of finding, and teaching the makers, the quality of oil best suited for the purpose; for I found, on obtaining samples of this oil from different makers, that a great difference existed as regards their insulating properties, ranging from worse than castor oil up to a degree superior to gutta-percha; and this is true of most oils; consequently, before using any oil, its quality as an insulator should be thoroughly known by constant electrical tests.

In selecting oil of high insulating quality, we must also have regard to the purpose for which it is to be used. Thus, as a self-restoring medium having very quick action, for condensers, transformers, or coils so closely wound as to be difficult for a thick oil to penetrate, a thin rosin oil, such as rosin spirit, might be best; but for cables and underground wires I found thick pure rosin oil best, because it was not only superior as an insulator, but it would not escape too rapidly at any large puncture.

Experiments were also carried on at the same time to observe the effect of any given oil on thin sheets of gutta-percha, india-rubber, &c. These were immersed in separate vases of different oils: they were weighed before and after prolonged immersion;

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the result being that some oils were found to be injurious to gutta-percha, and almost all, with the exception of castor oil, were more or less destructive to india-rubber.

I found, after numerous experiments, that pure rosin oil gave the highest insulation of all, for a spark that would pierce a given thickness of gutta-percha would utterly fail to pierce the same thickness of rosin oil; whilst, if the gutta-percha was pierced, its insulation was destroyed and could not be easily restored to its former condition; on the other hand, if by accident the rosin oil was pierced, it, by its own mobility, at once restored the insulation to its original state. Rosin oil, at the same time, had a preservative effect upon gutta-percha, for the sheets immersed in this oil had become slightly increased in weight, showing that it had penetrated into the pores of the gutta-percha; at the same time it was stiffer and tougher than before immersion. I will now show you an actual specimen of this fluid cable, cut off from a specimen one mile in length, made by the Gutta-Percha Company 33 years since. You will observe that the gutta-percha has absorbed during that time a large proportion of the oil, and that the gutta-percha is now, after 33 years' exposure to the air, apparently as good an insulator as when first made.

Experiments were now made as to the value of rosin oil when employed as the sole insulator of an electric cable. For this purpose, a short length of copper wire (say 1 foot) was coated, first, by being wound by well-dried cotton or hemp, or, better still, by a string wound round in wide open spirals, in order that the oil should penetrate as freely everywhere as possible; this was afterwards drawn into a small lead tube filled with rosin oil: thus the string or fibrous covering on the wire was simply to keep the wire concentric, so as to prevent its touching any part of the exterior tube without some separation of oil or oil-saturated fibre. It will be seen that these tests were identical, both as to form and material, in their essential points, with those patented by Mr. David Brooks, of Philadelphia, some 15 years later; but as these are clearly described in my patent of January 11th, 1859, there can be no question of priority in this respect, though no doubt

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many improvements suggested by practical experience in the mode of laying and manufacture are original with Mr. Brooks.

The insulation of this form of cable was not quite as high as solid gutta-percha, due to the fibre not being as good an insulator as the enclosed oil, but its mechanical powers of self-restoration and durability seemed to me to more than counterbalance its slightly less insulating power, for even with gutta-percha there are questions of strength and durability which ought never to be lost sight of in favour of a temporary high insulation; and I think that when the value of fluid insulation is more thoroughly known, especially for currents of high potential, it will be more generally used than at present, particularly as there are now no valid patent rights to interfere.

I again repeated these experiments, using a thinly covered gutta-percha wire for the inner conductor, drawn into a gutta-percha tube full of rosin oil: this gave me very high insulation—so much so, that I tried a bare copper wire in the gutta-percha tube alone, without any rosin oil: this, to my surprise, gave a far higher insulation than the best gutta-percha-covered wires as supplied to me by the Gutta-Percha Company. This surprised me very much, as the tubes, which I bought at an ordinary retail shop, were known to be of an inferior quality of gutta-percha. I found afterwards that this difference was due to the tubes having been long made, and kept in stock: they had gradually dried, and were free from combined moisture absorbed during their manufacture. I proved this by taking some newly manufactured gutta-percha, and heating it gradually for a length of time sufficient to drive off its moisture: this, when modelled on a wire, so as to form a cable, gave precisely the same degree of high insulation as the old tubes that I had purchased elsewhere.

I do not believe that the mechanical quality of the gutta-percha was improved by this operation; most likely it would have soon become brittle, for humidity, or an essential oil, seems necessary to its life and mechanical qualities.

I told Mr. Chatterton and Mr. Willoughby Smith, of the Gutta-Percha Company, of these results, and urged upon them the necessity of well drying the gutta-percha, and of manufactur-

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ing their cables, if possible, without constantly macerating it in water. I am not aware that my remarks had any effect, but this I remember—that in a few weeks after, they gave me a sample, which they called "special gutta-percha," which certainly had as good insulating qualities as my dried gutta-percha.

As I have already said, my main object being to produce a self-restoring insulation for cables and underground wires, I found that a thin fluid, such as rosin spirit or petroleum, might be objectionable, as at the point of fracture or puncture the fluid might escape with so great a rapidity as to be somewhat costly in its maintenance; but by employing a thick, heavy, insulating oil, whose rate of flow would be small, this would displace any water in the puncture, provided that there was slight head pressure given to the oil at a reservoir at certain landing stations: then it would answer all practicable purposes. For this I preferred rosin oil, which is already a thick, viscid oil, and can be made more so by the addition of solid rosin dissolved in it, or by the addition of palm oil residue, which has a remarkable property of thickening rosin oil.

In order to satisfy those who might object to even a very small leakage of oil at the puncture, I made several compounds of rosin oil, mentioned in my patent, that should harden when in contact with water, and thus prevent any waste of oil at the fault or puncture.

The static charge which I used was of high potential; the sparking distance in air, of the charge used, was about 1 inch, and it showed the remarkable insulation of rosin oil when 1-64th of an inch separation would effectually prevent such a spark passing through it. After having found this high insulating property of rosin oil, I coated all my Leyden jars, and all parts where I desired high insulation with it, and by this means I was enabled to retain a full charge of the Leyden jars for several hours, in an atmosphere full of humidity.

They have lately proved the value of oil insulation in Frankfort by the use of oil in transformers of 20,000 volts, and I am convinced that in all cases where we need a high insulation, together with the power of self-restoration, it can only be found

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in fluid insulators. It is useful in every case where it can be applied; it is far cheaper than gutta-percha, and I have no doubt that it will soon be used for insulation in condensers as well as transformers. Mr. Nikola Tesla, in his late charming lecture, spoke highly of the use of oil in his transformers, saying that it not only gave a perfect insulation, but had the power of restoring the insulation whenever pierced or punctured, as he believed his coils were at least several times every day.

I remarked during these experiments that, no matter how high the potential, the amount of leakage was equal in time, either with a high or low charge of the Leyden jars; that is to say, when the Leyden jars were discharging at a slow rate through a good insulator, the time of falling of the electrometer through any given number of degrees was very equal throughout its whole range from 200,000 to 1,000 volts.

Being desirous of repeating this evening some of my old experiments, I applied to Mr. W. H. Preece for some of the best samples of gutta-percha-covered wires as used by the Post Office, and which he has most willingly sent me. I also applied to Mr. James Wimshurst, the inventor of his remarkable static electric machine, who at once most kindly consented to lend the machine, and also design an arrangement by means of which we could show to those at a distance the different distances of the sparking through oil compared with air.

On testing this apparatus with oil, it seemed to show that the value of oil as an insulator increased both with the potential of the charge and also with the rapidity of alternations, for when the experiments were made by an impulsive rush so as to have the greatest number of oscillations per second—say 1,000,000—then even the poorest oil, such as castor oil, showed marked superiority over air or gutta-percha. This confirms the advantage of oil, and its value in connection with the rapid alternating high-voltage currents used in our transformers of to-day.

The experiments which we hope to repeat this evening, have already shown us that a spark, or charge, from a Leyden jar which would easily pierce 4 inches of air, will not pierce 1-19 inch of rosin oil; proving that oil, when resisting an impulsive rush of a charge, has 79 times higher insulation than air.

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To obtain these results we used a large and most powerful Wimshurst machine; but this evening, for convenience sake, we shall use a smaller machine. We may not be able to obtain quite as high comparative values, still we hope to obtain results showing oil to be 50 times better an insulator than air.

With the largest machine we obtained the following remarkable effects. The apparatus for holding the oil was a glass vase, 4 inches in diameter by 3 inches deep. The lower portion of this vase had a copper plate connected with one portion of the circuit. A brass knob, $\frac{1}{2}$ inch diameter, connected with the other portion of the circuit, could be raised or lowered in this vase in order to show the different striking distances when the vase contained air or oil. This gave the comparative results already mentioned; but, in addition, we noticed that when the knob was only covered three-quarters with oil, the spark rose from the upper portion to the upper portion of the vase, and then descended on the outside in order to reach the lower copper plate—a course of some 5 inches through air, compared with 1-19 inch through oil. If now the current was increased, the current, or spark, became a continuous sheet flow all over the entire surface of the vase, resembling in appearance a waterfall, or exactly imitating the well-known Gassiot cascade. On again increasing the charge, in an attempt to pierce the oil, the spark pierced the glass vase, making a small hole of about 1-32 inch diameter, at a place $\frac{1}{2}$ inch above the surface of the oil: thus the spark preferred to pierce the glass and travel several inches through air, rather than traverse a distance of 1-19 inch through oil.

I will now show several forms of oil insulation as applicable for submarine or subterraneous wires, and the remarkable power of self-restoration when the insulation is temporarily destroyed by a puncture or cut across so as to lay the wire bare for an instant.

It will be seen in the experiments following this paper that I have arranged a battery so that one pole is connected through a sensitive galvanometer to a metal tank containing salt water, the return circuit being completed through the salt water and wire to be tested to the other pole.

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If we take any sample of gutta-percha-covered wire and place it in the water, the insulation seems perfect and all that could be desired; but if we make a small incision with a knife, so as to leave a minute portion of the copper exposed, then (as we already know), on placing this in the tank, the water at once percolates through the cut to the wire, and its insulation is completely and permanently destroyed.

If we try this same experiment with oil cable, such as a bare copper wire in a gutta-percha tube containing oil, or, better still as a proof, a thinly coated gutta-percha wire, and this plunged into a small lead tube containing thick rosin oil, the whole having the same diameter as the gutta-percha-covered wires, we find, on testing this lead-covered oil cable, that its insulation is as perfect as the gutta-percha wires.

If we now cut through the lead tube and the gutta-percha wire once or several times so as to lay the wire bare, the insulation for an instant is destroyed, but in less than a second the oil percolates through the wound or cut, and its insulation is instantly restored to its previous high value.

I believe that these experiments sufficiently show the merits of a self-restoring insulating medium, such as rosin oil, for all purposes where a permanent insulation of wires conveying an electric current is desirable.

In conclusion, I will mention a few proofs of my claim to be the first to recognise the value of oil insulation for wires conveying an electric current, based upon some earlier experiments, but patented 11th January, 1859, entitled, "An Improved "Mode of Insulating Electrical Conducting Wires," which describes and claims most of that which I have already related. I will read a few sentences, in order to prove this fact.

After showing how the oil may be applied to submarine cables, and in order to claim its application to underground wires, the specification says (page 3, line 10): "With some trifling "modifications, the invention may be also applied for insulating "electrical conducting wires employed on land or underground."

As regards the materials used, it says (page 4, line 23): "I "propose and prefer to use rosin oil, rendered sufficiently thick

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" or viscid for the purpose by the addition of rosin or the solid " residuum obtained from the distillation of palm oil."

In order to show that the interior wire may be covered with a fibrous material instead of gutta-percha, it says (page 4, line 34): "Instead of bringing the soft or semi-fluid restorative " medium in direct contact with the enclosed wires, I sometimes " first coat the wires with a non-conducting material, such as " gutta-percha or india-rubber, and if preferred, the wire may " be previously covered with some fibrous material."

My invention being as well adapted for numerous wires in an underground cable as in a single-wire cable, it says (page 5, line 6): "And the invention is as applicable when two or more " electrical conductors are placed in one common tube or outer " covering as when only one conductor is employed."

If the patent is hurriedly read, it would seem as if I only intended to use the oil in connection with gutta-percha cables; but I foresaw the use of other outer covering, such as lead or iron tubes, for it says (page 4, line 37): "The insulated electrical con- " ductor thus coated or covered is then placed in a gutta-percha " or other tube." This phrase "other tube" referred to my experiments with an outer tube of lead or other metal; and I again employ the same phrase at page 5, line 13, where it says that the wires surrounded by the rosin oil "may be placed in the gutta- " percha or other outer covering."

A paper on this subject was read before the Society of Arts and published in their journal, April 15, 1859, in which I demonstrated by practical experiments the restorative powers of my fluid insulation.

Sir William Fothergill Cooke, the Chairman, in reply, spoke in praise of the results, and said he "also thought the invention " would be very valuable as applied to the street wires of electric " telegraphs, and these afforded more facilities for testing its " value." So it cannot be said that its applicability to underground wires was not foreseen at that date.

From 1858 to 1860 I tried by every means to get the various land and submarine electric telegraph companies to try this system, but in vain. I thus lost two years of valuable time, and

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then went to France in connection with my printing telegraph instrument, to which I devoted my whole time for many succeeding years, thus practically abandoning my self-restoring fluid insulation to the public.

The cause of its failure to be used then was due to the fact that the invention was made before its time, or that the need of its use was not then as great as at the present time. I am glad to say that now, thanks to electric light and power, the use of a self-restoring oil insulation is fast coming into actual practice under the names and patents of numerous succeeding inventors; so I sincerely thank Mr. Preece for citing my early work, and thus giving me this opportunity of claiming as priority, the work that I had done before and patented in 1859.

My best thanks are also due to Mr. Preece for the samples of gutta-percha wires, galvanometer, &c.; to Mr. James Wimshurst for the loan of his static electric machine, and for his great assistance in preparing some of our experiments on high-tension currents; also to Messrs. Grindley & Co., of Upper North Street, Poplar, for having supplied me with numerous samples of their best oil, from which I have selected the oil that I have used this evening.

Professor HUGHES: The experiments which I am now going to make are to a great extent a repetition of those I made before the Society of Arts in 1859, and by which I demonstrated the restorative powers of oil insulation when a cable was injured mechanically or otherwise.

In order to test the insulation of a short length of cable (say 10 inches), I have arranged a battery of three lithanode elements (6 volts). A wire from the positive pole is in direct communication with a small metal tank containing salt water, in which the cable or insulated wire to be tested is placed. By means of a wire from the cable the return wire passes through a telephone (in order to detect a very feeble loss of insulation) and a large vertical galvanometer (on which the audience can see the results). From the galvanometer a wire leads to the negative pole of the battery, thus completing the circuit through the insulation of the cable to be examined.

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If I now plunge a well-insulated gutta-percha wire in the tank, there is no perceptible loss of insulation in the telephone or galvanometer; but if I now take this out of the salt water and make a cross cut with a small hand-saw through the gutta-percha to the internal copper wire, and this is replaced in the tank of salt water, loud tones are heard on the telephone, and the galvanometer shows that a total loss of insulation has taken place of an entirely permanent character.

I will ask Mr. Preece to kindly sit near my experiments, in order to listen to the telephone and closely watch these effects. [*Experiment repeated.*]

I will now repeat this experiment with a cable containing rosin oil. This, as you see, consists of an inner thinly covered gutta-percha copper wire placed inside of a small lead tube containing the oil, the whole being of the same exterior diameter as the gutta-percha wire in the previous experiment. The oil cable is hermetically sealed at the ends by means of closely packed cotton fibre, so that the oil does not run out even when the lead tube is cut. [*Experiment made by cutting four deep, wide saw-cuts to the internal wire, after which the insulation was immediately restored by the oil.*]

Professor PERRY: Does it harden at once?

Professor HUGHES: No; in this experiment it does not harden nor flow out of the cable: the oil closes on itself, and there is no oil apparently runs out of this cable.

Professor PERRY: What would it be like under pressure?

Professor HUGHES: It has been put under several thousand atmospheres pressure by Messrs. Easton & Amos. You may say I have not cut it under water, but out of the water and in the air, which gives it time to heal. Well, I will now cut it under water. [*Experiment made with success.*] I think if any surgeon cut a man's leg like that, and said it would at once heal of itself, it would be thought rather hard to believe. I have had injured cables for months like that under water, and without the slightest loss of insulation at all. The operation is that the oil closes the wound and is not washed away. Here I have a gutta-percha cable with a groove cut in it 3 inches long, and a gap one-

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eighth of an inch wide. There is dead earth there. I am going to try to restore that. I will give it a little rosin oil as a remedy. I will plunge this wire into a phial of rosin oil, and on again testing, the insulation is found to be perfectly restored. I will leave that on for the whole evening, and you will see it still good. There is no doubt that the insulation is most perfect with small battery powers. But the question arises, Is it as good with high battery powers? And for that reason I addressed myself to Mr. Wimshurst, who most kindly prepared some experiments which I will now show. We shall have high potentials—about 100,000 volts—and you will see the comparison.

Mr. Wimshurst.

Mr. WIMSHURST: This piece of apparatus is very simple. It consists of a wooden base from which rises an upright carrying a scale. This is a rocking beam, and upon it are two glass arms projecting about 5 inches. The arms are situated at a radial distance of 3 inches from the centre. Fixed to the rocking beam is a pointer to reach the scale. The pointer is 2 ft. 6 in. in length from the centre; thus we have a multiple of 10. I have made the scale and its figures to suit the lengths of spark which may be safely obtained with the small influence machine now in use. When the rocking beam is placed horizontally—these pendant balls are attached to the ends of the projecting glass arms—the one ball is left free to spark through air, while the second ball hangs within a vessel containing oil. The containing vessel is made with glass sides and a suitably shaped metal bottom. Now, while the index pointer is vertical, the scale shows the sparking gap to be 1 inch, both through the air and through the oil; but when the pointer is moved out of the vertical, the spark on the one side is lengthened and on the other is shortened, the altered lengths of both sparks being shown by the pointer and the scale. Placed as it now is the sparking gap through oil would be 0·05, and in the air 1·95. We shall, of course, while using oil, work down at this end of the scale. I will first show the relative value of castor oil, but it is not a very good specimen. The connections are for the ordinary spark, and you see the pointer shows 0·25 as the length of the spark in the oil, and 1·75 as the spark through air. When the connections are made in

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the customary manner, so that the impulsive spark is in use, then a much higher value is taken by the oil, and it will be seen that this sample of poor oil shows a reading of 0·1 in oil, and 1·9 in air. Mr. Wimshurst.

Professor HUGHES: We have two systems of communication to-night. The first one is the ordinary spark—the ordinary arrangement of sparking through from the top of a Leyden jar. Then we have another arrangement—theoretically the impulsive spark—in which the conductors are connected with the two Leyden jars, and from the lower portion of the Leyden jar connected between the air and the oil. That, according to Dr. Lodge, is very much more rapid, giving a rapid oscillatory current of some 1,000,000 per second. You see with the impulsive spark the oil is much superior to what it is with the ordinary spark. The higher the oscillation the greater the value of the oil. We shall commence with the ordinary spark. Professor Hughes.

Professor PERRY: The only difference is, you connect one with the inside of the Leyden jar, and the other with the outside?

Professor HUGHES: The first is the ordinary way. The other spark is from the outer coating of one jar to the outer coating of the other.

Mr. WIMSHURST: The reading here is 0·15, as against 1·85.

Professor HUGHES: With the impulsive spark—the more rapid spark—you see the distance is less; it is about 40 or 50 times different in favour of oil. Unfortunately, we have no rosin oil. We had some, but in making the experiments we broke the glass and lost the oil. It seems to make no difference, for what I considered very poor oils in my old tests show remarkable results. The tests I made showed that rosin oil was very far superior to any vegetable or other oils. But it seems that with high currents there is little difference. I think it is very hopeful for oil as an insulation that the higher the voltage the better it seems to be. This seems to me important, and gives to oil insulation a very great future. I must express my grateful thanks to Mr. Wimshurst for his experiments to-night, and also to the audience for listening to me.

The CHAIRMAN: After your expression of appreciation, I think it is really only a matter of form to call on you to accord to Mr. Siemens.

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Mr. Siemens. Professor Hughes a hearty vote of thanks. His paper was all the more interesting because over 30 years ago he had already recognised the value of oil, and it is not at all improbable, as he said in his remarks, that it may yet occupy a very important position as an insulating material. I think, also, our thanks are due to Mr. Wimshurst for the kind way in which he has undertaken the great trouble of bringing here and adjusting his apparatus, which illustrates in such a very striking manner the difference between the various insulating materials. I beg to put it to the meeting that our best thanks be accorded to Professor Hughes and Mr. Wimshurst.

This proposition having been carried by acclamation,

Professor HUGHES: I should like to have added to my lecture that Mr. Latimer Clark is the only living person who saw my experiments 35 years ago.

Mr. PREECE: I beg to claim to be an exception. I certainly saw the experiments.

Professor HUGHES: I am very pleased to hear it.

Mr. Alexander Siemens here vacated the chair, which was then taken by Professor W. Grylls Adams, F.R.S., Past-President.

The CHAIRMAN: We have now to proceed with the discussion on Mr. Siemens's paper, which was adjourned at the last meeting.

Mr. Housman. Mr. HOUSMAN: I think that the results plotted on Diagram II. are of special interest and importance, as they are obtained by a new and distinct method; but there are peculiarities about them which deserve special attention. It would seem, from a casual inspection of the diagram, that the hysteresis increases tolerably uniformly from static hysteresis, as shown in Curve 6., up to 100 periods per second, as shown in Curve 4. But it must be noticed that these are losses per second, and not losses per cycle. If the losses are calculated per cycle, it will be seen that those at 66 periods per second, instead of being about 25 per cent. less than those at 100, are about 25 per cent. greater. Curve 6. is evidently obtained by multiplying Professor Ewing's measurements of energy dissipated per cycle by 100 cycles per second, so that it is directly comparable with Curve 4. It will be noticed that the

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losses on Curve 4 are about double as great as those calculated from Professor Ewing's results; while the losses per cycle at 66 alternations per second are about 2½ times as great as Professor Ewing's figures. The exact ratio, however, seems to vary considerably with the induction density. The actual figures at 5,000 lines per square centimetre are: Loss per cycle statically, about 1·125 joules; at 100 alternations per second, about 2·25 joules; the loss at 66 alternations greater still, being about 2·7 joules. This is a result which hardly agrees with previous determinations. I think Mr. Swinburne, in experiments made some time ago, found the losses per cycle at about the same frequencies to be from 3 to 25 per cent. greater than measured statically, and I do not remember that he noticed any great variation with the variation of frequency. My own experiments have been made with much lower alternations and higher intensity—from 10,000 to 20,000 lines per square centimetre, and from 3 to 30 alternations per second. The losses measured are from 10 to 20 per cent. greater than Ewing's figures, and do not show any noticeable variation between the limits of frequency within which the experiments were made. It would seem as if there were some special disturbing influence due to the peculiar form of transformer used. The wires constituting the transformer are of comparatively large diameter, and the calculated Foucault current losses constitute a rather large proportion—about 30 per cent. of the whole. In the experiments made by Mr. Swinburne the calculated Foucault current losses constituted an extremely small percentage. The question therefore arises whether the discrepancies noticed may not be due to the Foucault currents disturbing the uniformity of distribution of the induction by screening the interior of each wire, so that we have the intensity on the outsides much higher than the average, and on the insides much lower. On this account it may be predicted that the apparent hysteresis loss will be found to vary with the size of wire used, and that it is only with very thin wires that reliable data will be obtained.

Mr. Housman.

Mr. Stuart Russell: The point I would like to refer to in connection with Mr. Siemens's paper is the sparking distance in air, curves of which are shown on Diagram VI. as there is a striking

Mr. Russell.

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Mr. Russell.

difference between these results and those obtained from experiments made by me some time ago. Our tests were made more especially because we wished to find out whether the spark gauge was a reliable instrument to use for approximate measurements of the E.M.F. given by an induction coil, and we therefore made a series of experiments with various electrodes. The spark gauge consisted of two metal brackets supported on ebonite pillars, one being arranged to take a sphere, cylinder, point, or disc as the fixed electrode, and the other carrying a micrometer screw graduated in thousandths of an inch, and arranged to take either a point or a sphere. The first point particularly noticeable is the fact that Mr. Siemens has obtained very nearly the same striking distance when using a point and disc as when using two discs, or a disc and sphere. I have put up some curves showing the results we obtained, the maximum striking distance in each case being plotted, and it will be noticed that two curves are very much above the other pair. The two top curves are with a point and disc and a point and cylinder, the two lower ones with a sphere and cylinder and two spheres. I have handed in to the Secretary a table of results, which, however, I need not read, as it will be seen from the diagram that at about 10,000 volts, when the point is used as one electrode, the striking distance is, say, three times the striking distance between two spheres, or a sphere and cylinder. This difference, though considerable, is not so great as that obtained by Mr. Warren de la Rue, who with 7,000 volts obtained between a point and disc about four times the distance that he got between two spheres, and at 2,500 volts about twice the distance.

Since the reading of Mr. Siemens's paper, I have repeated some of these tests, to see if I could possibly get such short striking distances as he obtained, which, as you see, below 5,000 volts are actually less when the point is used than between two discs, or a disc and sphere, and only slightly greater when higher voltage is used. Although I have tried all sorts of conditions, I have been utterly unable to get such short striking distances. With regard to the reliability of the spark gauge, Mr. Siemens found that he was able to repeat his tests with very small degrees of

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STRIKING DISTANCE, IN INCHES, IN AIR WITH VARIOUS ELECTRODES. ALTERNATING CURRENT OF ABOUT 100 ∿. RATIO OF MAXIMUM VOLTS TO MEASURED VOLTS = 1·54. Mr. Russell.

Volts (Mean).	Point 470° and Disc.	Point 470° and Cylinder ⅜″ dia.	Sphere 3″ rad. and Cylinder ⅜″ dia.	Two Spheres, each 3″ rad.
2,000	·052	·048	·028	·028
3,000	·104	·100	·044	.046
4,000	·181	·155	·060	·070
5,000	·277	·231	·079	·093
6,000	·346	·326	·103	·119
7,000	·410	·399	·126	·146
8,000	·477	·459	·153	·169
9,000	·550	·530	·181	·194
10,000	·650	·590	·207	·217

error, but we found large differences, especially with the point. With two spheres, or a sphere and cylinder, the results were fairly comparable, but with the point as one electrode the results varied considerably. Some of the tests were made without repolishing the electrodes, but others were made repolishing the electrodes before each test, and even then within a short space of time we got differences of between 5 and 10 per cent. in the striking distances. Another point, also, that Mr. Siemens alluded to was that the time the pressure was on before the spark passed had no effect on the striking distance. This, again, was certainly not the result obtained in our tests, as we found that if the electrodes were set at a rather greater distance than we had been able to get an instantaneous spark across, after two or three minutes the spark would pass. The results of many tests showed this, and I cannot help thinking that the result of leaving the pressure on for some time has a decided influence on striking distance; and I believe the results observed by Mr. Ferranti some time ago showed the same thing. One other point to which I wish to refer is the shape of the curves. There is a habit of

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Mr. Russell.

speaking about the striking pressure in air being so many volts per millimetre, as though it were a straight line curve, whereas all the curves I have seen are convex towards the base line on which the volts are marked; that is to say, at high voltages the pressure is very much less per unit striking distance than at low voltages. Taking the curve for a point and disc at 10,000 volts, the result is 15,000 volts per inch, or 600 per millimetre; and at 2,000 volts the figures are 38,000 volts per inch, or 1,500 per millimetre—an increase of about 150 per cent. It is, therefore, very misleading to talk about the breaking-down stress being equivalent to so many volts per millimetre, as though the ratio were a constant one.

Mr. Esson.

Mr. W. B. Esson: I would like to ask Mr. Siemens whether he would not considerably diminish the exciting current required for his cable transformer by bending round the two ends, and so connecting the iron parts together. I can quite understand that if you have a transformer like Mr. Swinburne's "hedgehog," where to complete the iron circuit it is required to place a large-sized piece of iron on each side of the coil, in that case you would increase the iron loss very considerably; but in the case of Mr. Siemens's transformer, by completing the magnetic circuit through iron you do not increase the iron loss in any way, and it seems to me that in this way the design might be improved somewhat. Although we have several curves given by Mr. Siemens, showing the losses at different degrees of induction, and so on, I do not see amongst them any curve giving the efficiency of the cable transformer at different loads—an extremely important point. On the diagram here I have endeavoured to represent the importance of this by taking the curve of a central station and working out the mean efficiency of different transformers for a 24-hour load. The dotted line gives the mid-winter load curve of the Kensington and Knightsbridge station to a scale of amperes marked on the right. This may be regarded as a typical curve for the lighting of a residential district, and I am assuming that such a district would be fed by a low-potential network from transformer sub-stations supplied with current through high-potential feeders. The loss

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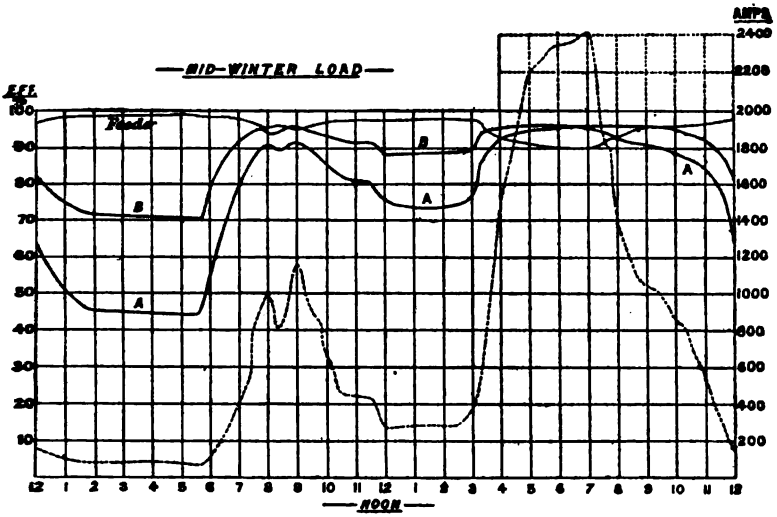
in a transformer may be taken, for rough purposes, as equal to $a + b\,l^2$, where a and b are constants, a depending on the

Mr. Esson.



iron loss, and b on the copper loss, and l being the load. On this basis I have calculated the loss for two transformers, one in which the copper to the iron loss at full load is as 1 to 3, and the other in which the copper to the iron loss is as 3 to 1. Both have at full load 95 per cent. efficiency, but in the first case the copper loss is only one-third of the iron loss at full load, whereas in the second case it is three times. The two curves, A and B, represent the efficiencies at different loads; the mean of the transformer with the large iron loss being 76·3 per cent. for the whole 24 hours, and the mean for the one with the large copper loss being 87·9 per cent. This, remember, is for the mid-winter load. The summer load I have not worked out, so cannot give the mean for a year, though it will be readily seen that with the diminished efficiency the advantage will be more on the side of the transformer with the greater copper loss. For the purpose of comparison, I have drawn a thinner line on the diagram, which represents the efficiency of a feeding main for direct-current distribution in which there is a loss of 10 per cent. at full load. The mean efficiency for the 24 hours is 96·2 per cent. You will see from these results that it is very important to know the

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Mr. Eason. efficiency of transformers at light loads, and the curves give some idea of the kind of result we get by varying the iron and copper loss while keeping the sum of the losses the same at full load. Of course, in transformer sub-stations there ought to be more than one transformer, and in the case of two it works out that the best result would be obtained for this load curve if their outputs were as 4 to 1. Then a three-wire cable can be employed —one middle core, and two outside concentric wires. The middle wire is connected to both transformers, and the concentric wires, one to the small and one to the large transformer. There is one other question I would like to ask Mr. Siemens: What was the impregnating material used for the calico mentioned on the last page of his paper?

Mr. Siemens. Mr. SIEMENS: The principal point I would impress upon you is, that the title of my paper is "Some Experiments with Alter-"nating Currents." It was not a treatise on transformers, nor a treatise on sparking distances; but I wanted simply to put on record certain facts. In the discussion which took place at the last meeting, I was very much struck with the way in which doubt was expressed as to the accuracy of the experiments. Anybody who has had anything to do with cable manufacture knows perfectly well that with the usual instruments there is no difficulty whatever in reading the resistance to the fourth decimal of an ohm.

Then Mr. Evershed thought that measuring temperature by electrical resistance was not a very safe method, and he seemed to prefer the method Mr. Swinburne mentioned of placing the thermometer into the bristles of a "hedgehog;" but I would remind you that the electrical pyrometer devised by Sir William Siemens was on exactly the same principle upon which these temperature measurements were carried out. I really do not know of any method which could be more accurate than measuring the increase of resistance of a wire, whether copper or iron or platinum, to determine the temperature of that wire. To apply this particular method to measuring the rise in temperature in a transformer has been, I think, first suggested by Dr. Baur, as I mentioned in the paper. I think I expressed very clearly in my

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paper that the results which refer to transformers were all arrived at when that particular form of transformer which I call "cable "transformer" is used. That reminds me I received a letter from Mr. Uppenborn, of Berlin, who informs me that he had applied for a patent some time in 1881 suggesting a cable transformer; but from his description it appears that he wanted to make it by having a primary conductor surrounded by a number of parallel secondary conductors, and then the whole cable was to be wound round with iron wire in the direction of the lines of force. That is practically the transformer mentioned in my paper as being suggested by Dr. Werner Siemens, which did not act, and the essential difference of this new transformer is that the iron is inside. Mr. Siemens.

I am very sorry to hear from Mr. Russell that his results differ so much from mine. We have been exceedingly careful in making the experiments, and the electrodes which we used with the sparking micrometer we have polished up after every experiment, and our experiments were in very good accordance with each other; I think I mentioned in the paper that there was only 1 per cent. difference. I believe you can attach great value to the distances given in the paper. That the point did not show a great difference as against the sphere and disc was rather surprising, but every care was taken to obtain accurate results. With regard to the influence of time, I specifically said time has no influence on sparking over in air; Mr. Russell specifically mentioned that time has influence in the case of cables and other materials. There lies the difference.

With regard to Mr. Esson's remark, it is really answered in the paper, which states that the induction along the whole of the cable transformer is very uniform—that it only falls off a very little at the ends. Of course that falling off would be avoided if the ends were united. But I do not think the difference is worth having. It would probably give more trouble to unite the iron in a thoroughly magnetic way, if I may say so, than to leave it as it is. I particularly did not say anything about efficiency of transformers, so as to avoid discussion on that. I simply wanted to bring before you some experiments on alternating currents, but by no means exhaust the subject.

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Mr. Siemens.

The canvas about which Mr. Esson inquires was impregnated with india-rubber solution.

I think those were all the remarks that called for any answer.

Professor Grylls Adams.

The CHAIRMAN: I am sorry personally to be in the position of having been absent from the meeting at which Mr. Siemens's paper was read, and that I have not therefore been able to properly fill the chair on this occasion and make any remarks with regard to it. I am, however, very glad to have heard part of the discussion upon it. I now call upon you to accord your thanks to Mr. Siemens for his valuable paper.

The motion was carried unanimously.

Mr. SWINBURNE: I suggest that the discussion on Professor Hughes's paper be adjourned; the subject is a most important one, and I should think a great many members would like to speak upon it.

The proposal was agreed to.

The CHAIRMAN announced as the result of the ballot that the following candidates had been duly elected:—

Associates:

Joseph Cecil Bull.
George Johnson.
William Alexander Wilson.

Students:

William A. Brodie.
J. Hermann Field.
Gerald Douglas Gibson.
Harold George Rea.

The meeting then adjourned.

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The Two Hundred and Thirty-fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 17th, 1892—Prof. W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 10th, 1892, were read and confirmed.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Stanley Bone. | William James Davy.

Donations to the Library were announced as having been received from Mr. A. R. Bennett and Mr. Killingworth Hedges, Members; to whom the thanks of the meeting were duly accorded.

Mr. Fairfax and Mr. Blackwell were appointed scrutineers of the ballot for new members.

The PRESIDENT: We will now resume the discussion on Professor Hughes's paper, "On Oil as an Insulator."

Mr. A. A. CAMPBELL SWINTON: I would like to give particulars of some experiments I made about 16 months ago of a similar character to those dealt with in the paper, but in which rather different results were obtained to those of Professor Hughes. The oil used was resin oil of the kind employed in Mr. Swinton

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rinton.

Brooks's system, and the current was obtained from an induction coil instead of from a Wimshurst machine.

Spark in Air. In.	Spark in Resin Oil. In.	Ratio.
1	0·1	0·1
1·125	0·2	0·1777
1·5	0·3	0·2
1·5	0·4	0·2666
1·75	0·5	0·2857
2·75	1	0·3636
5·125	1·5	0·2926
7·875	2	0·2539

The results, as will be seen, were exceedingly different from those of Professor Hughes. The best result is the top one on the list, in which the oil had a resistance to sparking of 10 times that of air. As will be observed, another result came out with a spark in oil of 1 inch, making the resistance of oil a little less than three times that of air. The experiments were carried out in exactly the same manner as those made by Professor Hughes, the only difference being in the use of the induction coil and the absence of any Leyden jar. The induction coil was capable of giving 12-inch sparks in air, but we were unable to go higher than 7·8 inches, because at this point the sparks came out through the glass and destroyed the apparatus. It is very possible that the oil used was not so anhydrous as that employed by Professor Hughes. I do not know whether his oil was specially treated to remove the water or not, but I would like to point out that oil thus specially prepared, however useful for certain purposes, when enclosed so that moisture cannot get to it, is bound to absorb moisture from the air if it has a chance. Therefore oil in its natural condition is more permanent than oil which has been specially anhydrated. I think that, as a matter of fact, the differences between my results and those of Professor Hughes are probably due rather to the use of the induction coil than to differences in the quality of the oil itself, and of course the current produced by the induction coil is more nearly comparable to that employed in ordinary electric lighting with high-tension alter-

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nating apparatus than is the current obtained from a Wimshurst machine. I also experimented with ordinary commercial paraffin oil, which was found to give about one-half the resistance to sparking obtained with the resin oil. I should mention that with every discharge through oil there is a very distinct amount of carbon formed. I did not change the oil after each experiment, and it is just possible that the carbon formed in the oil may have had some effect in making the sparking more easy in the subsequent experiments. The best insulation that I obtained with oil was 10 times that of air, whereas Professor Hughes apparently got 79 times. I think it would be very interesting if Professor Hughes would try his experiments over again with the different classes of oil that he used, using an induction coil: I think he would obtain very different results from those that he got with the Wimshurst machine. Mr. Swinton

Mr. W. C. JOHNSON: The last speaker has given you some practical information, but before I venture to contribute any remarks under that head I should like, with your permission, to revert to history. I know that the history of inventions is not a very palatable one to practical men, but I feel very strongly that it is my duty on this occasion to vindicate the rights of the late Mr. David Brooks, whose representative I have been in England. I may perhaps be allowed to digress so far as to refer to what I consider parallel history. In 1876, my firm, Messrs. Johnson & Phillips, patented the oil insulator for overhead wires which is now well known. At first but little interest was shown in the invention. In fact, it was derided on all sides, and Messrs. Johnson & Phillips found it impossible, as a firm, to get it taken up in England, after experimenting at great expense for some years. But the India Office took it up, and they soon recognised the value of the system, working lines, especially those near the sea coast, at all times, whereas previously they could not work these lines with any degree of reliability except under very favourable atmospheric conditions. Well, just at the time that this invention was likely to become a commercial success, we were naturally very easily frightened by people who were able to show that other inventors had been on the same track. An Mr. Johnson.

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Ir. ohnson.

American patent specification was put in our hands which at the time, and for a moment or two after reading some parts of it, seemed to threaten to upset our patent altogether. It specified, for instance, that paraffin should be poured into an annular space around the insulator, and this apparently disclosed the fact somebody else had been before us in the field as inventors of a *fluid* insulator. But, after reading on to the next page, it became evident that this was solid paraffin that was proposed, for it spoke of its surface as capable of being wiped clean. Perhaps I have dwelt too long on the subject of this invention of Mr. Phillips, but it may be regarded as a parallel case to the one under discussion. I think that after hearing Professor Hughes's paper, and after reading the extracts from his specification of 1859, which he quotes in his paper, anyone might well come to the conclusion that David Brooks had been anticipated. But it is quite obvious that Professor Hughes's description of his invention, if you will read his blue-book, is that the outer covering of the core, which he quite correctly describes as a tube, is in all cases an *insulating* tube. You cannot, by reading the complete specification, or by referring to the lecture which Professor Hughes gave before the Society of Arts, come to any other conclusion. For example, how could an oily coating, as applied by Hughes—between the layers of a gutta-percha core—be of any service in healing up a leak in the iron pipe of a Brooks main? and this sealing up of flaws is the essence and sole object of his invention. Professor Hughes's patent No. 84 (dated Jan. 11th, 1859) was tried and experimented with at the Gutta-Percha Works. Mr. Willoughby Smith—who, alas! is no longer able to give us interesting records of those experiments—found it to be of no practical value. The idea of closing up gutta-percha faults by such an expedient was an idea which did not go down. People said there should be no faults. Manufacturers objected to having gutta-percha coverings healed when they did not require healing. In fact, objection was at that time taken even to tarring the yarn serving of a cable, because the tar reached the surface of the gutta-percha core and closed up incipient faults. For that reason it was not allowable, because it was likely, as

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with Professor Hughes's invention, to close up incipient faults. I consider that the use of Chatterton compound, which came into use about that time, was much more like an infringement of Hughes's patent than that of David Brooks. Mr. Johnson

Professor HUGHES: I think Chatterton was a little before me, and I must have infringed him, then.

Mr. JOHNSON: I will not tell you any more history, but I would like to tell you what Messrs. Johnson & Phillips have done in the way of laying down lines. The business done in that direction has been small, because there has been a great want of faith in it; but now that such men as Mr. Nikola Tesla, Professor Crookes, Mr. Preece, and Professor Forbes have all spoken within the last month or two of the merits of oil insulation, there is naturally great interest shown, and a great number of inquiries are made as to what can be done. I am not going to deal with the electrical part of the matter, because I am not sufficiently armed on this occasion to give you any details. But my firm have, in the nine or ten lines which they have constructed, aimed at making the lines practically commercially efficient, rather than at obtaining any high insulation; anything over 100 megohms being practically good enough for mains for electric lighting and power transmission. There are many here who have been at the works at Charlton, and who have seen the Brooks line which was laid there, and which has been tested for many hours continuously at a pressure of 15,000 volts, and that alone speaks very well for the system. The great unreliability of the different samples of oil has been a matter of real difficulty, and Professor Hughes is evidently well aware of that fact, from what he has said. It is a very easy matter to test a small quantity of oil; it can be done in the laboratory with great ease. For testing such large quantities of oil as have to be dealt with, however—I think over a thousand tons have passed through the Charlton works during the last two years—I have devised a method, which is now only in an experimental stage, by which I hope to electrically test oils in bulk, so as to readily determine the uniformity of their insulating qualities. I shall drive the oil at great pressure between two metal surfaces, which, except for the intervening oil, are

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Mr. Johnson. otherwise perfectly insulated from each other. To test the insulating properties of the oil, as it flows between these plates a current of high E.M.F. is supplied by a dynamo. It is so arranged that a galvanometer in the circuit will indicate any variation in the conductivity of the oil as it passes between these plates. It is very unfortunate that Professor Hughes did not press his claims earlier, as now Willoughby Smith and David Brooks are necessarily absent; but at the same time I need hardly say I congratulate Professor Hughes and admire his original investigations in this matter. He is, as an inventor, a giant in this country, we all know, and therefore I need not detain you by paying any particular tribute to his genius on that score. The work carried out by Brooks was well known by Mr. Preece, and it can hardly have escaped his attention that Brooks, whom he warmly congratulated upon his invention, was trenching upon the ground claimed by his friend Professor Hughes. Mr. Brooks has upset a patent of mine. I patented putting transformers in oil; but after I had done so, I found that David Brooks had anticipated me by putting induction and similar coils for electrical purposes in oil, so I had to give up that patent. I have nothing more to say, but to repeat that it is obvious, on reading the patent specifications, that the Brooks system was not in Professor Hughes's mind when he patented those ingenious devices to be used in submarine telegraphy, and I am sure that Professor Hughes can well allow David Brooks his claim on this subject in full.

Mr. Rogers. Mr. F. M. ROGERS: I should like to ask if an analysis was made of the various forms of oil submitted to test, because I suppose there are few commercial products which vary in composition more than oils. Castor oil, for instance, is often a very impure preparation. Resin oil, again, which is largely employed upon railways and for machinery, deposits its resin, as a gummy substance, upon the axles and other parts of running machinery, as users find to their cost. Now it has occurred to me that, unless care be taken to ensure some standard of purity, the resin would be deposited as a film upon the conductor within the pipe; and since I imagine it to be the chief insulating property, it may

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be that the residue would not give much protection if the film were perforated under the high pressures now employed. Some common vegetable oils, such as castor, walnut, and others, are at times produced in quantities much in excess of demand in Northern India and elsewhere, and consequently can be bought at low rates; and since I believe that paraffin will readily dissolve in some of these fatty oils, it seems to me that the crude residuals from the distillation of paraffin, which are of little value, could, after the removal of injurious impurities, be mixed with some one or other of these, and thereby form a cheap non-conducting fluid of high quality. I have at the present time a sample of one of these residuals which came from South Russia; it is a black, semi-fluid substance, that looks promising in insulating properties, and I shall be very pleased to send some of it to Professor Hughes, if he would like to test it. Mr. Rogers.

Mr. W. GRAY: I have listened with great pleasure to Professor Hughes's paper, which I think gives a great deal of valuable information on the work done by electricians of the older school; and as these experiments were originally carried out many years ago, I would like to congratulate him on the complete way in which he has been able to put his results before the Society. I do not propose in any way to discuss the paper, but perhaps I may be allowed to quote a few facts which may interest the meeting, as looking at the question from a different point of view. I will not enter into the matter as to whether Professor Hughes's patent was original or not, or whether it covered the present practice. Many experiments have been made—notably some by Rousseau, in 1826 I think—on the insulating qualities of oil. Professor Hughes no doubt knows of these, but I am not aware that anything practical ever came of them. Mr. Gray.

In testing various oils we found the insulation resistances obtained were very uneven—due, no doubt, to uneven quality of the oils. For example, taking 10^{20} C.G.S. units as unity, rape oil was found to vary between 0·5 and 5; linseed oil, from 0·28 to 0·75; lard oil, from 2·2 to 5; colza oil, 1 to 15; cottonseed oil gave an insulation resistance of about 50 units. A mixture of tulip and linseed oils gave a resistance of only from 0·3 to 1;

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Mr Gray. another mixture of nut oils, from 0·4 to 1; thus showing that, allowing for a variation in oil itself, there was a still greater variation in the different classes of oil, linseed oil giving only 0·28, whereas cottonseed oil gives as much as 53. It is therefore obvious that at the present day, when referring to oil insulation, the oil itself should be specified. After testing for insulation resistance, these oils were put into glass tubes, and a number of experiments conducted on the sparking distances through them.

The current used was obtained from an alternating-current transformer at 10,000 volts potential, and not from an induction coil or a Wimshurst machine, and to this is no doubt, in some degree, due the different results obtained by Professor Hughes and ourselves. The conductors terminated in two spheres, and it was found that with rape and linseed oils the sparking distances were only a little less than for air. I was therefore rather surprised to note that Professor Hughes found this to be only 1-79th of the sparking distance in air: under ordinary conditions this latter figure is no doubt excessive. After the oils had been sparked through, they were again tested for insulation resistance, and were found in many cases to give only slightly less insulation. This result appeared strange, since, especially with the heavier oils—which were tested without disturbing or mixing them—heavy carbon deposits were found when sparking through with the 10,000 volts, and it was observed after the formation of these deposits, the circuit being broken, that on re-making the circuit with a much lower difference of potential at the spheres (which were kept the same distance apart) the oil sparked through, in one case with a heavy oil, at as low a potential as 2,000 volts, showed that the carbonaceous deposit had certainly greatly affected the sparking distance, without apparently much affecting the insulation resistance.

Mr. Bruce Warren has kindly given me the results of some tests upon oils made by him in 1867 with a battery E.M.F. of about 150 volts (100 cells); on re-testing the same oils in 1889 (22 years later), the insulation resistance was higher when tested with a battery of 25 cells, say 38 volts; but when the battery was increased to 100 cells (150 volts), as originally used, the insulation practically broke down, although during the whole of this time

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the oils had been kept in glass bottles, and not in contact with metal, where they might become decomposed. During the sparking distance experiments it appeared that in the case of thick oils, such as resin, the gases formed by this sparking cannot readily get away, and for this reason the sparking distance becomes distinctly reduced; whereas with the thinner oils and spirits the gases escape more easily, but the oils were destroyed in much greater degree when the heavy discharges took place through them. Mr. Gray.

Mr. FREDERICK H. VARLEY: Two causes induce me to speak. The first is, that I had the distinguished honour to see Professor Hughes's experiments at the Society of Arts in 1859; and the second, I have also made some experiments with oil as an insulator. Professor Hughes last evening employed the multiple-arc, or differential, method: he took sparks in air and balanced them against sparks in oil. My method differed from Professor Hughes's, inasmuch as I employed a series test: the current had to spark through oil and through air simultaneously, the length of spark in air determining the insulating or conducting properties of the oil. The sparking points for oil were made to face each other horizontally by bending the ends of the wires at right angles, and the points adjusted by means of a graduated wedge to 1-20th of an inch apart; one point was connected with the terminal of the induction coil, the other to the spark-receiving terminal of the air discharge. By this means I was able to take sparks entirely through air, or by raising a cup containing oil to immerse the adjusted points at will. I found that rape-seed oil, linseed oil, and cocoanut oil were less resisting than air. Olive oil was about the same as air. With the three first oils the sparking distance in air was pretty constant at a little over 1½ inches. In the case of paraffin oil and rosin oil the distance between the sparking loop and receiving terminal had to be brought down three-eighths of an inch before the discharge would spark through the insulating oils. I would mention that the sparking point in air was made by a loop of fine copper wire, coated with cotton and moistened with alcohol, attached to the adjustable sparking rod of the induction coil, the receiving terminal Mr. Varley.

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Mr. Varley. being a flat brass plate; the discharge from the induction coil being controlled by putting on resistance between the battery and primary until regular sparks from 1½ to 2 inches were obtained. I think this method of series testing, if I may use the term, as against the multiple-arc system, affords a more ready means of picking out oils which are better conductors than air from oils which are distinctly insulators.

My experiments were made in 1869 or 1870—about 10 years after Professor Hughes's memorable experiment. I was trying to make static condensers, the idea being that if you had a fluid insulator you could adjust the extent of the tinfoil surfaces by cutting out or adding to the inductive surfaces until an exact standard microfarad condenser was obtained. With paraffin oil as an insulator a very curious thing happened, with a current of 400 insulated Daniell cells. Three sheets of bank post paper were pierced and the foil carried through the hole, making metallic contact, and so the condenser became perfectly useless. A precisely similar set was tried with a charge of 100 volts, which stood very well for a few days. In all these cases the paper was carefully dried and soaked in paraffin oil which had previously been filtered through unslacked lime, which had been heated to drive out any moisture. I found the oil so dry that, on passing a strip of paper soaked in it between the fingers, exerting a slight rubbing friction, it readily became excited and acquired the electric state. The condenser under the charge of 100 volts gradually increased in capacity, then became unreliable, and finally failed to hold any charge at all. The cause was not far to seek. I found that paper is much more hygroscopic than paraffin oil, and that moisture still retained in the oil was carried into the paper by repeated charging and discharging of the condenser, until the paper became actually conducting from the amount of moisture it acquired. This, naturally, frustrated the purpose intended. When they began to fail, they first held a charge for a few minutes, and finally would not hold it for a second. I mention these experiences as they may be of use in reference to the employment of oil as an insulator, and because I think if the series method is used you get a little more information than from the differential method.

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Dr. FLEMING: A previous speaker this evening has alluded to early experiments of putting transformers into oil as a means of insulating them. Some time ago, when a good many patents were being taken out for this purpose, I had the curiosity to look up early literature on this subject. In 1851, Ruhmkorff was engaged in developing his induction coil, and he found great difficulty in securing permanent insulation. He used paper covered with shellac varnish. Du Moncel, in his "Applications "of Electricity," says that M. Jean, an amateur electrician in Paris, in 1854, made a suggestion of simply winding the induction coil with cotton-covered copper wire and plunging the whole coil under the surface of an insulating fluid. M. Jean employed oil of turpentine as his fluid insulation. He first dried the coil very carefully in vacuum, plunged it underneath the surface of oil of turpentine, and found that he was thus able to secure very high insulation; in fact, he produced a coil which gave sparks of 30 centimetres, or a foot in length, at a time when Ruhmkorff had failed to get anything like that length of spark, and Du Moncel therefore claimed for his countryman, M. Jean, the priority in this matter. I find, however, that at or about the same time a suggestion of the same kind was made by Poggendorff, of Berlin. The work of M. Jean will be found in the *Comptes Rendus* for 1858. Poggendorff's experiments are to be found described in the *Philosophical Magazine* for July, 1856, in a very long paper, in which he investigates the Ruhmkorff coil, and the difficulties encountered in trying to obtain a great length of spark, and he points out the difficulties of insulating by means of solid insulators. Therefore he suggested a *fluid insulator* for the coil, because such an insulator would be incapable of being permanently pierced. Instead of shellac dissolved in alcohol, and paper, he suggests a fluid insulator such as molten spermaceti or paraffin wax, or, better still, a fluid insulator such as vegetable oil or oil of turpentine, so that permanent rupture of insulation could not occur. Dr. Fleming.

A previous speaker has spoken of the difficulty of making condensers with paper soaked in oil, and these early experimenters likewise found the same difficulty, and knew perfectly well that

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Dr. Fleming.

in order to get high insulation by using paper or cotton saturated with oil, it was necessary to first carefully dry the fibre. It is not sufficient merely to dry it at a low temperature; it must be dried in a high temperature, or, better still, in vacuum—the object being to thoroughly remove the moisture from the tube, or fibre, of the material, so that when plunged in oil it becomes a thoroughly insulating material. One query suggested itself to my mind after reading the paper—whether we have really got to the bottom of this oil insulation. It may be that we have not yet found the best method of using oils. We know perfectly well that alloys have much higher specific resistance than pure metals. Is it possible that it is so with oils? May you not, by selecting by trial certain fluid insulators and mixing them together on some such similar principle, obtain a fluid insulator having a much higher specific resistance than any pure oils? Then, in using such a method of insulating transformers or induction coils, it seems essential that the cotton covering or paper, as the material wrapping round the wire, should be immersed in a tolerably fluid oil. My own experiments have shown me that in using a very stiff oil, or resinous oil, the oil does not penetrate into the paper. The best way is to use a fluid oil, carefully desiccated by chemical means, and afterwards the insulated coil may be removed with advantage into a heavier oil. In that way you obtain a less tendency to break down than if cotton were immersed in thick oil. The subject is by no means exhausted, and doubtless the paper of Professor Hughes will stimulate to further research.

Professor PERRY: I should like to ask Dr. Fleming if he is satisfied that M. Jean or Poggendorff did not merely dip their coil in oil and pull it out again.

Dr. FLEMING: Both evidently kept it in the oil. Poggendorff expressly says so; so also does Du Moncel in describing Jean's experiments.

Mr. Donovan.

Mr. H. C. DONOVAN: It was Mr. S. E. Phillips, jun., who first drew his attention to the use of oil as an insulator, as far back as 1873. That gentleman intended then to use paraffin oil as a means of insulating the surface of aerial insulators for land lines; afterwards resin oil was used.

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I call to memory an experimental line which Messrs. Johnson & Phillips erected at their works, which contained as many oil insulators as would be used in 6 miles of land line, which gave an insulation resistance equal to 15,000 megohms per mile. Mr. Donovan.

The late Mr. Schwendler used these oil insulators to great advantage in some districts in India. Considering the difficulties experienced in England in maintaining good insulation on land lines, it would be interesting to hear an authoritative statement as to how far oil insulators were used by the Government.

He knew no one who had experimented more with oil as an insulator than his friend Mr. Phillips, who had obtained the most important data from exhaustive electrical tests of almost every description of oil at various temperatures.

Paraffin, in combination with other oils, might be used to advantage for insulating purposes.

Mr. H. CUTHBERT HALL: About two years and a half ago, when the late David Brooks was in England, I had the opportunity of making some rather extensive experiments with liquid insulation, and in the course of the investigations I noticed a few peculiarities of oil as an insulator, which may possibly be of interest to this meeting. One of the most striking of these is the extremely large temperature coefficient of oil, and more particularly of resin oil. I began a series of experiments a short time ago with a view to establish the temperature coefficient of resin oil between 13° and 24° C., but, unfortunately, the severing of my connection with Messrs. Johnson & Phillips, at whose works I was carrying on the experiments, prevented me from completing the investigations. I may say, however, that between 16° and 18° C., with a mixture of half resin, and half, what is known commercially as, first-run oil, there is a variation of about 20 per cent. in the specific resistance of the oil. For higher temperatures the variation is more marked. I would suggest that this feature of resin oil becomes a very important consideration when oil is used for insulating transformers. Mr. Hall.

There is a sentence in Professor Hughes's paper, page 248, to which I should like to call attention. He says:

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Mr Hall. "The insulation of this form of cable"—that is, a cable covered with a fibrous substance—"was not quite as high as solid gutta-"percha, due to the fibre not being as good an insulator as the "enclosed oil." This is contrary to my experience in the matter, as I have found that, at any rate as regards jute-covered cable, before it is immersed in oil it is considerably higher than afterwards. I should like to hear what the experience of other experimenters has been on this point.

With regard to the preparation of the oil, the most important precaution to observe is that the oil should be very slowly raised to a high temperature. This drives off moisture, and the light volatile oils, which are invariably present in the resin oil of commerce, and are inferior to resin oil in insulating properties. I think the great discrepancy of results, brought out in the discussion, may possibly be due to the fact of the oils not having been carefully freed from moisture in this way.

In another part of his paper Professor Hughes speaks of the desirability of using a light oil for transformers, or condensers, or coils which have been so closely wound as to be penetrated with difficulty by thick oil. This difficulty, to which Dr. Fleming has also referred, may of course be entirely overcome by heating a thick mixture to a high temperature, and then pouring it on the coil. It soaks into the fibrous covering of the coils, and so forms a more efficient insulator than a very liquid oil. Another speaker—Mr. Gray, I think—has referred to the carbonising of resin oil after a spark has passed through it. In experimenting with light paraffin oil, I have noticed that once the spark is established the discharge continues; but, with a mixture of half resin and half resin oil, I have found that, even after a spark has once passed between the terminals immersed in the liquids, it is very difficult to maintain a continuous discharge. I find the best mixture, both as regards specific resistance, and resistance to disruptive discharge, to be three parts of solid resin and one part of first-run oil.

The President. The PRESIDENT: When you speak of the temperature coefficient of oil, do you really refer to the specific resistance—that is to say, the resistance for small differences of potential—or to

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the dielectric strength—that is, the resistance to cracking under high P.D.s—or to the surface insulation, which resists creeping of the electricity through the layer of dust and dirt on the surface? The President.

These three totally different kinds of resistance have, I fear, been rather mixed up in the discussion this evening, and it is very important to distinguish between them. For example, air has a very high specific resistance, but not much dielectric strength; glass has a large dielectric strength, but not nearly as high a specific resistance as air; and paraffin wax has a larger surface insulation than glass, but not as high a specific resistance as good glass.

Mr. H. CUTHBERT HALL: Perhaps I shall make my meaning clearer if I describe the way in which my tests were made. I used two metal plates, kept them a certain distance apart, and immersed them completely in the resin oil, the surface of the oil being covered with paraffin wax. I then observed the specific resistance of the oil at various temperatures, using a battery power of 500 volts. A thermometer was immersed in the oil, and the whole arrangement was kept at an equable temperature by surrounding the containing vessel with water. Mr. Hall.

Mr. GISBERT KAPP: I can bear out what Dr. Fleming has said about the necessity of getting the oil into the fibres covering the wires. Some two years ago, before I attempted to use a Brooks line on a large scale, I asked Mr. Phillips, of Messrs. Johnson & Phillips, whether he would demonstrate to me that the Brooks line was absolutely reliable, and I would not be satisfied with anything less than 10,000 volts. He said he would do it, and laid down a line to show that it was absolutely reliable at that pressure. I designed some transformers to raise the pressure of an ordinary alternator from 2,000 volts up to 10,000. When the experiment was ready we tried it, and I must confess that Mr. Phillips's line behaved much better than my transformer. The first thing that happened was that the transformer broke down. I was very much astonished, because, as it was in oil, I thought it could not break down. It had only been running two or three minutes when there appeared a curious red globe of fire Mr. Kapp

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Mr. Kapp. in the oil, then there was a puff of smoke, and the fuse in the primary went, and stopped the experiment. I took the transformer out of the oil to examine it, and found the fine wire had short-circuited. It was silk-covered wire, and the whole thing was immersed in oil, but it was quite impossible to keep it in order. As soon as the winding was repaired it broke down again after a few minutes. At last, after many trials and failures, I passed the fine wire through boiling paraffin before winding it on. This impregnated the fibre of the covering with paraffin, and after this treatment the transformer stood the test perfectly. The voltage was eventually raised to 17,000 volts, and the apparatus kept working for days at a time with perfect safety. The Brooks line also stood this test without any sign of weakness, and having thus satisfied myself of its reliability, I ventured to put down in some of my work Brooks lines, which, I am happy to say, have stood perfectly ever since.

Mr. Trotter. Mr. A. P. TROTTER: I would draw attention to a question which no other speaker has touched on as yet in the discussion. What is the weak point of oil insulation? Is it the oil, or the solid separator which is used to keep the conductor away from the tube? Hemp has been employed, I think, in the Brooks insulation. Mr. Varley has mentioned paper, which broke down through being hygroscopic. I think Professor Hughes used gutta-percha preferably, and we have heard also that jute, cotton, and silk have been used. We have had very little information yet as to the real resistance of the oil, and I understand that in the early Brooks system laid down between Clapham Junction and Vauxhall the insulation was rather low; and that seems to be due rather to the jute than to the oil, if oil is such a good insulator as we have been led to imagine.

Professor Forbes. Professor FORBES: I think during the whole of this discussion we have been so completely taken up with the important relative qualities of different kinds of oil as an insulator that we have neglected what seems to me to be one of the most important points in the whole of Professor Hughes's paper, viz., his remarks about the action of oil upon gutta-percha. In the early days of electric lighting we were inclined to try gutta-percha, which had

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been such an admirable insulator for telegraph cables. I think the reason that gutta-percha was given up altogether for electric light wires was probably the failure at Paddington, where the exceeding heat melted the whole of the wire insulation of gutta-percha into a solid mass; and I think the reason why it was given up altogether was the effect that heat has on gutta-percha. Now at the present time we have completely changed the conditions of electric lighting, and we never have our wires and conductors at such high temperatures as would be injurious to gutta-percha, and it is well worthy of consideration whether gutta-percha is not now a very valuable means of insulating our cables. These considerations have been floating in my mind, and, I daresay, in those of others, for some time past, but Professor Hughes's paper brings before us some facts which add far greater weight to them. That is to say, that gutta-percha, when it is treated in a suitable manner, is improved enormously in its insulating and mechanical qualities. Professor Hughes points out how, when the gutta-percha has had the moisture completely driven out of it and it is immersed in resin oil, the oil is taken up by the gutta-percha and prevents the admission of moisture afterwards, which would be injurious to its insulating properties; and he showed us a piece of such a cable which had thoroughly absorbed the oil and had remained in perfect condition for 33 years, and to everyone who handled that specimen I am sure it must have been apparent that it was a very satisfactory class of insulation indeed. I only wish to draw your attention to these points, which I think deserve more attention than they have received at present, and which I think will probably lead to some further experiments and more practical results in future. Professor Forbes.

Mr. E. T. CARTER: With regard to the difficulty in getting viscous oils into the inner layers of a finely wound coil, it occurs to me that complete impregnation could be effected by subjecting the oil to hydraulic pressure. If the coil were placed in a closed vessel containing the heavy oil, the oil being subjected to a considerable pressure for a short time, there is no doubt that the whole of it would become impregnated with oil. A few alternations of Mr. Carter.

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Mr. Carter. pressure would assist the operation, and would ensure the wetting of the whole coil. Mr. Kapp has described how the windings of a transformer may be insulated, the wire being served with paraffin oil before winding on. This method, though successfully insulating the coil, seems a little troublesome, and it would probably be easier to wind on the dry wire and afterwards to drive in the heavy oil under hydraulic pressure. This is more especially the case since we have been told that the paraffin oils will not mix with some of the heavy oils used for this purpose. There would be little or no capillary force to induce the heavy oil to replace or mix with the paraffin, and the insulation of the inner layers would be entirely due to the serving of the latter oil.

Mr. Preece. Mr. PREECE: Mr. Donovan raised the question just now as to why it was that oil had not been used in this country, and he appealed to some Post Office official present to report to the Institution how it was that that ill-used Department had not been occupying itself with the settlement of this question. Well, Sir, we are accused of a great many things: we do not deliver every letter that is sent; we do not transmit every telegram accurately. But I have never heard before that the Post Office has not endeavoured to the best of its ability to look into the merits of every invention, of every application of electricity, brought before it. The use of oil as an insulator is not an exception to this rule. I take this opportunity of correcting my friend Mr. Johnson's history by calling to his mind the fact that one of the first lines put up with oil insulators was on a Post Office line from Paddington to Uxbridge, along the canal. It was so long ago I forget the date, but I believe it was before any such line was put up in India. That line was put up with the greatest possible care. I walked over that part of the canal myself on many a dirty day. I took a deep personal interest in oil as an insulator, and regretted very much that the result of an experiment that continued certainly over two years was, as far as this country was concerned, a failure. The failure arose from the existence in this country of three abominable enemies to telegraphy—rain, wind, and insects. Now the first effect of these oil insulators on this Uxbridge Canal line was to find, to our surprise, that after not a very long time

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the rain performed the part of the cuckoo: it bowled out the oil, and instead of having a little cup filled with oil it was found filled with water, and the oil was bespattered upon the ground. That arose from the fact that the first oil used had a very light specific gravity—lighter than water. But doubtless the use of heavy oils would prevent that defect. The second failure arose from the presence of wind. I do not know why it is that the wind plays such pranks in the way it does in this country; it is driven in every direction, and it sooner or later inevitably blows out the oil in these cups. Then, again, there seems to be a kind of affinity between spiders and oil, for a remarkable effect of the presence of this oil was to apparently congregate upon these insulators all the spiders that existed in the United Kingdom. It was found after a short time that the spiders cast their webs and bridged across the cups and the iron stem of the insulator. The result was, the insulators soon lost their insulation. When I tell you that the tests made were conducted by one of our most experienced experimenters, Mr. H. R. Kempe, you will know that they were carried out with great rigour. Mr. Preece.

We were the first, also, to use oil as an insulator for underground conductors. No more genial soul ever crossed the Atlantic than Mr. David Brooks. No one was ever more practical and honest in his views; no one ever succeeded more admirably in impressing those about him with the merits of his inventions. The Post Office gave Mr. David Brooks a good solid order, which was paid for, to lay down between Vauxhall and Clapham Junction a line of iron pipes, carrying 30 wires, filled with oil. In answer to Mr. Trotter's question, in this particular instance I think I am right in saying the first portion of the line had the copper served with cotton, but the other part of the line was served with hemp; I do not think jute was used. The oil that was first used was a light petroleum oil with a specific gravity much less than that of water, and the pipes themselves were not very well made; the joints were not good, nor were the pipes, because we found afterwards one with a big crack in it. In fact, I should be sorry to tell you the number of barrels of oil used between Vauxhall and Clapham Junction. However, we persisted in this line: we

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Mr. Preece. removed every fault, every cracked tube was replaced, we made every joint good; and at last, when we found that water did, in spite of all our care, enter the pipes and oust the oil, we had from America some heavy oil, sent over by Mr. David Brooks; but, unfortunately, the oil sent over was so heavy that that we put into the tank at Vauxhall did not go much more than 200 or 300 feet, and even after some months did not find its way to Clapham Junction. This line existed for eight years. It would have existed now if the South Western Railway had not widened their line from Clapham Junction to Waterloo. Our lines were transferred to the roads and this line disappeared. However, it existed there for eight years, and worked well, although the oil was soaked up and disappeared. Every one of those 30 wires gave us a uniform insulation of something like a megohm a mile. Now in telegraphy, if we can only secure uniform insulation of a megohm a mile we are perfectly happy. But when we deal with open aerial lines, whether with oil or ordinary insulators, we have to deal with a variable resistance, which sometimes goes down so low as 100,000 ohms, and sometimes goes up so high as 100,000 megohms per mile.

Next as regards oil itself. Now I think we must draw a distinction, which was partially drawn by Professor Forbes, between the use of oil for cables, for underground wires, and for transformers. The use of oil for cables is essentially and especially Professor Hughes's invention, and the general use of oil, spite of what has been said, for insulating purposes is unquestionably his invention. The use of oil for submarine cables was brought out at a time when the attention of all of us was turned to the failure of the Atlantic Cable. There was a reason in those days why oil was not used, and that was simply because gutta-percha was then in its prime; they found it as a something sent down from heaven for the special use of telegraph engineers—something that met all our wants. It was cheap, it was good, it was abundant. There was no competition. The result was, being in the hands of one firm, and one firm only, we had the real thing, and there was no attempt at adulteration. I am sorry to say, in answer to Professor Forbes, since those days

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the pleasure of handling real gutta-percha has ceased. In fact, I may tell him—tell you all—I believe in the present day gutta-percha, as gutta-percha, does not exist. The stuff sold as gutta-percha is no more gutta-percha than that table. It is called gutta-percha, but as compared with the original material it is poor indeed. It is because it has ceased to be gutta-percha that it has ceased to be used for other purposes than for cables. It does not seem much to matter what is used for submarine cables. When it is laid, and subject to the low uniform temperature of the ocean, it remains there placid and quiet, and whether it is called gutta-percha or anything else it seems to last very well indeed. But this morning I had brought to my attention a specimen of material that was called gutta-percha that was used in covering and insulating one of our submarine cables. It had failed, and this had only been down in the water some three or four years when it was brought out, and it happened at this time of the year to be rather cold in the water, and the material became brittle, and as it came over the bow sheave it cracked in every direction as though it were resin or practically friable glass. The result is, we do now look with a great deal of suspicion indeed upon any of these cables that are called gutta-percha cables, and although obliged to accept them as gutta-percha, and although obliged to specify them as gutta-percha, and although they fulfil all the requirements and uses of gutta-percha, still I am afraid we must arrive at the conclusion that there is not much gutta-percha in them. There is a great deal of what we call india-rubber which is not always india-rubber. It is a well-known fact that there is imported into this country only one-third of the india-rubber that is sold in this country as manufactured india-rubber. Where does this two-thirds come from? The poor ignorant consulting engineer, whose interests are his employer's, has to specify and draw up specifications, and he does his very utmost to specify the pure jam; but when we find in this hall that contractors themselves enunciate the principle that the specification of a consulting engineer is something to be avoided, I do not think you can feel that they are doing very wrong in doing everything to the best of their

Mr. Preece.

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Mr. Preece. ability to prevent the introduction into the electrical industry of what is absolutely mere rubbish.

Mr. Swinburne. Mr. SWINBURNE: There seems to be an impression that oil not only acts as a good insulation, but that if a breakdown occurs it is self-healing. This may be the case to a large extent in experiments made with small induction coils or Wimshurst machines, but it is quite another matter in cables or transformers where there is some power available. If an oil-insulated transformer goes once it is ruined: the oil is carbonised and the copper fused, and the destruction is as great as in the case of any other kind of insulation. Experiments made with apparatus with no available power are most deceptive.

Oil itself is not to be regarded so much as the insulator, but as a means of keeping cellulose in any of its forms dry. Dry cotton, for instance, is a very good insulator, but if left exposed to the air it absorbs moisture and breaks down. Oil is therefore put round it to keep it from getting moist. It is clear that the oil will increase the conductivity, however good an insulator it may be, for if the hemp or cotton in a cable has a certain conductivity per mile, the oil, being in parallel with it, increases the conductivity. Of course, as oil has a higher disruptive or dielectric strength than air, the addition of the oil will make the cable stronger with reference to sparking through than before, though the insulation may have been reduced by its addition.

The importance of keeping the fibre dry is fully appreciated by the leading cable companies who use oiled jute or hemp. As soon as a cable is cut the end is sealed, or the joint is made at once. It is said that a good joint cannot be made on a large cable during foggy weather, for the serving attracts moisture so quickly that the cable is injured in the neighbourhood of the joints.

Mr. Johnson. Mr. W. C. JOHNSON: The only trial of the fluid insulator with which I was connected was at Birmingham. I am not quite sure whether it was before the one at Uxbridge Mr. Preece has spoken of. At Birmingham about 2 miles of overhead wire on the fluid insulators were erected, under the superintendence of Mr. Cordeaux,

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the Post Office official. The tests were returned to us, and we could not make them out, or how it was they were so exceedingly low. I went down with my partner, and very early in the morning we determined to walk over the course. We had not walked more than 200 yards, following our insulators, when we came to an ordinary shackle, and we then counted six or seven rusty old Post Office pattern shackles introduced into the circuit. Nor was that all. When we got nearly to the end we found the leading wire from our insulators descended into a drain or tunnel, and there it was connected to about a quarter of a mile of very old gutta-percha lead! Mr. Johnson.

Mr. PREECE: It was because the experiment at Birmingham was so absolutely unsatisfactory that we put up the long length from Paddington to Uxbridge. Mr. Preece.

Professor HUGHES: We have had a most interesting discussion this evening, and there are some points which I will try to answer as briefly as possible. I have first to make excuses for my paper, as most of the experiments were originally made 33 years ago, and many of the speakers are more up to date, therefore, than I am on the subject. But I will try to answer some of them. I think Mr. Swinton said there was a great difference in results obtained by himself. I should not think there was any great difference between an induction coil and a Wimshurst machine. But we find an enormous difference in different samples of oils. We tried to get the best we could, and rejected a great many. As regards the 79 times we got, it was by an impulsive rush—a special arrangement, using a Leyden jar that gave the highest oscillation possible. I think Mr. Wimshurst will bear me out in that respect. Coming to Mr. Johnson: I am rather sorry to be compelled to reply to his remarks, because I feel a kind of regret at having infringed upon Mr. Brooks, although it was 15 years before him. I felt so sorry about this that when Mr. Brooks came over (he was such a genial gentleman, I liked him so much) that I never told him I had taken out a patent on this subject; I let him be in his glory, and let him do as he liked. In fact, I should never have spoken of it had not Mr. Preece mentioned my prior claims in his last paper. I Professor Hughes.

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Professor Hughes.

felt very glad that Mr. Brooks was going to test it, because I could never get it adopted, and I was very desirous of knowing what results Mr. Brooks would obtain. As regards Mr. Johnson's remark, that I never mentioned metallic tubes: Well, I suppose if witnesses were needed, I could find witnesses in London who saw those tubes at the time.

Mr. JOHNSON: I alluded to the specification.

Professor HUGHES: I do not think I mentioned the outer tube as an insulating material. I said gutta-percha or other outer tube. It was simply a medium for holding the oil. As regards the Brooks patent, I was under the impression that Mr. Brooks had taken out a fundamental patent for oil as an insulation in a tube. That was absolutely the essential that I patented. But in looking at Brooks's patent I find that he did patent that at first, but immediately afterwards there was a disclaimer. He disclaimed; in his disclaimer he said he was aware there had been previous patents for oil insulators (he does not mention my name, but he must have read up my patent). He makes two new claims —one for heating the oil before it is poured into the tube, and the second for stand-pipes. I believe those are the two main points, and upon those he rests his claim. That is, he has made an improvement on the old oil insulation. Therefore Mr. Brooks does not claim the fundamental principle at all. Mr. Gray, I think, doubted also that I had obtained 79 times difference between the sparking distance of oil compared with air; but, as I have already said, this result was only obtained by using the best oil we could find, and also by using a highly oscillatory spark. Mr. Rogers offers to give me some Russian oil. I should like very much to test this Russian oil, but I may say that I quite agree with Mr. Preece. I wanted, of course, to get resin oil. I at once tested it with my old Peltier electrometer, and I am sorry to say I could not find the same qualities I used to. I had some 30 or 40 specimens, and some were almost as bad as water; in fact, I have not yet got up to my old standard. It is a question of manufacture. I do not know what is the cause at present. It required time to find out the right quality even years ago. Therefore I could not say resin oil resembles at all what we used to have. It is like Mr. Preece's gutta-percha.

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Mr. JOHNSON: I can fully endorse that.

Professor HUGHES: Mr. Varley made some interesting experiments on condensers. He seems to have been puzzled by the different samples of oils, as I am. They overlap one another, so that you get one that is perfect, and the other bad. Thirty-three years ago linseed oil and castor oil were the very worst oils I could try. Now they turn out to be the best. Perhaps they remain constant, whilst the others have deteriorated in the mode of manufacture. You must not take the oil and use it, but test it. Take the best oil, and take the very best insulating oil you can. Mr. Hall says jute covering is better before than after putting in the oil. Well, that is quite true if you pour in bad oil; there is not the slightest doubt about it. If you poured in water it would be still worse. If your resin oil was higher than jute it ought to have improved it; if worse, of course it made it worse. Mr. Trotter wants to know which is best—fibre, jute, or gutta-percha covered wire. I can only speak from past experience: I never found any fibre that could equal gutta-percha. I have found the highest insulation obtained by using gutta-percha-covered wire. I believe with fibre some portions are conducting and some portions are not, and it is very difficult—it requires a great deal of selection. Professor Forbes thinks that gutta-percha could be greatly improved if we could use oil surrounding it. I think so myself, and I pointed out the fact to the gutta-percha manufacturers, and suggested that they should, if possible, introduce oil in their manufacture of it, as it would be a very good thing, because there is certainly a great deal of moisture in gutta-percha, and if manufacturers would take it and dry it thoroughly it might be a far better insulator. Mr. Wimshurst showed me some samples of gutta-percha wire which he uses with his electric machines with ordinary common gutta covering, but kept long in stock so as to be thoroughly dried. This old gutta-percha he finds very superior. It is a question of manufacture. I do not know whether they can mix oil in manufacture or not, but if they could it would certainly improve it. Mr. Swinburne says it will not heal or restore. That is one of the very things everyone is using it for, in Frankfort

Professor Hughes,

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Professor Hughes.

and elsewhere, and Mr. Tesla said his coils were probably pierced many times a day.

Mr. SWINBURNE: Mr. Tesla had no power behind it. With small sparks it will heal, but not an ordinary transformer. With such a small quantity to go through it does not disturb the thing.

Prof. HUGHES: That is one of its merits. I believe it does restore itself, notwithstanding Mr. Swinburne. It heals in every case I have tried. It is very simple to say it will not do it, but I do not believe that it can fail to restore. Mr. Johnson replied to Mr. Preece, and Mr. Preece replied to Mr. Johnson, and the discussion went off a good deal at that moment. At any rate, I have to thank you all very much for your kindness in listening to my reply to the discussion.

The PRESIDENT declared the result of the ballot, the following candidates being elected:—

Foreign Member:

Louis Goichot.

Members:

Robert Hope-Jones. | John Shaw.

Associates:

John Archbutt.	Arthur Hobart-Hampden.
James A. Brown.	Samuel Heap.
Edward Charles Daubeny.	Arthur Hutchins.
Albert Maitland Dempster.	Alfred Leopold de Lattre.
Herbert Willoughby Ellis.	A. H. Wood.

Students:

Walter Ernest Fowler.	Granville Carew Knight.
William Jas. Payne Howard.	Ernest Francis Tallents.

The meeting then adjourned.

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The Two Hundred and Thirty-fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, on Thursday evening, March 24th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 17th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Messrs. C. H. W. Biggs and G. Binswanger were appointed scrutineers of the ballot for new members.

The following paper was then read:—

LOAD DIAGRAMS OF ELECTRIC TRAMWAYS, AND THE COST OF ELECTRIC TRACTION.

By A. RECKENZAUN, Member.

In the present paper I propose to draw attention to two important branches of this rapidly growing subject, "electric "traction." These branches relate to the energy consumed in propelling electric cars, and the commercial results of several tramways in different parts of the world.

Of the errors into which the pioneers in electric traction fell, none was more serious, and I may say more general, than that of providing insufficient motive power. The early motors were all too small, too lightly built, and consequently were not durable. In spur gearing, the double-reduction gear has been abandoned by the principal American firms on account of its great cost of maintenance; the high speed at which the motor pinion had to run was detrimental to the life of the mechanism. A reaction has set in, and attempts are being made to drive cars by placing the armatures directly upon the axle. It

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is questionable whether this method will prove successful in the long run. But the single-reduction gear—that is to say, one pinion and one spur wheel running at moderate speeds—appears to give excellent results, and the repair bills have been largely reduced in consequence.

If we calculate from the accepted coefficients of resistance to traction on common tram rails, we find that an ordinary tram-car will require but 3 to 4 H.P. for its propulsion when once in motion. But it is the setting of a vehicle in motion from a state of rest which demands the greatest amount of energy, and the electric motors which were built in the early days were soon knocked to pieces by the enormous strain put upon them on starting. On English tramways, it has been computed, a tram-car has to stop from four to eight times every mile, according to the amount of passenger traffic. With a view of ascertaining the exact conditions as to the variations in the power consumed on an electric car, I made a series of tests four years ago with an electric car in the public streets of Philadelphia. Fig. 1 represents a portion of a diagram obtained from careful readings with reliable instruments. The whole trip lasted 71 minutes, but the diagram (Fig. 1) represents only the first 19 minutes. Readings were taken and recorded every three seconds. The maximum current, it will be observed, reached at times 120 amperes. This occurred when starting on curves and gradients. Frequently it dropped to zero, and, in fact, these gaps, representing periods when no current was used, gave, on being added up, 45 per cent. of the total. The car contained 84 storage cells weighing 3,400 lbs., and it carried an average of 26 passengers. The aggregate weight propelled was 7 tons. The distance covered during the entire journey was 7 miles, giving a mean speed of only 6 miles per hour, and this had to be maintained on account of the horse cars which ran on the same line. Averaging the current consumed, we find it to be 31 amperes. The maximum E.M.F. was 160 volts, and the mean 157 volts, giving an average of 6·52 electrical H.P. used by the car motor. The maximum current of 120 amperes into the minimum E.M.F. of 140 volts would give 22·5 electrical H.P., but this occurred only nine times during the journey, and

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each time but for a second or two. Currents of from 60 to 100 amperes were recorded more frequently.

The next three diagrams are exceedingly interesting, as they represent the working of three different systems of electric cars on an overhead conductor line at Des Moines, U.S.A. These tests were made on the same day, over the same length of track, with the same number of passengers carried, and the same kind of car body. The cars were operated by the same man, and the readings were taken by officials of the tramway company.

Fig. 2 was obtained from the car fitted with two 15-H.P. Thomson-Houston motors. The maximum current at any time was 75 amperes, the mean 22·4, dropping at times to zero, while the E.M.F. varied between 360 and 520 volts on a run of 20 miles.

Fig. 3 gives the curves from observations on a car propelled over the same distance by means of two 15-H.P. Sprague motors. Here again we observe a maximum current of 75 amperes, average 25·95 amperes, with E.M.F.'s varying from 320 to 560 volts at the motor terminals.

Tests represented by Fig. 4 gave the following results with two 15-H.P. Westinghouse motors:—Maximum current, 95 amperes; mean, 31·3 amperes; maximum E.M.F., 560; minimum, 380.

I could give more diagrams of a similar kind, but these will suffice for the purpose of showing the enormous and remarkably frequent fluctuations of current, and the attendant rise and fall of the energy consumed. With several such cars running on a line with overhead or other conducting medium for the transmission of electricity from the generating station to the moving cars, the peaks would occur at more frequent intervals, filling the great gaps in proportion to the number of cars running simultaneously. Little reliance, however, should be placed upon the possibility of obtaining even a moderately constant load at the generating station. From numerous experiments, I have found that it takes about 20 seconds to bring a tram-car from rest to its normal speed; during this period the current drops gradually from its maximum, due to the ohmic resistance of the

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circuit, to a value corresponding with the load upon the car motor at its normal speed. The stoppages on tram lines with much traffic are so numerous that it will often happen that a large percentage of the cars start simultaneously, when for a moment the engines and dynamos have to exert their utmost power, and before the engine governor has time to act, the current may be down again at, or below, its normal.

I am indebted to Mr. Gibson Carey, of the Thomson-Houston Company, for the diagram (Fig. 5) which represents the fluctuations of the load at the generating station of the Roundhay Electric Tramway at Leeds. Five cars were simultaneously on the line, yet the current dropped to zero 20 times within the space of one hour. These abrupt changes have the effect of reducing the average efficiency of the whole system to a comparatively low figure. The engine and dynamo must be able to furnish the maximum energy demanded at any instant, whilst the mean power in many cases scarcely comes to one-third of the maximum. Measurements made by Dr. Louis Bell on several American electric tramways have shown that the efficiency of an entire system, taking the brake horse-power of the car motors, divided by the indicated horse-power of the engine at the generating station, amounts in some cases to only 25 per cent., and that in no case does it reach over 40 per cent. in everyday practice on a tram line. With electric railways having fixed stopping places at long intervals, with the time-tables so arranged that only on rare occasions several trains start simultaneously, a higher efficiency may be aimed at.

Electric light engineers will look with amazement, and perhaps envy, upon the variations in the voltage marked on Figs. 2, 3, and 4. But these are exceptionally great fluctuations, and there are many electric tramways where the volts do not vary more than 15 per cent. A sudden call for large currents always lowers the potential, and this is partly due to the feeders being proportioned to the average output, and partly to the momentary slowing down of the engine. When the governor begins to act, the excess load has very often already disappeared, and then there is a tendency to racing for the next moment, and a simultaneous

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rise of E.M.F. above the normal. At the City and South London Electric Railway, which is only 3¼ miles in length, the deflections on the voltmeter vary between 370 and 500, notwithstanding compound-wound generators, and a regular schedule time of running the trains. Several years ago I suggested a remedy for equalising to some extent the load upon the generating plant of an electric tramway with overhead conductors, but my suggestion has not been adopted. I advised the use of secondary batteries to act as regulators—not batteries of large capacity, but such as would give a very high discharge rate. Plain lead plates of very large surface, formed on the Planté or any similar process, would certainly add to the smooth working of any railway or tramway where great fluctuations of load are unavoidable, and compound winding insufficient. An accumulator of this description could be made very cheaply, and by allowing sufficient material it can be of any durability desired. The diagrams show that the large excess of current is only required for a few seconds at a time, therefore a storage battery of large surface would furnish this in a far more rational way than the best designed dynamo worked by the most delicately governed steam engine. It would save engine power, and reduce the chances of breakdowns due to sudden shocks to the generating plant.

Again reverting to our diagrams, which to the practised eye reveal a number of interesting points, we find that on starting a single car, from 20 to 50 electrical H.P. are absorbed, and this occurs just when the motor is giving a comparatively small mechanical power at its minimum efficiency; this energy is absorbed in producing the initial statical pull which must be created by many ampere-turns. Now, in order to get many ampere-turns without excessive currents, I designed, some eight years ago, a switch which throws the motors in series when starting, and in parallel (when two or more motors are used) when a sufficient counter E.M.F. is created by virtue of a certain speed. I also subdivided the field-magnet coils, in order to obtain several graduations and regulate the fields to the requirements of the load and speed. Up to that time electric engineers used idle resistances for varying the speed and power. Mr.

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Sprague and others have adopted a similar method of regulation. The Thomson-Houston Company use resistance coils up to a certain point, after which portions of the divided field coils are thrown out of circuit. By a judicious arrangement of field windings to suit the exigencies of a given road, a very fair all-round motor efficiency can be obtained.

The battery car from which diagram Fig. 1 was obtained, required for its propulsion 1·086 electrical H.P. hours per mile run, with an average load of 7 tons over a line having no gradient above 4 per cent. The figures obtained by Mr. O. T. Crosby on three American lines with overhead conductors accord with the above very closely. Thus, on the Richmond line the readings were 1·18 H.P. hours per car mile, at Cleveland 0·9, and at Scranton 0·94 H.P. hours. These cars were very much lighter than the one driven by accumulators, but there is a gradient of 9 per cent. at Richmond, and one of 7 per cent. at Scranton, while the Cleveland line is tolerably level; the speeds at Cleveland averaged 9 miles per hour, and at the other two lines 6 miles per hour. The coal consumption measured at the generating stations came out as follows:—For the Cleveland line, 8 lbs. per car mile; and at Scranton, 5·6 lbs.; that of Richmond could not be ascertained, as the same boilers were used for both the tram line and the electric lighting plant.

On the Frankfort-Offenbach Tramway, and on the Mödling line, near Vienna, both built by Messrs. Siemens & Halske, the coal consumption varies between 7 and 8 lbs. per car mile. Unfortunately, I have not been able to ascertain the evaporative quality of the coals used in these places.

Mr. J. S. Badger, in a highly interesting recent communication, relates that the very best station performance he met with in the United States was 1 electrical H.P. for 5 lbs. of slack coal evaporating 7½ lbs. of water. Writing on the consumption of fuel in general on a number of American tram lines, he found that it varied between 4·3 lbs. to 12·2 lbs. per car mile, according to the quality of fuel and the nature of the road and load. Many of the American tramways have recently adopted large cars up to 25 feet in length inside, whereas the usual

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standard type of car is only 16 feet, carrying a proportionately smaller number of passengers, although I have frequently seen 70 passengers occupying a 16-foot car intended to seat 22 people. Overcrowding is not allowed on English tramways.

I have collected a very large amount of material with regard to the all-important question of operating expense, and I am greatly indebted for information given by Mr. Alexander Siemens, Dr. E. Hopkinson, Mr. Holroyd Smith, the Allgemeine Elektricitäts-Gesellschaft, Mr. J. S. Badger, and Mr. Robert W. Blackwell.

Full tabulated statements are appended: to read the same would occupy more time than is at our disposal; moreover, it would add to the monotony of an already very dry paper. With your permission, I will only make such observations regarding each table as may render some assistance to the study of them.

Due regard should be taken, in each of the lines, to local conditions. In Germany, for instance, the wages, which form the principal item in the operating costs, are about one-half those paid in the United States. Wages and salaries in England take an intermediate position. Again, the cost of fuel varies in almost every city in the world.

One of the oldest electric tramways in the world is that of Frankfort and Offenbach, in Germany. It was built by Messrs. Siemens & Halske, and opened to the public in April, 1884. Table I. shows the detailed working expenses for the year 1890, giving the sum of 4·608 pence per car mile, including all charges. The first financial year—1884-85—gave a much higher figure, namely, 8·256 pence per car mile; and it is interesting to note that the working expenses were reduced year after year: this was largely due to improved appliances and the consequent reduction in the maintenance items, as well as in wages. The expenses of the other tramways in the city of Frankfort (which are worked with horses) for the year 1890 amounted to 9·022 pence per car mile, or nearly double that of the electric line. The slotted-tube overhead-conductor system is used here, and the entire rolling stock consists of only 14 cars—10 of which are fitted with electric motors, serving as tow cars—holding 24 passengers

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each. About 1,050,000 passengers are carried per annum; this is considered a very low figure, and is partly due to the competition of a local railway. In consequence of this small traffic, the ratio between expenditure and revenue is as high as 70 per cent., notwithstanding the moderate traction costs.

A more recent and equally interesting example of electric tramway enterprise in Germany is that of the city of Halle. The cars on this line were originally drawn by horses, but a year ago the municipal authorities gave permission to the Allgemeine Elektricitäts Gesellschaft to erect overhead conductors on the plan adopted by Mr. Sprague in America. I am indebted to Mr. Emil Rathenau for a detailed statement of working expenses ranging over six months—from the 1st of July until the end of December, 1891. Table II. gives the averages per month and per car mile. The general expenses include taxes, municipal charges, and water rates. The water is obtained from the town supply, the sinking of a well having been found impracticable. The rolling stock consists of 25 cars; the average number actually running was 20·55. They carry drivers, but no conductors, and this fact renders the wages account remarkably low, the entire working expenses coming to only 2·624 pence per car mile, or 54·5 per cent. of the gross revenue. It must be observed, however, that no allowance has been made for depreciation, which, if included, would add nearly another penny per car mile to the expenses account. Incidentally it may here be mentioned that the Imperial German Government stopped the running of the electric cars immediately after the opening ceremony because they interfered with the proper working of the telegraphs and telephones. The case came before the law courts, when the judges decided in favour of the tramway company, stating at the same time that the streets of a city were intended for general traffic, that their course could not be altered, but that the postal authorities could easily arrange the telegraph and telephone wires so that they should not be influenced by the electric lines, which use the rails and earth for the return circuit. The alterations were made, and the tramway has had a good time ever since.

The most remarkable of European electric tramways is that of

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Budapest, which was constructed by Messrs. Siemens & Halske, of Berlin. A few months ago I visited the Hungarian capital, and I was delighted with the perfect working of the electric cars. Underground conductors are used in this case, and the conduit is immediately underneath one of the tram rails, so that there is no additional slot in the roadway. About 50 electric cars are now in operation over 20 miles of track. The system is exceedingly popular, and it carries a larger number of passengers per mile than the horse tramways. This is probably due to the greater speed, the electric cars being allowed to run 11 miles per hour in some of the streets. I have not been able to obtain detailed working expenses, but I have been informed that the same never exceeded 50 per cent. of the revenue since the opening of the line two years ago. The ratio of working expenses and revenue on the horse-car lines at Budapest is 72 per cent.

Another among the numerous Continental electric tramways is that running between Florence and Fiesole, a distance of 4·5 miles, which was the first continuous steep-grade electric line in Europe. The Sprague system is used in this case. Table IV. is prepared from data contained in a recent paper by Mr. C. P. Sheibner, of the Institution of Civil Engineers. The author of this interesting paper gives, under the heading, "Motive "Power," the sum of the expenses arising through wages at the generating station, wages of car drivers, fuel, water, repairs, &c. These I have divided in my table in order to facilitate references with data obtained from other lines. It would be an excellent thing if all the electric trramways would keep a uniform record of all the details of working expenses, in the manner done by the existing horse tramways under the Board of Trade rules.

Coming now to electric traction enterprises in the United Kingdom, we find in Table III., referring to the Blackpool Tramway, that the gross expenses amounted last year to 9·91 pence per car mile, or 57·8 per cent. of the gross receipts. This is a most satisfactory result, considering that the winter traffic of this seaside resort is only just sufficient to cover operating costs, and all the profits have to be earned during the holiday

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seasons. This is the only line in England, and the oldest line in the world, worked with underground conductors in a slotted conduit. The rent charged by the Blackpool Corporation for the use of the line forms a considerable item—1·773 pence per car mile—in the sum of the working expenses. Directors' fees and secretarial charges also seem to be liberally arranged, making 1·187 pence per mile.

On the Bessbrook-Newry Tramway, where water power is used for driving the generators, the haulage costs during six months ending December 31st, 1890, amounted to 3·97 pence per train mile. This includes water rent, rental of buildings; wages of driver, guard, and dynamo attendant; maintenance and repairs to dynamos and conductor; oil, tallow, and waste. During the period stated the train mileage was 10,400; the number of passengers, 50,800; and goods carried, 8,600 tons.

The Americans, as you are well aware, are far ahead of us in matters relating to tramways; they count their electric roads by hundreds, and their electric cars by thousands. I need not enlarge here upon figures well known to all readers of engineering literature, but I venture to point to recent statistics on working expenses, and especially to those prepared by Mr. J. S. Badger for the Street Railway Convention last October when meeting at Pittsburgh. Mr. Badger gave an immense amount of data, all systematically arranged, of which Table V. is an abstract, converted into English equivalents as to money values. Those interested in all its details will find a reprint of this elaborate communication in the *Electrical World* of October 31st, 1891. Indeed, in order to fully appreciate the data contained in Table V., certain descriptive matter should accompany it, and this deals with the number and sizes of boilers, engines, dynamos, the quality and price of coals, condition of road and track, sizes and number of cars, total mileage, and wages in various localities. I merely give the most essential ones.

Road No. 1 has a generating station containing four boilers, one 250-H.P. Corliss engine, one 150-H.P. Ball engine, one 150-H.P. Brown engine, and two Edison generators. The line runs through 5 miles of streets, and the steepest gradient is

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13·2 per cent., 100 feet long. There are 36 motor cars of the ordinary size, of which 20 are in daily use, making annually 601,966 miles. The price of coal is 8s. 4d. per ton, and the consumption 7 lbs. per car mile.

Road No. 2: Four boilers, two Taylor-Beck engines of 125 H.P. each, one of 175 H.P., one Armington & Sims of 250 H.P.; five Edison dynamos. Track, 6 miles long; steepest gradient, 9·25 per cent. for 400 feet. 18 motor cars, of which 16 are in daily use, making an annual mileage of 724,000. Price of coal, 7s. 3d. per ton; consumption, 11 lbs. per car mile.

Road No. 3: Eight boilers; nine steam engines, of which five are of 125 H.P. and two 100 H.P., all of the Phœnix type; also two 125-H.P. Beck engines. Nine Edison and seven Thomson-Houston dynamos supply current for 50 motor cars in daily use, making an average of 1,819,000 miles per annum. There are altogether 53 motor cars, of which four were supplied by Westinghouse, 20 by Edison, and 29 by the Thomson-Houston Co. Steepest grade, 8 per cent. for 800 feet; total length of streets traversed, 35 miles. Cost of coal, 11s. 2d.; and the consumption of fuel is calculated to be 12·2 lbs. per car mile.

Road No. 4: Two boilers and three 150-H.P. Ball engines, driving six dynamos. Length of streets run through, 14 miles, with 18 motor cars, making annually 700,000 miles. There is one steep gradient of 10 per cent. rise for 775 feet. Coal in this case costs 8s. 9d., and the amount used per car mile is only 6·4 lbs.

The fifth road, out of seven given by Mr. Badger, is only 4 miles long, with five passenger cars and one freight car. It has only been running for nine months up to the date of the report. The coal costs 12s. 8d. to 15s. 10d. per ton; water, 5d. per 1,000 gallons. The wages on all the above lines varied between 40s. and 60s. per week for drivers, conductors, and mechanics.

The most important of all the American electric tramways is that of the West End Railway Company of Boston, where they have now 400 electric cars in operation. Table VI. gives a comparative statement of gross receipts and working expenses of the electric cars and horse cars in the city of Boston for five

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months ending August, 1891. No detailed account is given, but the rolling stock is continually being increased, and the central power station in course of construction will contain engines capable of converting 12,000 H.P. into electrical energy. All the American lines referred to are worked on the overhead conductor principle.

I have intentionally omitted comparisons between electric traction and various modes of mechanical traction, such as cable, steam, and compressed air. Our object in this Institution is to inquire into the merits of systems involving the use of electricity, and to further their extension in every legitimate way, and with this point in view I have endeavoured to collect and bring before you such data as may tend to increase the general interest in one important branch of our profession.

Table I.—The Frankfort-Offenbach Electric Tramways.

WORKING EXPENSES OF THE YEAR 1890.

	Pence.
Fuel, per car mile	0·48
Wages of drivers and conductors ...	0·864
Salaries and wages at the generating station	0·96
Maintenance of machinery	0·576
„ „ line and buildings ...	0·288
Taxes and municipal charges	0·058
Miscellaneous	0·518
Depreciation account	0·864
Total per car mile	4·608

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Table II.—The Halle Electric Tramways.

WORKING EXPENSES FOR SIX MONTHS ENDING DECEMBER, 1891.

Average number of cars running ...	20·55
Total car miles for six months	266,796
Average number of car miles per month	44,466
„ „ „ „ day ...	1,449·3
Daily average mileage for each day, per car	70·6
Total gross receipts for six months	£5,354 . 8s.
Average gross receipts per month	£892 . 8s.

WORKING EXPENSES.

	Pence.
Coals	0·3980
Waste, oil, and cleaning materials ...	0·1444
Wages and salaries	1·4430
General expenses	0·3930
Life insurances	0·0216
Maintenance of track and overhead conductors	0·0459
Maintenance of buildings	0·0036
„ „ boilers and machinery ...	0·0120
„ „ cars	0·1480
„ „ workshops	0·0148
Total working expenses per car mile ...	2·6243

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Table III.—Blackpool Electric Tramway.

WORKING EXPENSES FOR 1891.

		Per Car Mile. Pence.
Total number of car miles for one year ...	98,000	
Average car miles per month	8,166·6	
Gross receipts for one year	£7,241	
Repairing centre channel		0·345
„ roadway		0·452
Rent, at 6½ per cent. of cost of line, to Corporation		1·773
Repairs of armatures, fittings		0·461
Decorating and plumber's work		0·189
Fire and boiler insurance		0·019
Ground rent		0·125
Wages of drivers, conductors, engineer, ticket clerk, &c.		3·260
Coal, oil, waste, &c.		0·714
Water and gas		0·223
Rates and income tax		0·874
Printing and stationery		0·076
Salaries of directors, secretary, auditors, and clerks		1·187
Compensation for damages		0·016
Miscellaneous		0·197
Total working expenses per car mile ...		9·911
Ratio of operating expenses		57·8 %

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Table IV.—The Florence and Fiesole Electric Railway.

WORKING EXPENDITURE, BASED ON AN AVERAGE OF 600 CAR KILOMETRES, OR 375 CAR MILES, PER DAY.

	Per Car Mile. Pence.
Traffic—	
11 guards and 3 inspectors	0·960
Maintenance—	
Inspector, foreman, and 8 men	0·680
Motive Power—	
Wages at generating station	0·386
10 car drivers	0·898
Fuel, 1 ton per day, at £1 12s. per ton...	1·003
Water, oil, repairs, and sundries... ...	0·513
Depreciation and Renewals—	
Boilers, 7 per cent.; electric plant, 8 per cent.; cars, 5 per cent.; line, 1 per cent.	2·080
General Charges—	
Rates and taxes, office, and administration	1·050
Total per car mile	7·570

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Table V.

WORKING EXPENSES OF FIVE REPRESENTATIVE AMERICAN ELECTRIC TRAMWAYS.

	Road 1.	Road 2.	Road 3.	Road 4.	Road 5.
	Per Car Mile.	Per Car Mile.	Per Car Mile.	Per Car Mile.	Per Car Mile.
Permanent Way.	d.	d.	d.	d.	d.
Maintenance of road-bed and track	0·225	0·335	0·475	0·085	0·050
Maintenance of line	0·035	0·075	0·100	0·105	0·030
Maintenance of Power Plant.					
Repairs of engines and boilers	0·018	0·055	0·195	0·013	0·036
Repairs of dynamos	0·008	0·007		0·018	0·008
Miscellaneous repairs		0·118		0·019	0·004
Cost of Power.					
Fuel	0·335	0·483	0·822	0·345	1·078
Wages at generating station	0·296	0·186	0·212	0·196	0·437
Oil and waste	0·032	0·077	0·102	0·029	0·109
Water	0·046	0·035		0·027	0·147
Other supplies	0·005	0·010		...	0·116
Maintenance of Rolling Stock.					
Machine shops, and repairs to motors and cars...	0·922	0·414	1·503	1·162	0·293
Transportation Expenses.					
Wages of conductors and motor men...	2·318	1·778	2·236	2·516	1·826
Miscellaneous wages for inspectors, trackmen, and cleaners	0·526	0·159	0·095	0·018	1·234
Accidents to persons and property	0·013	0·053	...	...	0·003
General Expenses.					
Insurance	0·766	0·094	0·230	0·036	0·084
Salaries, &c.		0·324		0·272	0·685
Office expenses	0·021	0·014	0·081	...	0·035
Advertising, printing, legal and miscellaneous expenses	0·085	...	0·094	0·693	0·131
Total expenses per car mile ...	5·601	4·217	6·145	5·529	6·301

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Table VI.—West End Railway Company of Boston.

WORKING EXPENSES FOR FIVE MONTHS ENDING AUGUST, 1891.

	Electric Traction.	Horse Traction.
Gross receipts	£29,337 . 6	£78,779 . 4
Track and car expenses ...	9,030 . 8	26,729 . 6
Motive power	5,733 . 0	23,949 . 0
Total operating expenses ...	16,274 . 2	55,043 . 4
Miles run	375,474·8	1,093,808·2
Ratio of mileage	25·75 %	74·25 %
Ratio of operating expenses	55·47 %	69·87 %
Total expenses per mile run	10·41d.	12·06d.
Gross receipts per mile run	18·76d.	17·26d.

The PRESIDENT: We have, I believe, several gentlemen present representing electric railways and tramways, some of whom are members, and some of whom are visitors. We shall be very glad if one of them will open the discussion. Perhaps Mr. McMahon will do so? The President.

Mr. MCMAHON: Mr. Reckenzaun, in speaking of the variation of voltage, says that at the City and South London Railway, which is only 3¼ miles in length, the deflections on the voltmeter vary between 370 and 500, notwithstanding compound-wound generators and a regular schedule time of running trains. I cannot agree with him on this point. Reading it as he puts it, it would appear that we have a continual variation of 130 volts in 500. It may possibly occur once or twice a day through a belt slipping, or if the machines are running shunt; but as a rule the machines are running compound, and then the variation is between 420 and 500. In plotting load diagrams from observed ammeter readings, I have come to the conclusion, after taking several thousand such readings, both at the generating station switch-board and on the locomotives, that unless these readings are taken almost every second the real variation and that observed will differ by a considerable amount. This statement I have verified by comparing two curves, one from a recording ammeter, Mr. McMahon.

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Mr. McMahon.

and the other plotted from readings taken every five seconds. The sharp peaks so clearly shown on Mr. Reckenzaun's diagrams are rounded off, and the points sometimes become quite flat, and last for a couple of seconds. Even with readings taken every five seconds you lose these interesting points. I have taken a number of these readings and diagrams, and had intended to enlarge the diagrams, but had not time; however, I hope at an early date to bring some of them before the Institution. I would like to ask Mr. Reckenzaun how the three-seconds readings were taken. I have found it difficult to take even five-seconds readings with any accuracy, especially over a long period, unless one calls them out and another dots them down, and then you want somebody to call the time out as well. Mr. Reckenzaun does not seem to place much hope in getting nearly a straight line, or at least a wavy line, for the current-curve. But I think it is at least possible to get something of that sort, and this assumption is based on my experience of the difference between a load diagram for eight trains and one for five trains. With eight trains running the current from one generator varies between 100 and 350 amperes, and will not drop as low as 80 in an hour. With seven and six trains the curve becomes more jerky; and with five trains the current varies between 300 amperes and zero in a few seconds, and remains between 0 and 50 for about five minutes, seldom keeping at 300 for more than 30 seconds. This is a very interesting diagram, and I am sure when we have more trains running that those hollows will be filled up and we will get an almost flat curve. I think with 10 trains running and a more frequent service we shall only get a variation of something like 10 per cent. in the voltage. I do not know whether the suggestion of putting accumulators as regulators would be very effectual. I think it would be very expensive, and as far as I have seen accumulators used for traction purposes they are rather a nuisance. Mr. Reckenzaun also makes mention of high resistance in the locomotive. Well, those are only used in switching on and off, and, as a matter of fact, once the locomotive gets under way the resistance is not used. The back E.M.F. simply controls the

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current, as it were. I should also like to know at what point the Thomson-Houston motors have the resistance cut out, and at what point they vary or regulate their speed by adjusting the field-magnet coils. I think those are all the points that strike me as likely to be of interest to you. Mr. McMahon.

Mr. MAGNUS VOLK: I do not know that I have much to add to what Mr. Reckenzaun has said; but upon looking at those diagrams before us, it seems to me very desirable that some means be contrived to equalise the load upon the engine: certainly it will not tend to very economical working if it be necessary to provide engines of a power equal to three times the average load. Mr. Volk.

I would suggest that a heavy fly-wheel, running at a very high rate of speed, be tried. I do not know that I am sufficiently acquainted with the engineering problem involved in getting a sufficient weight to run at such a high velocity, to indicate the precise means of attaining such a desirable object; I am perfectly well aware that the engines used at present have large and heavy fly-wheels, and that the engines in some cases run at a high rate of speed, but the speed of the engine is not enough to attain the desired result.

In my own case at Brighton I only use a 12-H.P. gas engine, provided with two very heavy fly-wheels, running at a comparatively low rate of speed, viz., 160 revolutions per minute; but two heavy cars can be started practically at one time without seriously checking the speed of the engine, whereas a steam engine of the same power having the ordinary fly-wheels would probably be pulled up at once.

Of course, sufficient margin must be provided in the dynamos to allow for the extra flow of current.

Referring to the working expenses, it seems that, averaging up the various tables—although, as they embrace diverse matters, it is impossible to get the figure exactly—we may assume it to be, when driving by the direct current, about 5d. to 6d. per car mile, if there be a fair amount of traffic.

I have here an average for several years' working of the

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Mr. Volk. Brighton Electric Railway, and the cost per car mile is as follows:—

	Pence.
Gas, oil, and waste	1·20
Repairs to gas engine	0·04
Engineers' wages	0·70
Repairs to electrical machinery	0·16
Drivers, conductors, booking clerk	1·60
5 per cent. depreciation	0·30
(All repairs being being paid out of revenue.)	
	4·0

Number of passengers per car mile ...	8·51
Daily average mileage per car	75·0
Proportion of gross expenses to receipts ...	55 per cent.
Mileage per car per annum	23,475

The working expenses on my line come rather lower than usual, owing to the cars running on a T-headed rail, and not in a grooved rail, as used in the public streets.

Mr. Baker. Mr. Graff Baker: I have listened with a great deal of interest to Mr. Reckenzaun's paper, and I think I have seen a good many of his figures before. The American figures I am fairly familiar with, but the German figures are rather new, and I think compare favourably with figures obtained anywhere. Mr. Reckenzaun makes a point of the low cost of labour in Germany. The cost of labour there is of course very much less than it is in America, but I would like to hear his comparison of the cost of labour there and the cost of labour in England. Then his cost of fuel in Germany: I would like very much to have that compared with the cost in England. In the States the company I am connected with have down a great many tramways quite out of the experimental stage; and in almost every case, if anyone wants to know what it will cost to operate his tramway electrically, he can ascertain it by turning to the operating costs of some similar road. Over here the circumstances under which tramways are run, the conditions of climate, the conditions of rails that are used, the streets, and, altogether, the

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Mr. Baker.

electrical conditions, are so very different that it is practically a new field; and my experience with the Roundhay Road at Leeds is that every precedent established from American practice has to be modified to meet the different conditions governing here, viz., the use of 300 volts instead of 500 volts. At Leeds we find that, owing to the grooved girder rail being used—which is, I believe, the latest improved thing in tram rails—the rolling friction is much higher than in the States; and, owing principally to the climatic influences, the rails are more slippery, their adhesive properties are less, than in the States, where they use either a step rail or a T rail. The atmospheric conditions, the fog, the grease, the mud from the macadam roads getting on the rail, make a difference between the clean rail and the usual rail that you get in winter weather that is perfectly marvellous. We sent over to Leeds motors that have done very excellent work—phenomenal work in some cases. When we got them to Leeds, and got them running in November and December, the traction results varied in almost every point from the results given by the same motors in the States. I knew, of course, beforehand that the working was going to be slightly different; but as soon as I discovered the very great difference that existed, I immediately set to work to ascertain just what the points of difference were, and what was needed for the English conditions. To this end I had to employ a much larger staff than was necessary for the working of the tramway, so as to be able to secure every engineering detail that would be any clue to future working. At Leeds my line is a summer line, in that it leads out to a pleasure resort. On Sundays and holidays there are far more people than I could carry with 50 or 60 cars. The expense has been greater than it would be on any line that is run purely upon a commercial basis, owing to the cost of getting out these particulars, and testing and finding out all the engineering difficulties to be overcome. Mr. Reckenzaun asked me to give him some figures as to cost of operation. I felt some hesitation in complying with his request, without being able to state some facts first. I want to show the results of what I have done, and I want to make the figures public; but I do not want to do so

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Mr. Baker. without prefacing them with the explanation of why they are not so low as some American figures, where everything is cut and dried,—where you can contract for your coal and always get it. You can count upon the prices for most things continuing fixed for a fairly long stretch of time. But I have brought some figures to-night which I am afraid do not compare very favourably with Mr. Volk's figures, but may be of some interest. We keep our records of cost in Leeds under four headings—(1st) *General Expenses*, which include management, salaries of clerks, accountants, and so forth, miscellaneous office expenses, stationery, printing, injuries, and damages; (2nd) *Transportation Expenses*, which are the car service expenses: this includes all wages of drivers and conductors, ticket inspectors, and starters—everything in connection with the service of the cars—and inclusive of car house expenses, which comprise the wages of the inspectors of motors, engine-drivers, of cleaners, &c., waste, and fuel, water, gas, &c., &c.; then there is (3rd) *Maintenance of Ways and Buildings*—repair of track, renewal of overhead line, and generally all necessary material and labour for the proper maintenance in first-rate order of the buildings and lines; (4th) *Maintenance of Equipment*—car bodies, trolleys, sundry repairs of same, and of electrical power plant, &c.

The general expense is very high. Having extra clerks for the purpose of securing and keeping a record of the engineering data will make, I should think, this item about twice what it should be. Then I have Mr. Wimslow in charge. He is an American, and naturally a man to do the same work that lived here, and was accustomed to the country, would be able to get along a bit more economically. This general expense account comes to about 1·13d. per car mile. Transportation Expenses.—These, again, are high. I have a temporary installation at Leeds, and no duplication of plant of any sort, only one high-speed engine with two fly-wheels weighing about two or three tons each, running at 200 revolutions, and two dynamos. Of course, if anything happens to the engine, we shall have to stop the road. We have been running for four months so far, and have only stopped for an hour and 20 minutes, owing

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to heated crank-pin. We have to be very careful to see every- Mr Baker.
thing right when we shut down at night. We have three engine-drivers. I pay my head driver £3 10s. a week. I have two assistants; there is always one on duty, and the chief has to be there, if wanted, 24 hours a day. I had a great deal of difficulty from the first in getting men at all familiar with motors in this country—that is, the type of motors we use. We could get plenty of good electrical men, and plenty willing to learn; but it was difficult to find a man whom we could trust to go in and take charge of a motor and know what to do without being told. I found two Englishmen who worked in the States over here, on a holiday, and offered them sufficiently high wages to induce them to stop. All these things seem very little, but they add enormously to the cost per car mile on any road. Transportation expenses, including everything—conductors, drivers, ticket inspectors, and starters—everything you can possibly put down to transportation—come to 5·17d. per car mile. These figures are for one fortnight, taken at random. Maintenance of Way and Buildings.—The permanent way is almost new throughout, and the buildings, having been only completed last autumn, have needed no repairs. Maintenance of Equipment.—That has been since we started rather heavier than it should be, owing to our having got our wheels from the States. Our wheels were made for running on step rails, and when they got into grooved rails, the flanges did not suit, and we had continual trouble; the flanges kept chipping off, and every fortnight or so we had cars in, putting on fresh wheels of suitable dimensions. Then we have had rather a severe winter; at one time we had an average of 7 inches of snow over the road. This had all to be cleared away. It was not worth while having a snow-sweeper, as used on a regular "snow" road, and it had all to be done by hand; we had also to use salt on the track; and this all makes the expense of maintenance of the line a great deal more than it would otherwise be. This comes out at 0·33d. per car mile. This makes a total cost of operation for this particular fortnight of 6·63d. We ran in the fortnight 6,039 miles with, say, an average of five cars. We find the five cars,

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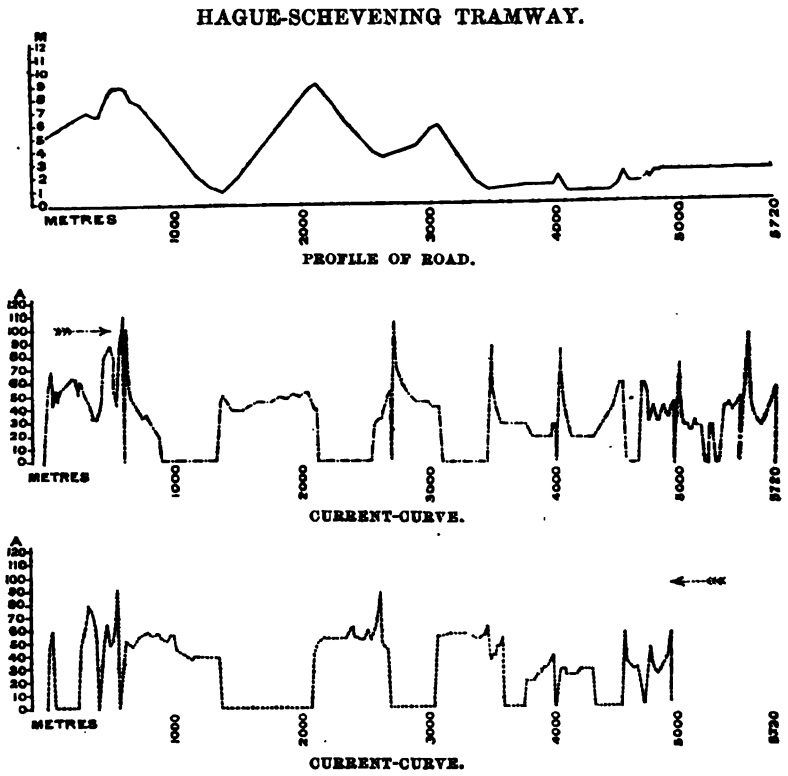
Mr. Baker. unless in very good weather, more than carry the people, and, as I said before, when the people do come out to ride, if we had 60 cars we could not transport them. But we have carried 100 people on a 16-foot car, which is not in accordance with the Board of Trade regulations; but these Yorkshire youths—miners and others—when they come out for a holiday, if they want to ride on the electric car, they have got to ride on it, regardless of rules.

Mr. Russell. Mr. STUART RUSSELL: The diagrams I have put on the wall shows the results of some tests I had the opportunity of taking



about two years ago on the Hague-Schevening Tramway. In the diagrams the full curve shows a profile of the line, the vertical heights being shown on a scale of 100 to 1 as compared with the

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horizontal distances. The broken line curve shows the current taken from readings made as rapidly as possible whilst running from Schevening to the Hague—that is, from left to right on the diagram—and the dotted curve shows the similar readings on the return journey. You may note that on the right-hand side the broken line curve only, is shown for about the last 1,000 metres: that is because in part of the town the up and down lines do not run through the same streets, and the loop thus formed has been shown as a continuation of the line, and the current has been shown by the broken line curve as far as the point where the up line rejoins the down again, the dotted curve commencing from this point. From these figures, and many other tests taken at the same time, we found that on a straight and level piece of track the average current was about 30 amperes, which means a tractive force of nearly 20 lbs. per ton. About two-thirds of the track is outside the town boundary, and for the greatest part of this raised T-headed rails are used; but within the town of course it is all grooved rails. Outside the town the average speed is about 14 miles an hour, reaching a maximum of 20 for some parts; whilst within the town the average speed is 7 miles an hour. The cars are battery cars, 32 feet long over all, and weighing, with their full complement of passengers, battery, motor, &c., 17 tons. On the curves and gradients the maximum reached in the tests I took whilst running amounted to 110 amperes, which is equivalent to about 145 lbs. per ton. On starting the car the average current, calculated from a large number of tests, comes out at 80 amperes, or about 100 lbs. per ton; but the maximum observed when starting on curves or gradients was 180 amperes, which is equivalent to a tractive force of nearly 240 lbs. per ton. Mr. Russell.

Mr. Vanvloten, the engineer of the line, recently sent me figures of further tests he has made, and he gives for a double journey to the Hague and back to Schevening—that is, about 6½ miles—occupying about 36 minutes, an output of about 20 ampere-hours. The batteries are arranged so that they can give 400 volts, or can be coupled in parallel to give 200 volts within the town boundaries, where the cars are obliged to run at half-speed. Of these 20 ampere-hours it appears that

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Mr. Russell. 9 ampere-hours are required for starting the cars from rest and bringing them up to their normal speed, and only 11 ampere-hours for the other running time; so that, according to these figures, we may say, roughly speaking, that half the output was required for starting the cars. This was when making an average of five or six stops per mile only. The observations were made by taking readings as rapidly as they could be written down, and the times of passing certain definite marks on the line were noted; so that the readings were divided into sets, each located to a certain known portion of the line. The general result from the double journey with this car was about 8 h.p.-hours—that is, on the length of 6½ miles, about 1·25 h.p.-hours per car mile—and the average rate of working over the whole trip is 13 H.P. The figures I took were taken when the road and motors were quite new, and when some of the curves gave a good deal of trouble. Those curves have been altered since, and the engineer tells me that they do not now have quite such large current-outputs when running.

Mr. KAPP: What is the weight of the cars?

Mr. STUART RUSSELL: About 17 tons, loaded.

Mr. KAPP: That is a tremendously heavy car.

Mr. RUSSELL: They are 32 feet long, carry 200 cells, and weigh empty—that is, including cells and motors—about 14 tons. They carry about 60 passengers.

Mr. Dolby. Mr. E. R. DOLBY: If you will allow me, I would like to speak on one or two points. The great difficulties in motor and tram-car working have been with the gearing and with the enormous current which is needed to start the motor. From experiments which I have been able to make, I believe that the number of amperes into volts giving the watts equivalent to the top of each one of those peaks which represents the starting of the car is very much in excess of the actual energy which is necessary to start that car; and I should like to ask the author of the paper if he has been able to make any experiments with an ordinary tension dynamometer to find out how much power is absolutely necessary to start that car. I believe that, owing to the fact that the armature of the motor is perfectly stationary, current

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above that requisite for the starting torque is used. I have come to that conclusion, for one reason, from experiments I have made upon electric cranes. Where you have the armature shaft of the electric motor geared directly to the large wheel of the lifting gear, you have a somewhat similar condition to that which obtains in a single-gear motor used for tramways. On the City and South London line the average current is 50 amperes, and at the starting of a car that current rises to 150 amperes. Therefore you have a starting current three times in excess of your average working current. Now, supposing you have in an electric crane the armature shaft provided with a papier-maché or paper wheel at the end of it, which can by means of gearing be entirely freed from the smooth pulley on the main shaft of the crane: you can then start your motor with no load on, run it up to high speed, get the requisite E.M.F. for damming the current back, and then by means of a screw, put the paper wheel into gear with the lifting shaft, or with one of the lifting shafts. In my opinion, an ideal motor would be one which could be run with its armature perfectly free, and always in one direction. If you can arrange some kind of clutch gear or friction gear, such as I have alluded to, by which you can then put the load upon one of the driving wheels, I believe the top of the peaks recorded at starting would then be avoided, and a much more rounded outline would be obtained. Mr. Dolby.

Mr. MANVILLE: Mr. President and gentlemen,—I am sorry that my friend Mr. Reckenzaun considers his paper a very dry one. For my part, I think it is perhaps a paper of greater utility on the subject than any that has yet been before us. Perhaps the one misfortune we have suffered from in this country, where we have talked a very great deal about electric tramway traction and done so very little, is the want of proper and authentic information as to actual technical results obtained in running electrical tramway systems on a large scale. Also, instead of mere estimates of the cost of running, which directors justly fear, we want the actual financial results of such tramway systems; and Mr. Reckenzaun's paper, devoted to these two points, has given us information invaluable to all engaged in developing this subject Mr. Manville.

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Mr. Manville.

in the future. The question of efficiency of engines referred to is certainly rather a serious one. If it takes—and I presume we all know it does take—about four or five times as much power to start a car as to keep it in motion, and if, as is shown to be the case at Roundhay, all the cars may stop at a time, it is obvious that we must have engines of such a power that we can never run them at anything like a real efficiency. We must always have the ability to put forth a much greater power than the normal power. I think the idea of using a rough accumulator, which is not difficult to manage, and is no expense to maintain—an accumulator with plain lead plates—ought to be a very valuable adjunct to a tram system, enabling us to use engines of an economical size for the normal work, and depending on the accumulator for the extraordinary efforts. There is one question seriously affecting us in this country in developing electric traction—the question of the interference with telegraphs and telephones of electric tramways using earth returns. I believe, in the States, as in Germany, the Courts have held that the streets are properly used for locomotive purposes, and nothing else using the streets incidentally should be allowed to interfere with locomotion. Therefore the electric tramways had, if I may so put it, the electrical right of way in the streets. During the last three sessions in Parliament some four or five tramway companies have applied for powers to use electric traction on their tramways, and I have given evidence on their behalf against the telephone clause being inserted. Unfortunately, with one exception—a slight one—this clause has always been inserted. If electric tramways go ahead, as they must and will do, they must not be hampered by such restrictions as those imposed by this clause. I have no hesitation in saying myself that it is almost physically impossible to run, certainly with any amount of success, a tramway on the overhead system with double conductors having a difference of potential between them; and although it may be possible with double conductors in a conduit, it will be so much more expensive in capital cost that tramway directors will hardly care to incur it. Last year the Birmingham Central Tramways, one of those large companies

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to whom we look to help us in advancing electric traction, which can only be successfully introduced by powerful companies, promoted a Bill in Parliament, and, after a most severe fight with the telephone companies, had perhaps the strongest telephone clause yet inserted in a Bill proposed to them by the Committee. The company thought it was of no further use to proceed with the Bill, and they dropped it, and another chance for electric traction was altogether lost. In this session of Parliament an effort is being made to try the effect of union against such clauses being inserted in favour of the telephone companies. Mr. Manville.

The PRESIDENT: We shall scarcely be able to finish the discussion to-night; but if there are any visitors who will be unable to attend the next meeting, we shall be very glad to hear their remarks now.

Mr. STUART RUSSELL: I should like to ask what the weight of the American cars are on the Thomson-Houston, Sprague, and Westinghouse systems.

The PRESIDENT: I now adjourn the discussion on Mr. Reckenzaun's paper until April 7th.

The following candidates were declared to be duly elected:—

Member.

Charles Francis Quicke.

Associates.

John May. | William Henry Stevenson.

J. T. Sutcliffe.

Students.

W. L. Baylay. | William Prout Whitehead.

Murray Bolton Henry. | Herbert Edward Shreeve.

Frank Lewis.

The meeting then adjourned.

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The Two Hundred and Thirty-sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 7th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 24th, 1892, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Messrs. H. B. Bourne and M. Cooper were appointed scrutineers of the ballot for new members.

The following transfer was announced as having been approved by the Council:—

From the class of Students to that of Associates—
W. J. Fryer.

The PRESIDENT: The next business is to submit the statement of accounts for the year ending 31st December, 1891, a copy of which having been sent to each of you, I propose that it be taken as read; and any remarks which members may desire to make, we shall be very glad to receive now, or any questions with reference to the details. If there is no remark, I move—"That the Balance-"Sheet for the year 1891, as just presented, be received and "adopted."

Mr. CROMPTON seconded the motion, which was carried unanimously.

The PRESIDENT: We will now resume the discussion on Mr. Reckenzaun's paper read at our last meeting.

Mr. Snell. Mr. ALBION T. SNELL: Mr. Reckenzaun's interesting paper has brought before the Institution a form of load diagram not very much studied in this country, for few of us have had much experience in traction work. It has been my lot to work in this direction more or less during the last seven years, and I am especially glad to see the subject receive the prominence it

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deserves; for I feel sure of the future of electric traction in England as well as in America. Mr. Snell.

Whether the trolley, conduit, or accumulator car is to be the most generally used is as yet open to question.

There are a few points in the paper to which I wish to refer. I would emphasise the marked difference between the accumulator-car diagram and that of the trolley system. In the former the current required for only one car is plotted, hence the relatively long periods of "no current," during which the car is either running down hill, over points, or stopping. In the trolley diagrams the current is read at the power station, and is the total quantity supplied to the line; hence the periods of "no "current" are less frequent and of shorter duration. To compare the two systems, it would be necessary to take simultaneously separate diagrams for each accumulator car, and then to add the ordinates corresponding to the same moments of time. This would be very difficult to do accurately, but an approximation would be interesting.

I have prepared a load diagram for one of the Barking accumulator cars driven by a double-reduction motor. The figures are taken from amongst a series of tests, and are not selected for any special features; but I believe them to be fairly representative of daily work. The diagram is very similar to that in Fig. 1 of Mr. Reckenzaun's paper; but the current seems to vary less, and the period during which the switch was open is only 30 per cent. of the total time of running. (The readings were taken every quarter-minute.) Apparently the road was less hilly, or else the resistance to motion was less. The energy required on the particular run was about 0·9 e.h.p.-hours per car mile. The car weighed about 9 tons.

Some reference to the accumulator service at Barking may be interesting, as the battery is the crux of the self-contained car problem.

There are four cars running daily, in round numbers, 220 miles. In ordinary weather each car has three changes of accumulators per day, so that 12 sets of batteries are in constant use, and one set is also held in reserve. The cells are the well-

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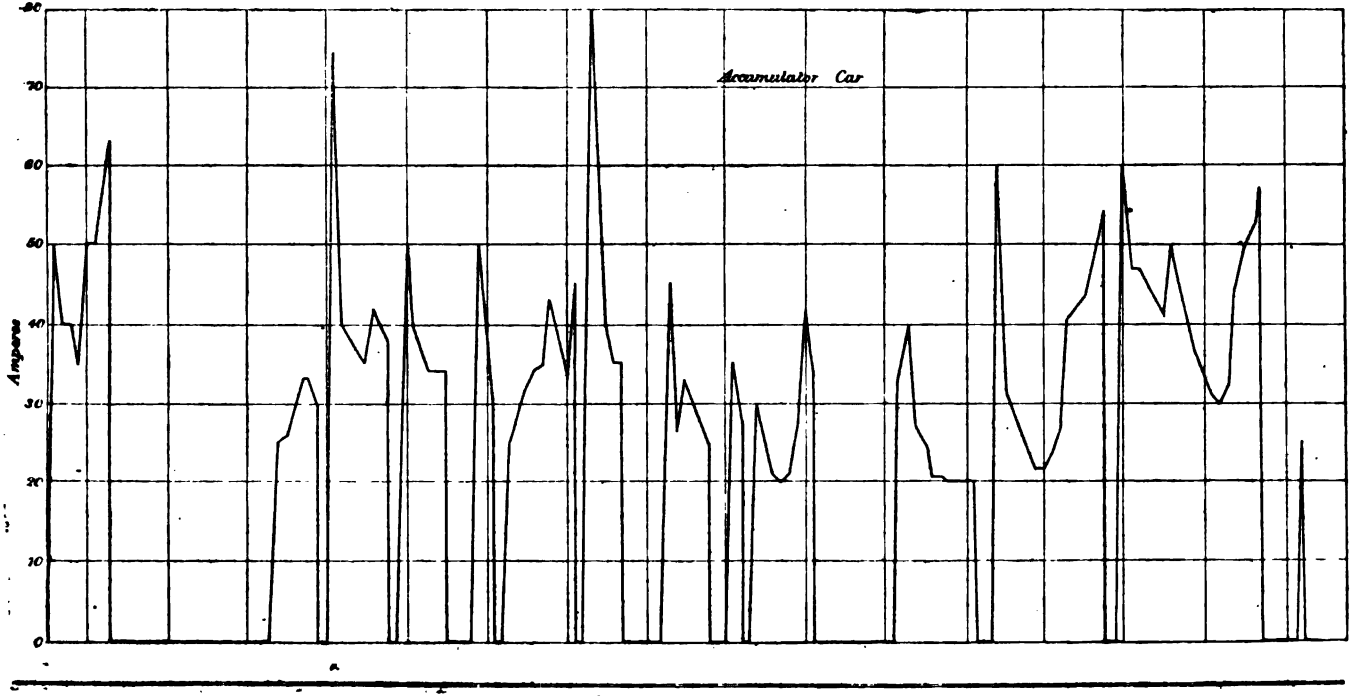

Load Diagram of Accumulator Car, single motor, with double-reduction gearing, and rolling load of about 9 tons. The time of run was 39·75 minutes; the period during which the switch was open was about 30 per cent. of this, or 12·5 minutes. The observations were made every quarter of a minute. The average speed, excluding stoppages, was about 5·5 miles per hour, and the energy given out by the accumulators approximately 0·9 e.h.p.-hours per car mile.

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known K 19 E.P.S. type. The round journey from the depôt to Canning Town and back again is just 2·35 miles, and each car averages eight journeys per day. So one set of cells, at a maximum, runs a car 18·8 miles per day in a general way. The current output per car mile averages 5·1 ampere-hours. This requires a cell capacity of 96 ampere-hours, assuming them to be discharged in series. The makers rate this type at 120, and they certainly do not overstate it. But since the accumulators are coupled in two parallels during at least one-half of the run, it is clear that the cells are not discharged to too low a point under normal conditions. In bad weather the cars have four changes of cells, so as to avoid any risk. This matter is easily arranged, as the engine is kept under steam from about 10 to 12 hours per day,

Mr. Snell.

It is by no means easy to estimate the charge delivered to the 13 sets of cells daily; but from a number of figures I average it at about 450 e.h.p.-hours, this being, if anything, rather too high than too low.

The energy supplied to the motors is still more difficult to estimate, as the coupling of the cells is continually altered from two parallels to series, and *vice versâ*. The state of the road, the number of passengers, and the fancy of the driver largely affect the running. The drivers have instructions to always start in "parallel," and to use the "series," or "high speed," only when absolutely necessary. On the whole these orders are carried out, but the personal equation upsets all uniformity of practice. From careful observations at Barking, and an extended experience of this class of work, I estimate the electrical energy delivered to the motors to average about 1 e.h.p.-hour per car mile, or a total useful output of 220 e.h.p.-hours per day.

I have stated my estimate of the dynamo output to be somewhere in the neighbourhood of 450 e.h.p.-hours. So the efficiency of the accumulators may be assumed with reasonable accuracy to be, in this case, about 50 per cent. It must be borne in mind that this figure is necessarily only an approximation; but it may be fairly called an "educated guess."

It seems to me probable that there is a liability to charge the cells more than is really required, owing to the peculiar conditions

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Mr. Snell. of working at Barking. In the first place, the engine is running at little more than half its rated I.H.P., and so there is a tendency to "racing," which, of course, means a temporary increase in the rate of charging. And, secondly, owing to the general character and newness of the work, the Electrical Power Storage Company, who are contractors for the accumulator maintenance, have very properly deemed it advisable to overcharge rather than otherwise. It would appear probable, however, that with the present type of plate an efficiency of about 60 per cent. could be realised with a large installation properly proportioned for the work expected. In such a case, assuming the combined dynamo and engine efficiency to be 77 per cent., and the aggregated efficiency of the motor, gearing, and starting device to be 60 per cent., the ratio between power at car wheels and I.H.P. of engines would be ($0{\cdot}60 \times 0{\cdot}77 \times 0{\cdot}60 = 0{\cdot}28$) 28 per cent. nearly. I believe that the trolley system, at present, does not average more than from 25 to 40 per cent., excepting on lines where the conditions are exceptionally favourable. The Electrical Power Storage Company are continually making improvements in the plates, and as they are pleased to maintain the accumulator plant at a fixed rate per car mile run, the cost of traction by self-contained cars can now be fixed with a reasonable degree of certainty.

I would like to make a few references to the use of single and double motors in tram-car work. It seems to me that in most cases a single motor is preferable, because it may be made far more efficient, and there is ample adhesion in the weight carried on one pair of wheels for all ordinary grades. If inclines of more than 5 per cent. have to be dealt with, it is advisable either to couple the four wheels or to use two motors. Since there appears, as yet, to be no simple and convenient way of coupling the wheels, the double motor is practically compulsory. It is urged sometimes, as an argument in favour of the dual arrangement, that if one motor is disabled the other can run the car home. This is true; but the proper safeguard, having due regard to commercial considerations, seems to be the next car. The combined efficiency of the double motor and gearing must be at least from 5 to 10 per cent. less than that attainable with a

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single set. But two motors have a decided field in certain cases. For instance, on a line serving between a residential district and a town. Here the motors could with advantage be coupled in series within the town boundaries, and run in parallel for high-speed service outside. Some discussion has taken place, too, with reference to single, double-reduction, and gearless motors. I have had no experience myself with gearless motors, but it seems to me that if the stoppages are not too frequent such motors should be better than geared ones. But from the accumulator point of view a double-reduction motor is advisable. In comparison with the single-reduction it weighs less, requires a smaller current at starting, and also the net efficiency is greater. All these points are of the highest importance in self-contained car traction. But if a motor be used on a light railway, like the City and Southwark line, where the stoppages, although numerous, do not occur so frequently as with a tram-car service, probably a gearless motor may be desirable; yet, judging from my own experience of motor work, I should prefer one with single-reduction gearing. After carefully weighing the results at present achieved in electric traction, I think the best all-round service will be obtained with single-reduction motors for all classes of work excepting for accumulator cars, where the double-reduction is advisable, and possibly for express service on railroads —the question now mooted in America, and which will probably be solved by some form of gearless armature.

Mr Snell.

Mr. R. W. BLACKWELL: It is scarcely possible to add to the valuable fund of information and statistics which Mr. Reckenzaun has laid before you; but, having long been connected with electric tramway construction and operation in the United States, I would like to emphasise some of the points he has made.

Mr Blackwell.

Those unfortunate pioneers of whom he spoke, repeatedly made the errors to which he has called your attention. We carefully worked out our efficiencies, loads, and strains on paper, and in practice found that we had under-estimated everything. That line of error has now been eliminated.

There can be no doubt of the immense practical advantage possessed by the double-motor truck. While, theoretically, a

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[r. lackwell.

greater economy may possibly be obtained by the use of a single motor, a separate and independent machine for each axle is the only accepted American practice where heavy work has to be done. It is possible that the overcrowded cars, heavy grades, and inferior road beds—difficulties under which many American lines labour—together with the high speeds run, and the necessity of successfully coping with snow and ice, have led us to adopt a power which may seem to you to be excessive; but I would earnestly caution you against lightening weights or decreasing power until the conditions of actual practice upon your lines have been definitely ascertained.

The great question for several years, and one that is yet undecided, is that of the most efficient and durable transmitting gear between motor and axle. The single-reduction spur gear is most approved, the gearless motor having yet to demonstrate its superiority. In this connection, I regret that Mr. Reckenzaun has not given us a comparison of the efficiencies of the present spur gear and the worm gear, which he has so long and consistently urged as the most effective method of transferring power from motor-shaft to axle. I freely acknowledge that the results practically obtained by his use of the latter form, especially as regards durability and silent running, have much impressed me, and I would not be surprised to see it take first place in the near future.

With regard to the figures that have been laid before us, I would say that too much importance should not be given to the apparent difference in the operating expenses of various roads. To secure an average figure that will apply to all cases is impossible in tramway work. Widely varying local conditions must give varying results, and of two lines, one working at twice the expense per "car mile" of the other, the one at the highest apparent cost may easily be the best paying property. The true test is to compare the working expenses of the same line when run by horse power and electric power in turn. I have no figures at hand that I can quote to you, but I know that no line in America which has seen fit to so change its motive power has had to regret its action. In every case increased

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economy and efficieney, larger dividends, and greater public convenience have resulted from such changes. The public has thoroughly supported those tramways which have introduced electricity, and has rewarded their enterprise by greatly increased patronage. Upon two branches of the Boston line the use of electric cars increased the passenger traffic by 30 and 70 per cent. respectively. Mr. Blackwell.

The question of interference with telephonic service, to which Mr. Manville referred, has been met and definitely decided in America. The telephone companies have been beaten in every case. In this connection, I would say that the use of a double set of elevated conductors for tramway service is a commercial impossibility, always inefficient, and unsightly to the last degree.

That electric traction will receive the same acceptance here that it has already gained across the water I do not doubt. While I have heard a great deal said about how much we have been ahead of you in this field, I may be permitted to say that I think you have lost little by your conservatism. We have preceded you because our needs were greater. The experience gained by the successful equipment and operation of 3,000 miles of track and 6,000 cars is at your disposal, and when you begin active work in England you can have no doubt of immediate and thorough success. The monumental scrap-heaps which we erected in the early days, the uncertainty that attended every trial,—all the innumerable vexations that fell to our lot in 1883 to 1889,—are spared you.

Our most conservative cities have been forced to accept the electric tram, even accompanied by the elevated conductor, so loudly condemned when first suggested. Boston is spending two million pounds on electric traction, and has 70 miles of track already in operation; Brooklyn has just given out contracts for a total of 1,200 motor cars; and even quiet Philadelphia has granted permission for the use of the electric cars upon its many lines. In the short space of three years the electric tramway, from being regarded as the dream of a few enthusiasts, has reached the position of a great recognised industry, with an investment of at least twenty million sterling.

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Mr. Blackwell.

It was in 1887, I think, when few of our tramway managers had ever seen an electric car, that Mr. Reckenzaun gave us "go-ahead" Americans a practical demonstration of how to boom electric traction, by appearing at the Street Railway Convention in Philadelphia to explain his views, with one of his own cars outside the door, upon which he invited the delegates to take trips about the city. I but express the feeling of American electricians in acknowledging our indebtedness to him for his years of earnest advocacy of the advantages of electric motor power, upon which subject his extensive experience so well qualifies him to speak authoritatively. To Professors Ayrton and Perry, the results of whose exhaustive researches were at our disposal long before any belief in electric traction had dawned upon the public mind,—to Professor Forbes, to whom we owe that most important practical improvement, which did away with so many vexatious difficulties, the carbon brush,—and to other members of your Institution for work done in the same field,—our best thanks are also due.

Mr. Condict.

Mr. G. H. Condict: It is with great pleasure that I take this opportunity of thanking Mr. Reckenzaun for the most admirable paper he read to us a fortnight ago. One or two points I would like to speak of. First, Mr. Reckenzaun mentions his previous suggestion of putting accumulators in with an overhead system as regulators. I think that suggestion is a most excellent one. I have been in the business for a long time, and understand what great variations of current the dynamo and engine have to overcome, and I think that if that suggestion were to be adopted in large plants it would be a great improvement.

Another point is the regulation of the speed of motors on tram-cars by various combinations of the coils into which the field magnets are subdivided. In Philadephia this system of regulation was used, and in some instances we even went so far as to divide the armature also, putting on two commutators, by means of which at one time the two halves were used in series, and at another time in parallel, giving most excellent results.

A few facts from the running of the Lehigh Avenue Railway Company's road in Philadelphia may be interesting. I was for

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nine months manager of that road, and have great reason to remember the joys and the miseries that were experienced at that time. The road is 3·15 miles in length—6·3 miles for the round trip. We had six accumulator cars, one 22 feet in body, 30 feet over all, and the others ordinary 16-foot tram-cars; and when I tell you that we often carried 80, and sometimes 100, people on the 16-foot cars—at one time as many as 115 managed to get on —you can see what was accomplished. The road was rough and hilly. About 200 feet were level, the rest grade. The grades were mostly 2, 3, and 4 per cent., with a maximum of 5 per cent. We began operating under great difficulties with regard to motors, which at first were not adapted to the work. We thought they were, but very soon found they were not, as in two weeks five out of the six cars were laid up. The 16-foot cars carried 100 accumulators, and the average current used was about 30 or 35 ampere-hours per trip of 6·3 miles, the weather and the number of passengers making the variation. Mr. Condict.

The subject of gearing has been brought before you to-night. I would like to add one or two words. Mr. Blackwell spoke about worm gearing. Our cars were equipped with worm gearing, and we had most excellent results. We know what double-reduction gearing means: you can hear the cars almost half a mile off. One of the greatest objections urged against the electric tram-car system is the noise made by the gearing; so that the question of noiseless gearing is of great importance. Tram-cars on which worm gearing is used make no more noise than horse cars. We were told we should not get any efficiency, and some engineers said that we would not be able to run at all. We made some Prony brake tests with the gearing we had in use, and found that with ordinary loads and speeds, such as employed on Lehigh Avenue, the efficiency of worm gear was from 75 to 90 per cent. We were greatly astonished and delighted at these results. In comparison with both the double-reduction and the single-reduction gear the worm wheel certainly came out very well, the efficiency being better than that of the former, and at least as good as that of the latter. The traffic on this line was very uneven. The road ran for a great portion of the way

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Mr. Condict.

through the open country, connecting two built-up portions of the city of Philadelphia, and most of the time we carried very few people; but on Sundays and holidays the tram-cars could not begin to carry the tremendous crowds. Each car weighed about 5,000 lbs., the motors and gearing about 5,000 lbs., and the batteries about 5,000 lbs.—a total of about 15,000 lbs.; 80 passengers added 12,000 lbs. more; making in all 27,000 lbs. From 50 to 55 minutes were allowed for the round trip of 6·3 miles; and while there were occasional breakdowns, and the tram-cars did not always get in on time, still the road was most successfully operated during the nine months. The total number of passengers carried during that time was about 336,000, and the total number of miles run was about 50,000.

Mr. Jarman.

Mr. A. J. JARMAN: I have listened with considerable interest to what has been said with regard to accumulator traction, and also to direct-current working. As no doubt most of you know, my name has been associated with accumulator traction for some years past, and I can only say this, after working for some considerable time with them: that they have proved themselves a success, and that they show they can pay better than horse traction—at least, on the lines on which we are working. Tramways in this country, I think, can be classed under two heads: there are those that pay, and those that do not. Those that pay do not feel disposed to alter their mode of traction, and those that do not cannot make the alteration because they have spent their money. Now it is upon some of these lines, it seems to me, there has been, and is, an opportunity for working accumulator cars, because you do not ask them to make any alteration in their outfit,—you do not alter their line. A great deal depends on the accumulator—its first cost and its lasting qualities. Respecting the number of passengers these accumulator cars are capable of carrying, I can say at the present time upon a 44-passenger car we often carry 84 passengers, and I can quite agree with the gentleman who spoke just now as to the number of passengers you can carry upon these electric cars.

There are many persons who may not have ridden on a tram-car before: they satisfy themselves as to the easy riding,

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and comfort, and speed; and once you obtain those passengers, whether from novelty or not, they most of them become regular riders; and so the earnings increase where electric cars are used. With regard to the grades on which accumulator cars can work, we often ascend at the present moment inclines of 1 foot in 16; and this has been done over and over again with 68 passengers on the car. One great thing is to have a good accumulator. Whatever may have been done in years gone by is not quite the thing to take to judge of accumulators at the present moment. After working for some months with accumulators—some of the Electrical Power Storage Company's, and some of my own design and make—I can say this: we have not yet had any trouble with any of them. One reason why, is the fact that we use a double motor. By so doing we are able to double the resistance. We reduce the current to half, although we reduce our rate to half in starting. A test of the motor with a single armature and 30 amperes gave a torque of 60 lbs.; 50 amperes, a torque of 97 lbs.; and 28 amperes, motors coupled in series, 117 lbs. That is to say, the torque is really doubled. Mr. Jarman.

This is where the strain comes upon the accumulator; this is where the plates commence to give way. But if you employ them in the way I mention, it makes all the difference in the world in starting. I did hope to hear something more from Mr. Reckenzaun upon accumulator cars. Perhaps this was not quite the subject that particularly refers to the diagrams, but I have always looked on Mr. Reckenzaun as one of the highest authorities in this country on accumulator cars. I can only say this much: you could not take one line and say that should be the standard for all others. Every line, you might say, would differ. At the present moment, upon one line for a mile the base of the groove is semicircular, and for the remaining distance it is ovoid on one side of the groove only. This makes a lot of difference to the wear of the flange of the wheel. If a line is kept thoroughly wetted, the amount of power used is about half what it is if dry. But you have to keep time. In working motors to gain time, we couple them in parallel by means of the switch. If we climb a hill, the motors work in series. This is

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Mr. Jarman. the most economical way to employ the motors for ascending inclines. But I might also mention that in working these cars by accumulators one very important point has to be considered, and that is the primary cost of working. Now we find by weight of fuel, upon days when it is wet we do not quite reach 1¼d. per mile for coke to drive the car 1 mile on the road; but when the road is dry we reach 1½d. per mile for coke consumption, or a little over that. You know tramway people have many difficulties to contend with. One gentleman some time ago, at a meeting of the Tramways Institute, discussing this matter, said: "You must understand we know nothing about "these volts and amperes. We do know how to measure up a "bushel of oats for the horses, and can tell exactly what it is "worth. What we want to know is, will it pay? If this can be "proved beyond doubt, then we should be very glad to adopt the "self-contained cars."

I hope at some future time to bring some more figures before you based on actual facts in everyday working.

The PRESIDENT: Can you tell us before sitting down exactly what has been by experience the cost per car mile carrying some definite number of passengers, including the cost of renewal of accumulators, for a period of a year or two?

Mr. JARMAN: The cars, or the accumulators, have not been running that time yet, therefore I can only give you the cost of fuel. Some of the accumulators have been running experimentally two years, and we have not renewed a single one yet. I attribute that a great deal to not using an excessive discharge in starting. You can start with ease with a small current, as well as rapidly with a large one. It is here where the destruction comes in.

Mr. Sellon. Mr. S. SELLON: Hemmed in as I am by so many electrical experts, I hesitate to criticise Mr. Reckenzaun's paper upon electrical grounds. There are, however, one or two points in the statistics he has given us which we might perhaps ask him to reconsider. One is with regard to the Blackpool line. I notice that he rather fell foul of me with regard to some remarks I made about the Blackpool, and I shall, if possible, try to answer

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him. He states that the Blackpool is at present working at some- Mr. Sellon.
thing like 57 per cent. of the gross receipts. If you look into the Blackpool balance-sheet, you will notice there are one or two items given which should not be included in the working expenses of a tramway. For instance, you find he gives a certain percentage, which he puts down here at 1·773, for the rent of the line.

Mr. RECKENZAUN: That is mentioned specially in the body of the paper, with some other items.

Mr. SELLON: That, taken as a whole, amounts to £723 a year paid to the Corporation for the rent of the line. Some little time ago, in another case, Sir George Bruce and I came to the conclusion that at least £150 should be taken off the directors' and secretarial charges at present charged to revenue, and placed to the capital account. These two sums together amount to £873, which, when deducted from the expenses of the company, reduce the working gross expenditure to 43·1 per cent. Still further in favour of the Blackpool Tramway, you must remember that for a great part of the year it is worked at a dead loss: at least for two months in the year the expenditure is over the receipts; and you will also find that during the six months from April to September the gross receipts are £5,575, and the cost of management and other expenses £1,524, being 27·33 per cent. of the gross receipts. This is an extraordinarily low item, and allowing for the proportionate deductions already referred to, the working expenditure is reduced to 25·90 per cent.

I simply put this forward as a correction to Mr. Reckenzaun's paper, to which I trust he will, if possible, give me an answer. The other remark I would make is with regard to accumulator traction, which is of interest to us tramway engineers. Some little time ago I tried, by means of making a direct charge against the accumulator companies, to get some idea of what the real working cost of the accumulator system at the present day really is. I am sorry to say, although I thought I put a very good bombshell into their midst, I got very little reply. The only bold criticism was from Mr. Carruthers Wain, whom we all know as a director of several tramways—among others, of the Birmingham Central, where the accumulator system is in use. In answer to my charge that

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Mr. Sellon. accumulators were working at anything from 100 to 200 per cent. depreciation, he made the statement that I was entirely wrong, because his cars have worked one set of accumulators for 785 discharges, or 4,000 odd miles, without renewal. Not being a mathematician, I did not take the trouble to find out by calculation what this vague statement meant; but my friend Mr. Swinburne is, and since I have been in the room he has kindly done so for me, and finds, on Mr. Wain's own figures, that the depreciation at Birmingham at the present moment is 91·72 per cent., which amount he is further good enough to add will be slightly increased during leap year.

Mr. Crompton. Mr. Crompton: I believe the chief difficulty which still exists in the practical application of accumulators as a means of driving electrical tram-cars is that of protecting them from the effects of vibration. It is only in this manner that we can account for the fact that on the whole the observed wear and tear of accumulators used in tram-cars has been so much greater than when they are used for electric lighting purposes. My own experience tends to show that accumulators that are regularly charged and discharged, such as in the case of those used for tram-cars, ought to keep in better order and cost less for repairs than those used for lighting purposes, in which the conditions of charging and discharging cannot be so regular; but in spite of this the amount of deposit in the bottom of the containing cells is notably greater in tram-car work than in electric lighting work. If I am right in this, the direction in which we can hope for improvement is in the method of protecting the cells themselves from vibration, either by mounting their containing boxes on separate springs, or by increasing the easy running of the tramway locomotive itself. It cannot be too clearly understood that although accumulators in the past have not financially been so successful as the other forms of electric traction by overhead wires, and by the slotted culvert system, yet there is no doubt they offer such peculiar advantages in allowing of the application of electric propulsion on tramways situated in crowded thoroughfares that the laws governing their economical use for this purpose deserve the most careful study and attention, as there is no reason why an electrical tramway

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system should not be worked with the two systems of locomotives, those carrying accumulators working the central and more crowded portion of the city, and those worked by overhead lines or underground conductors for the suburban and less frequented portions of the streets. Mr Crompton.

Mr. A. P. TROTTER: Electric traction seems to be made up of very important mechanical points, such as equipment of fly-wheels and governors for the steam engine, reduction gears, and the proper sections of wheels and rails, and so forth. These load diagrams seem to be peculiarly instructive, both to those who are interested in direct driving, and to those who are concerned with accumulator traction. The highest point shows those who are driving direct from dynamos, the maximum loads they must expect to carry, and the slope of the curve shows them what must be expected from the governor, and the closeness of the peaks together shows to what extent the fly-wheel will be beneficial. On the other hand, the load curves are exceedingly useful for accumulator work, as showing what actually the demand is in ampere-hours. I had occasion some time ago to make a test of accumulator traction, and obtained a curve very similar to those given in Mr. Reckenzaun's paper. Of course the readings should be taken as quickly as it is possible to write them down; it is better for one to call them out and another to put them down. The real output in ampere-hours is a very important matter in accumulator traction. I at first tried to convert all the triangles into rectangles, and smooth the whole curve down to the mean. I afterwards used a planimeter, and, though it was difficult to trace over all the peaks, the total came to about the same, making a load factor of about 30 per cent. I also traced the curve upon a sheet of thick oil paper, such as is used in the copying press. It is a singularly uniform paper, quite transparent enough to trace, and very heavy. I cut out the curve with a penknife and weighed the pieces, and obtained a result agreeing well with the others. The weight of the whole circumscribed rectangle represents a certain number of ampere-hours, and the ratio of the weights of the two pieces gives the load factor. Mr. Trotter.

Mr. P. W. WILLANS: I came down rather to listen than to say Mr. Willans.

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Mr. Willans. anything, but the problem of electric traction seems to involve some interesting steam engine questions. I should think, looking at those diagrams, that about 30 per cent. is the load factor. That must necessarily be a very wasteful way of working, and if it can be avoided, of course it would be a very good thing. In many of the technical papers in the States I see a good deal has been said about the wastefulness of compound engines for electric tramway working, as compared with single engines. There seems to be a popular impression that the compound engine is a bad engine at light loads; but this is not the case—that is, if the engines have the same final cylinder capacity. In this case the compound engine at light loads has about the same advantage as it has at full loads. But the difficulty is—assuming the governor to control the cut-off, which appears to be desirable for tramway work—the power in a simple engine—*i.e.*, the maximum power—is limited only by the power of letting the steam in for the extreme length of the stroke. In the compound engine it is limited by the power to admit the steam for the whole stroke of the H.P. piston—a very different matter. So in a simple engine you get the power due to the admission from the whole stroke; in the compound engine you must have two or three expansions at the least. The effect of this is that the compound must be a larger engine: if the same maximum power is required, and if you rely on the cylinders to start the cars from the state of rest, you must enlarge the engine. The effect of this is, that you get an engine which is necessarily more wasteful at light loads. That is the sole reason why compound engines have shown up badly in this sort of work. But I think we ought not to rely upon the cylinders entirely. Mr. Reckenzaun in his paper mentioned a case in which the governor had not "time to act." If the machine has time to run slower, the governor must have time to act. It is not that at all. The governor does not turn the engine round. The real point is, that for this starting power something more than a governor is required.

I was struck by what Mr. Graff Baker said. He could not get an engine well enough governed in this country, so he brought one over

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from America, and its most marked feature was a 12-ton fly-wheel. This is undoubtedly a move in the right direction. If economy is a consideration, the starting must be done by storage; and if accumulators will not work, it must be done by mechanical storage in some way or another. We may have a very large fly-wheel on the engine, and a governor able to admit steam the whole stroke—in other words, a simple engine. That seems to imply considerable waste, and it would seem to me desirable to employ a fly-wheel so heavy that a compound engine can be used. Of course even then a very large part of the power, while the motor is being put in motion, must be wasted in the leads, as well as what is lost necessarily in the motor itself. This loss in the leads can be, to some extent, reduced by using a running motor on the car, and starting from that by some system of friction gearing, or equivalent device of that kind. This would have the advantage that you would have a great deal of energy stored on the car, and you would not waste so much power in the leads. I have not gone sufficiently into it to know how far the practical difficulties of that plan may have weight, but there is no question that the heavy fly-wheel and compound engine is the right thing for economy. The problem is how to equalise the power excited by the engine and so avoid a low load factor, and to do this the starting power should be stored by degrees and without the sudden jumps shown on the diagrams; and this energy, after being stored, should be, as it were, transferred to the car, the engine power being increased as little as possible. Mr. Willans.

Mr. SWINBURNE: I would like those who have had special experience in tramway work to explain why such enormous motor power is needed. It seems as if the gear ordinarily used must be very inefficient. If two horses can draw a car, a two-horse motor might be expected to do so. The horses can exert a large pull at starting, so also can a two-horse-power motor. Of course I am here assuming that the car, load, and speed are the same in both cases. The old Elieson tram engine, full of batteries, with its absurd motor gear, used to draw a car up a fairly steep hill with only 10 horse-power from the cells. Mr. Swinburne.

Mr. Field told me some two years ago that he ran cars with

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Mr. Swinburne.

a two-horse motor. The motor was coupled to the axles by little coupling rods, the centres being on the same level. By this means the jar of the road is not communicated to the motor, and the coupling gear has the highest possible efficiency.

The plan which Mr. Willans advocates, of running the motor loose when the load is off, was suggested by me some nine years ago, but it is not of much use for tram-cars. It saves the energy of inertia of the motor, and nothing more. The power ordinarily spent on the extra resistances would be spent at the friction gear instead, and this is not an advantage.

There is thus a great opening for a reasonably efficient variable ratio speed-reducing gear.

Mr. Reckenzaun.

Mr. RECKENZAUN: I will take the various remarks *seriatim* as they occurred. Mr. McMahon opened the discussion a fortnight ago, telling us he found, since running with compound dynamos on the City and South London line, that he was able to keep the current within a range of 420 to 500 volts. I am very pleased to know that such was the case. When I had the opportunity of seeing the central station at Stockwell a year ago, it varied very much. With reference to the ampere readings on the cars on the underground railway, the load diagram, of course, can come out in a railway having fixed stopping places, much better than on a tramway. In the case of the underground electric railway there are not only fixed stopping places, but signals are so arranged that very seldom more than two trains need be started simultaneously. In the case of tramways a large number may often start simultaneously. The car service cannot possibly arrange itself according to any schedule time: they have to stop whenever a passenger wants to get on or off. It would have been very interesting if Mr. McMahon had brought one of the diagrams he spoke of.

I sympathise very much with Mr. Graff Baker in the difficulties on the Roundhay Tramway. He has had to go over new ground, and under new conditions. For this very reason he would have done very much better to have attached an English engineer to his American staff. Since then all that is remedied, and I only wish him success. Roundhay is a very

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popular line, and should be as successful as other similar enterprises—at least, we hope it will be. Mr. Baker seems to be under the impression that, since the average horse-power used on the line is 75, therefore 200 H.P. is very much too large. But if we take the load diagram which I have obtained from one of the officials of the Roundhay Tramway, we find the highest peak represents 160 E.H.P., which would be given off by about 180 I.H.P. This shows that his engine is not very much too large. Mr. Reckenzaun.

Mr. Dolby suggested starting the motors empty. But this does not get over the difficulty of the necessary large initial current. Some ten years ago I experimented in that direction. I had a chain gear, and upon the counter-shaft put a fly-wheel weighing about 150 lbs., so that I had not only the momentum of the armature, which was started empty, but also the fly-wheel and the gearing connected with it; and there was a friction clutch which threw the car wheels in gear. I found invariably that, directly I had put the friction clutch in, the whole motor almost stopped, and the current rose much as it would have done if I had not the fly-wheel. The momentum stored up in that fly-wheel was not sufficient to start the car, and of course we could not think of making the fly-wheel sufficiently large to do so. This may also answer the remarks of Mr. Willans.

Mr. Russell gave us some very interesting information respecting the Schevening line, and of a car weighing loaded 17 tons, and empty 14 tons. That represents a very small paying load. He also asked the weight of the American cars from which these particular diagrams were taken. Those cars were, when the experiments were made, 9 tons in weight, including passengers. The cars of the Frankfort-Offenbach line are 8½ tons in weight.

Mr. Snell's load diagram is very similar to mine. Mr. Snell found that the period of no current was about 30 per cent. of the total; in the case of my car it was about 45 per cent. of the whole. I will explain why that is so. Philadelphia, like most American cities, is built in square blocks, about 500 feet to a block. The streets are at right angles, and the tramways are at right angles. We had to cut off our current before reaching a

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Mr. Reckenzaun.

crossing, in order to be always ready to stop. The energy consumed, according to Mr. Snell's figures, is 1 h.p.-hour per car mile. It was 1·1 in the case of my figures, so there is very little difference in the energy consumed.

With reference to the question of single *versus* double motors, all American tramways, or nearly all of them, have adopted double motors, for several reasons. One is, that they have a margin: in case of one motor breaking down, the other can bring the car home. Secondly, the regulation of speed is much easier. Thirdly, it is better to propel two axles than to propel one, especially on lines with serious gradients. Now, if we couple the two axles mechanically, we find that the efficiency is lower, for this reason—that the wheels of one axle might be running partially upon the flanges, with a great deal of dirt in the grooves, and the other wheels may not do so; there is consequently a slight difference in the circumferential speed, and a great deal of friction is introduced thereby: whereas, if each axle was driven independently, each motor would exactly adjust itself to the exigencies of the track.

Mr. Blackwell paid me some nice compliments, to which I cannot very well reply; and also Mr. Condict. With regard to Mr. Sellon's remarks in reference to my having fallen foul of him at the recent meeting of the Society of Engineers, where I stated the expenses of the Blackpool line were 57 per cent. of the gross receipts, I have stated in the body of the paper, on page 301, that the results at Blackpool were very satisfactory considering that the winter traffic of the seaside resort is scarcely sufficient to cover the operating cost, and all the profits have to be earned in the holiday season. In another place I say of the rent paid to the Corporation, and the directors' and secretarial charges, that these would have to be deducted. But I did not intend to give you motive power costs only, but as many items of cost as I could possibly obtain from different lines; and anyone who wishes to know what the actual traction cost is—or, in other words, who wishes to ascertain the expenses due to wages, water, oil, waste, and coal consumed—can easily take them out and add them up separately. What I attempted to do was to give as complete a statement of each line as possible under the circumstances.

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Mr. Crompton's remarks about accumulators are very much to the point, and I quite agree with him that if accumulators were made which had active material which is not placed upon them in a mechanical way—that is, pasted or spread on them—one of the difficulties of accumulator traction would be solved; because it is quite true in my own experience of storage batteries that the pasted plates get destroyed, not so much by the chemical use they have, but by the mechanical shaking out of the paste. Mr. Reckenzaun.

With reference to Mr. Willans's remark of compound engines being suitable for electric traction, I quite agree with him; they are using a large number of compound engines in the United States, and they are using compound engines on the City and South London Electric Railway, also on Continental lines, and of course large fly-wheels are used in almost every case, in order to steady the load. Nevertheless, as you see by these peaks, the variations are enormous—the variations of E.M.F. as well as of current. I cannot understand how a motor of 2 H.P. can make a car run, much less start. As I stated in the beginning, the pioneers all made the mistake of putting too small motors on. When I first made a car I put a 4-H.P. on, and found it was not half big enough; and Americans did the same. It is a very great mistake to put motors on too small. Sometimes a car goes off the track, and it requires four or five times as much to move it then, and it is quite necessary that an electrical car should be able to carry itself back when it gets derailed.

The PRESIDENT: Would you answer the question of Mr. Swinburne: If two actual horse-power is sufficient to draw a car, why is 15 H.P. necessary in an electrical system?

Mr. RECKENZAUN: Experiments have been made by engineers with two horses, by putting a Salter's balance between the horses and the car, and it has been found that the starting effort is between 100 lbs. and 200 lbs. per ton, whereas the running effort is only about 25 lbs.; and that is the case whatever means are used—electrical or horses. With regard to the idea of starting a car slowly, it is almost impossible to do it with the usual single-reduction gear.

The PRESIDENT: We have had a very good paper, and a very

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good discussion, and I beg to propose a vote of thanks to Mr. Reckenzaun.

The motion was carried unanimously.

As the result of the ballot, the following candidates were declared to be duly elected:—

Associates.

Thomas William Andrews.
Arthur Herbert Curtis.
Malcolm John Ross.
Aymor Holloway Sanderson.
George A. Zeden.

Student.

John J. Francis O'Shaughnessy.

The meeting then adjourned.

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Telegraph Jubilee Fund—					
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General Investment Fund—					
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752 18 4	India 3½ % Stock, cost	800 0 0			
			1,275	10	0
Salomons Scholarship Fund—					
£1,000 0 0	New South Wales 3½ % Stock, cost		1,037	10	6
Furniture—					
	As per last Balance-Sheet ...	£152 0 0			
	Cost of New Purchase as above ...	118 12 7			
		£270 12 7			
	Less depreciation, say	27 1 8			
			243	11	4
Overdue Subscriptions estimated to realise			164	0	0
Stock in hand of Institution's Journals and Ronalds' Catalogue, estimated value			709	13	9
Books, Pictures, &c. (other than the Ronalds' Library),					
	As per last Balance-Sheet ...	£837 0 0			
	Add value of Books, &c., since				

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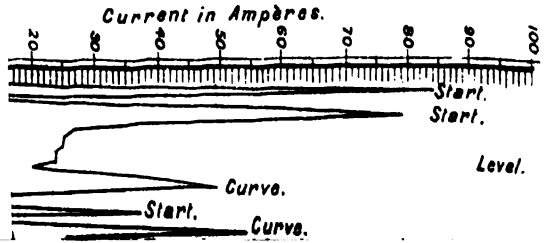

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ABSTRACTS.

ANTON ABT—THE MAGNETIC PROPERTIES OF MAGNETITE (LOADSTONE).

(*Wiedemann's Annalen, Vol.* 45, *No.* 1, *p.* 38.)

The author experimented on bars of loadstone about $4'' \times 1'' \times \frac{5}{8}''$—one, A, a fine-grained and rather porous sample; and another, B, traversed with yellowish veins and containing some particles of malachite. Sample A showed on analysis 17·5 per cent. of iron, and B 61·4 per cent. Each piece was compared with similar shaped pieces of steel, both glass hard and blue temper, no absolute measurements being given. The pieces were subjected to magnetising forces in a spiral through which the current could be varied. The author found that the permanent magnetism per c.c. of B sample was in all cases greater (up to as much as three or four times) than that of the steel, for all values of magnetising force H; the ratio decreasing as H increased. The P.M. of the A sample was also greater than that of steel; for all but low values of H, greater than that of either steel. The temporary magnetism of the magnetite, on the other hand, was always much less (say 0·1 to 0·3 of that of steel). There is reason to believe that at the maximum value of H the magnetite was very nearly saturated, but not the steels. The effect of demagnetising the various pieces was also tried; and magnetite was found to show much less hysteresis than steel. The above samples were from Moravicza, Hungary.

H. HERTZ—THE PASSAGE OF RADIATION FROM THE CATHODE THROUGH METALLIC FILMS.

(*Wiedemann's Annalen, Vol.* 45, *No.* 1, *p.* 28.)

Radiation from the cathode behaves differently from light in its power of passing through solid matter. The thinnest films of the most transparent substances are perfectly opaque to it, as also are thick metal plates; thin metallic films, however, are more transparent to it than to ordinary light. On a piece of phosphorescent uranium glass the author stuck a piece of pure gold leaf, and on the latter a few small pieces of mica. The plate was then placed in a vacuum tube with the covered side facing an aluminium cathode plate, and viewed from the other side. The tube was then slowly exhausted with the current on. While the vacuum was low, the uncovered part of the plate alone phosphoresced, the gold plate casting a shadow on it. As the exhaustion increased, the shadows cast by the leaf on the glass became less marked as the genuine cathode radiation increased, till, when the latter was at its maximum, the shadow nearly vanished. The small pieces of mica, however, cast a sharp black shadow. The same was repeated with various other metallic films, with similar results. The gold leaf acting as a mirror to the phosphorescent light, the effect on the eye of

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any phosphorescence produced behind the leaf is doubled: *e.g.*, if the leaf allows one-third of the radiation to pass, the part behind the leaf will appear, not one, but two-thirds as bright as the rest. If it allows more than half to pass, the covered part will appear *brighter* than the rest; and with a film of deposited silver the author thinks he has obtained this effect to a slight degree. If the rays pass through the pores, and not through the metal itself, they would afterwards be diffused in all directions, like light after passing through opal glass. To test this, a thick aluminium diaphragm was inserted 20 cm. in front of the cathode, and in the middle of the diaphragm was a hole a few mm. in diameter covered with a piece of thin aluminium foil. A plate of phosphorescent glass placed close behind the diaphragm showed a bright image of the hole; and as it was withdrawn further and further from it the size of the image of the hole increased in proportion, showing that the radiation through the foil was in straight lines, and not diffused.

D. DUJON—LIFE OF GLOW LAMPS

(*Lumière Electrique*, *Vol.* 43, *No.* 5, *p.* 205);

AND

FERGUSSON and CEUTER—VARIATION OF LIGHT IN GLOW LAMPS WITH CURRENT AND E.M.F.

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 7, *p.* 189.)

These are experiments carried out in the laboratories of the Cie. Popp at Paris. Four lamps of each type were tested with various E.M.F.'s, and curves plotted showing C.P., current, resistance, and watts per C.P. Curves are also given of various (unnamed) lamps showing C.P., and watts per C.P., at normal voltage for their lifetime; the readings being taken daily during the first week, then every two days, and then weekly.

The curve for current with E.M.F. varies considerably: in some types it is practically a straight line, in others considerably curved; but in all good lamps it is regular, any breaks in it showing bad manufacture. The straight current-line is a sign of a hardy lamp which will stand variations of voltage better than one with a curved current-line; the latter, however, with steady E.M.F. will often stand just as well as the former. The C.P. (from 0·8 to 1·2 of the normal voltage) may be with considerable accuracy expressed in terms of the volts by the formula, $C.P. = K(V - a)^n$, where K is a constant depending on the cold resistance, V the volts, and a and n constants, which are given for—

Edison-Swan	...	110 V	...	$a = 9{\cdot}75$	...	$n = 6$
Kohtinsky	...	105 V	...	$a = 46$	...	$n = 3{\cdot}5$
Cie. Française	...	110 V	...	$a = 9{\cdot}8$	...	$n = 5{\cdot}7$

The author believes that all *new* lamps have the same efficiency at the same state of incandescence, assuming the vacuum to be good; the only difference between lamps being their length of life, and variation of C.P. with running. Curves of C.P. at normal volts are given for three lamps throughout their life. One *rises* for the first 80 hours, and then rapidly drops, till at last it is only about half the

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original value; the curve being much like the expansion curve in an indicator diagram, watts per C.P. rising from 2·5 to 5. The rise at the beginning does not always occur. A third curve is unfortunately a rare one. The C.P. falls in a straight line with time, and the watts per C.P. only change from 2·5 to 3·6. Curves are also given showing how life and cost of running vary with watts per C.P.

Messrs. Fergusson and Ceuter's paper deals shortly with the variation of C.P. with E and C. The values of a, b, m, and n are given for certain Edison and Thomson-Houston lamps, for expressing C.P. in terms of $a\ C^m$ and $b\ E^n$. The principal point of interest is that in the modern make of lamps m and n remain approximately the same as they used to be for older types, viz., 5 and 6 respectively.

C. DIHLMANN—THE BRESLAU CENTRAL STATION.

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 1, *p.* 1.)

The station is worked on the three-wire system, with Tudor batteries capable of running 2,500 lamps (16 C.P.) for 3½ hours, the total capacity of the station being 8,000 lamps. There are three horizontal compound condensing engines, 250 H.P. each at 150 revs., built by the Gorlitzer Company, each driving two Siemens & Halske dynamos, direct coupled, on each side. The latter work up to a maximum of 175 volts and 465 amperes for charging, and 130 volts and 625 amperes when working direct on to the mains. There is no separate commutator, the current being taken off by brushes at six points on the armature itself. Armoured lead-covered cables are exclusively used, and principally laid under the foot pavement. Automatic gear is provided on the feeders to maintain constant E.M.F. at their ends by switching them on to different cells. The station was started in August with 3,000 to 4,000 lamps joined up, which number had risen to 10,000 before Christmas; 40,000 metres of cable are already laid, and 30,000 metres more will be down shortly.

F. STENGER—THE BACK E.M.F. OF THE ARC.

(*Wiedemann's Annalen*, *Vol.* 45, *No.* 1, *p.* 38.)

The author worked on the same lines as Lescher (*Ann.*, vol. xxxiii., p. 609), introducing a tangent galvanometer in the lamp circuit, with a stop preventing the needle going to the side to which the current to the lamp tended to move it, but free to turn in the opposite direction. He then short-circuited the lamp and galvanometer after the former had been running for some little time. If there were any back E.M.F., it would produce a current through the galvanometer in the opposite direction to the current from the dynamo, and the needle would give a deflection away from the stop. Negative results were obtained; there was indeed a very slight kick, but this was put down to the springiness of the needle, which had been pressed hard against the stop by the previous current. To prove that this negative result was not due to a great resistance in the heated gas of the newly extinguished arc, the experiment was repeated with five accumulators,

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opposing the dynamo current, in series with the lamp. These 10 volts were sufficient to give a very large deflection through the resistance of the extinguished arc. The author considers, therefore, that the existence of a back E.M.F. is disproved.

M. CARDANI—A SIMPLE ABSOLUTE ELECTROMETER.

(*Lumière Electrique, Vol.* 43, *No.* 6, *p.* 286.)

A flat horizontal iron plate has a circular depression in the middle, which is filled with mercury. The depression is connected by an iron tube at the bottom with a small mercury reservoir, in which, of course, the mercury stands at the same level as in the depression. The reservoir is then filled up to the top with a little distilled water, and a rubber cork inserted. Through a hole in the latter a glass tube with a very fine bore is inserted, and the water displaced by the cork rises in the tube.

The first iron plate is connected to earth, and another flat metal plate is placed above it, and joined to the E.M.F. to be measured. The surface of the mercury is attracted by the second plate and rises slightly, and the column of water in the tube falls, the fall being (omitting corrections which can be applied if necessary) proportional to the E.M.F.

C. DECHARME—EXPERIMENTS ON VARIOUS METHODS OF MAGNETISING STEEL.

(*Lumière Electrique, Vol.* 43, *No.* 4, *p.* 155.)

Given a helix of wire traversed by a current, can a bar of hard steel be more strongly magnetised by acting on it direct by the coil of wire, or by inserting a soft iron core in the latter and using it as an electro-magnet to rub the steel with? The author shows that for ordinary bars of steel the direct action with current gives a slightly better effect, say 6 to 8 per cent.; while for producing transverse magnetism the reverse was the case, and the difference more marked.

The most powerful method is that of Elias. The steel bar is made to complete the circuit of an electro-magnet, and at the same time a coil is moved backwards and forwards along it.

C. DECHARME—THE EFFECT OF LOCAL HEATING ON PERMANENT MAGNETS.

(*Lumière Electrique, Vol.* 43, *No.* 6, *p.* 258.)

Previous experimenters have dealt with the effects of high temperatures on the whole of a permanent magnet. Gaugain has shown that, if a magnet be raised to a blue tempering temperature, the magnetism first increases, and then diminishes again; and that repeatedly raising a magnet to 250 degs., and cooling it again, alters the distribution of magnetism. Both Wiedemann and Trowbridge have shown that the magnetism of a bar is lowered by diminishing the temperature below that at which it was magnetised. In the author's experiments he found that raising either end or the middle to a blue temper had no effect. His method

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of measurement was to bring the permanent magnet to a fixed position with respect to a pivoted compass needle, and count the oscillations of the latter, which in the earth's field only gave 25 oscillations per minute. A rod 22·5 cm. long and 1 mm. diameter, when its north pole was presented to the needle, caused 34 oscillations per minute. When the north pole had been heated to red, and allowed to cool slowly, the oscillations were reduced to 26. The diagram, as taken by iron filings, showed that the north pole had not been much diminished in intensity, but had, so to speak, been pushed back; the diminution in the oscillations being due to its consequent greater distance from the needle. The neutral point was also somewhat nearer the south pole than formerly. Another magnet was then tested, before heating, and after its pole had been successively raised to very dull red, to dull red, and to full red heat. The following are the results of the successive stages:—

N. pole—46, 41, 40, 38 oscillations per minute.
S. pole—48, 46, 44, 42 „ „

Another one heated to dull red in the middle showed some weakening of the poles and a slight shift of the neutral point. Heated first at one pole to red, and then at the other, a magnet loses nearly all its magnetism. If heated from the middle, the magnetism seems to shift up to the poles, and is not destroyed till the latter themselves become bright red. If heated at both poles, the magnetism rapidly diminishes, and finally vanishes. Pieces of steel magnetised transversely, and then heated at one end or in the middle, lost nearly all their magnetism in the heated part.

Dr. M. KUGEL—LOCAL ACTION IN SECONDARY BATTERY PLATES.

(*Electrotechnische Zeitschrift, Vol.* 13, *No.* 1, *p.* 8, *and No.* 2, *p.* 19.)

The author had to investigate the reason of a battery of 60 eleven-plate Tudor cells refusing to give their proper capacity, viz., 215 ampere-hours at 66 amperes; instead of this output, they dropped, after being heavily charged, to 90 volts in 40 minutes when giving 25 amperes only. The positive plates he found to be quite normal. The negatives, however, looked wrong. They gassed heavily for a long time after the charging current stopped, while on open circuit; and as soon as this gassing stopped, the volts fell suddenly to 1·5 volts: this occurred in some only after 30 to 40 hours; in most, however, after 8 to 10 hours. Negatives removed from the cells and placed in chemically pure acid and water still continued to give off gas, which was in every case pure hydrogen. The same drop when the gassing stopped was noticed when current was being taken off, but it was much sharper—from 1·8 to 0 volts in 5 to 8 seconds. The volts of these cells then remained at zero for some hours while current was being taken from the battery. The plates of a discharged cell were replaced by a new set, and the battery charged and discharged: the cell gave a good output when immediately discharged, but gassed somewhat after the charging was broken, and a second discharge was almost as bad as with the old plates. The acid of a discharged cell was replaced by new, chemically pure acid and water, without any improvement in capacity. The acid of a good cell of another battery, which had given

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177 ampere-hours, was replaced by some of the unused solution which had been used for the bad battery. The capacity of that cell fell to 32 ampere-hours. The cause of the bad results is shown to be in the acid. Analysis of this showed slight traces of nitric acid, chlorine, arsenic, and copper. The latter two are the active impurities; they reduce and deposit on the plate when charging, and promote heavy local action.

As soon as the charging current is started some lead is reduced, and local action begins almost at once; cells with these foreign metals deposited on the negatives begin, therefore, to gas when charging, much before good ones. The real capacity of the plates is not necessarily diminished, only they lose their charge while charging, almost as fast as they get it. If the foreign metals were removed the capacity might be found to be as good as ever. Again, if the plates be removed instantly after a long charge—*i.e.*, when all traces of foreign metals are deposited on the negative (they re-dissolve somewhat after standing)—the acid will be found to be perfectly right. Put the bad plates in another cell, charge for some time in the reverse direction, and the impurities will all dissolve out into the acid; when the plates, if at once replaced in their old cell, will be all right again. Now all the foreign metals which are electro-negative to lead are precipitated by sulphuretted hydrogen. This may, of course, be passed direct into the diluted impure acid. A more convenient method is to add to the dilute acid a little barium sulphide by a pipette. It should then stand for 24 hours (at the end of which time it should still smell strongly of H_2S), when it should be carefully filtered. Acid so treated is perfect for battery use. The H_2S precipitate re-dissolved and applied to a good plate starts it gassing at once: one part in a million of platinum has a marked effect. A patch on a plate was moistened with a weak solution of copper, and the plate placed in pure acid and water, without any effect. Another patch was then moistened with tungstate of soda—also without result; but when part of one patch overlapped part of the other there was at once a strong evolution of gas from the part where there were both impurities. The same effect was also noticed with copper and either arsenic, antimony, and molybdenum, but to a less degree. Dr. Kugel concludes by showing that the different plates behave differently in the same (bad) acid, and attributes this to slight differences in the physical condition of the reduced lead. An analysis is given of the impurities in some specimens of "bad" acid :—

Copper	1 part in 15,000 to 30,000
Arsenic	,, ,, 25,000 ,, 50,000
Antimony	,, ,, 25,000 ,, 50,000
Molybdenum	,, ,, 1,000,000
Platinum	,, ,, 10,000,000

A. BOUCHER—THE WATER-POWER INSTALLATION AT VALLORBES.

(*Lumière Electrique, Vol.* 43, *No.* 1, *p.* 13.)

The installation is for supplying the electro-chemical works with current; it is situated on the banks of the Orbe, just below a series of natural falls. About

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160 metres higher up the stream has been dammed by a masonry barrier, the level of the water in the dam being 70 metres above the turbines. The water is brought down partly through tunnels, and partly through iron tubes 1·2 m. diameter. There are ten 160-H.P. dynamos, each rigidly joined up to its own turbine, the two being on the same bed-plate. They run at 850 revolutions, and give 150 volts. There is no regulation of any kind; nine machines run at full load day and night the tenth being a stand-by to allow for cleaning, or in case of a breakdown. Of the latter, two only have occurred during the 18 months that the machines have been running—one when a water pipe burst and deluged a working machine, the other being due to carelessness. Of such a record the makers, MM. Thurey, may be justly proud. Besides these ten machines, there are two of 700 H.P. These are arranged with the axis vertical, and the dynamos are unipolar. The smaller machines are six-pole, and the shafts horizontal. The author claims that the annual output of the installation is the largest in the world. Although the machines are simply left to run at full power on to various series of baths, the latter keep the E.M.F. perfectly constant, so that the factory is lighted by 150-volt glow lamps direct off the dynamo leads. The whole motive power required in the works is supplied by motors.

The following is given as the cost of the installation:—

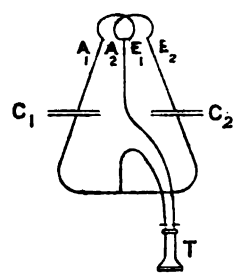
	Frs.
Land and concession	30,000
Dam	20,000
Tunnel, &c.	50,000
Pipes, and laying	30,000
Turbines, &c.	110,000
Buildings	20,000
Dynamos, &c.	300,000

or about £8 per horse-power.

A. ELSAS—A NEW METHOD OF DETERMINING SPECIFIC INDUCTIVE CAPACITY.

(*Wiedemann's Annalen, Vol.* 44, *No.* 12, *p.* 654.)

This is a bridge method with alternating currents and telephone. As source of current, the "differential inductor" previously described by the author is used (cf. *Wied. Ann.*, xlii., p. 165, 1891; and Abstracts, *J. I. E. E.*, vol. xx., No. 92, p. 257). This is an induction coil with two exactly equal and similar secondaries, and the circuit is joined up as shown in figure. A_1, E_1, and A_2, E_2 are the two circuits of the inductor; C_1, C_2, two condensers; and T is a telephone. When there is no sound in the telephone the capacities of C_1 and C_2 are equal. C_1 is of fixed capacity; it may consist of a small glass plate with tinfoil on each side, or of a small Leyden jar. C_2 has a fixed bottom plate, and a movable top one. This is raised or lowered, the capacity being thus diminished or



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increased, till silence is obtained in the telephone; when the height above the fixed one is carefully measured. A plate of the substance whose S.I.C. is to be determined is then introduced between the two plates, and the height of the plate again adjusted till silence is obtained. These two readings, with the dimensions of the dielectric introduced, are, of course, sufficient to give the S.I.C. For measurements on liquids a small brass beaker was fixed inside a larger one. In series with this was C_2, and C_1 was chosen so that its capacity was slightly less than that of the beaker condenser when the space between the beakers was filled with air; a balance was then obtained by separating the plates of C_2 (in air) till the capacity of the two in series was equal to that of C_1. The space between the beakers was then filled with the liquid, and a balance re-obtained by further separating the plates of C_2, and finally the beakers were short-circuited and a third reading obtained on C_2. Readings on oil of turpentine only are given, but observations on other liquids are promised at a future date. Several measurements on solids are tabulated, which agree well with previous determinations. Also some on celuvert: hard grey S.I.C. = 1·2; hard red, 1·4; hard black, 1·9; and flexible red, 2·7.

Prof. J. J. THOMSON—ON THE DISCHARGE OF ELECTRICITY THROUGH EXHAUSTED TUBES WITHOUT ELECTRODES.

(*Phil. Mag.*, *Vol.* 32, *Nos.* 197-98.)

The experiments were on various gases enclosed in bulbs, or in a glass tube bent four times at right angles, and having the two ends joined together so as to form a continuous tube of square shape; such a tube filled with a rarefied gas may be considered as the secondary coil of a transformer short-circuited on itself. The primary consists of one or two turns of glass tube bent to the same sized square as the secondary, and filled with mercury. The two ends of the primary are then connected to the outer coatings of two Leyden jars, whose inner coatings are joined to the two poles of a Wimshurst machine. When the gas is enclosed in a bulb, the latter is placed in a glass beaker round which a few turns of gutta-percha-covered wire are wound, and connected in the same way. When a spark passes between the poles, the electrical oscillations set up produce a large E.M.F. in the neighbourhood of the coil, sufficient, under favourable circumstances, to cause a bright discharge to take place in the tube or bulb. The gas must be perfectly dry, and the exhaustion not carried too far. To avoid static effects the coil was earthed at one point, and the secondary was separated from the primary by a screen of wet blotting paper. In this way all electrostatic effects previous to the actual discharge are screened off: when the discharge has once commenced, the author thinks that any distinction between electrostatic and dynamic forces is introduced for convenience only, and is not as a physical reality. It is sometimes difficult to get the first discharge to pass through; when it has once passed, other discharges follow with less difficulty. The effect seems due to the splitting up of the molecules by the first discharge; some of the atoms being left uncombined, and so ready to conduct the heat.

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Appearance of the Discharge.—A square tube is placed against a square primary, or a bulb inside a circular one. The bulb or tube is then connected to a mercury pump and slowly exhausted, the jars sparking all the time; when the pressure gets down to about 1 mm. a thin thread of reddish light appears, getting rapidly brighter and thicker as the exhaustion proceeds, and turning from red to white, till it becomes an exceedingly bright and well-defined ring. On continuing the exhaustion the light begins to diminish till, with a very high vacuum, no discharge passes. The pressure for maximum discharge is very much less than that for a tube with electrodes. There is never any trace of striation.

Action of a Magnet.—The luminosity being due to alternating currents, it seemed possible that its uniformity was due to the superposition of two stratified discharges in opposite directions. The two were separated by applying a magnetic force at right angles to them, attracting one and repelling the other, but each portion was perfectly uniform and unstriated. It was found, however, that such a magnetic force made it much more difficult for the discharge to pass; and, if strong enough, it would stop it altogether. A magnetic force in the direction of the discharge, on the other hand, considerably facilitated it. Professor Fitzgerald suggests to the author that this may account for the streamers from N. to S. in the *Aurora borealis*.

The bright discharge in oxygen is succeeded by a phosphorescent glow, which may last for as long as a minute. It is seen most strikingly by using a long tube with a bulb in the middle, round which the primary is wound: when a spark passes a bright ring discharge passes in the bulb, from which, as if shot out from the ring, the glow travels in both directions along the tube, slowly enough for the eye to follow its motion. The glow seems to consist of gas which has been in the path of the discharge, and whose molecules have been split up and projected from the line of discharge, and which phosphoresces as it slowly returns to its original condition by a process of chemical combination. By heating the tube the glow can be diminished or entirely suppressed. Its spectrum is continuous; no bright lines could be detected. It was only obtained in the case of oxygen, and, to a lesser extent, with common air. In the latter case the spectrum showed bright lines. The first few discharges through acetylene are a bright apple green; the subsequent ones are white: the gas is probably decomposed, as the green does not reappear. The discharge without electrodes produces vivid phosphorescence in the glass of the vessel in which the discharge takes place, or in a piece of glass tubing held as much as a foot off it. The gas in the vessel may be considered as the secondary of an induction coil, and the discharge in it shows similar properties to those of a metallic secondary: *e.g.*, there is no discharge if the tube is fused together at one place; but it may also be stopped by closing the bore of the tube at one place with a globule of mercury or other conductor. Some screening effects of one discharge on another are described.

In the figure, A C is a test tube, fused into the outer vessel, round which the primary, L M, is wound. E is an exhausted tube. When the air in the outer vessel, B, is at atmospheric pressure, there is a luminous discharge in E. When B is exhausted sufficiently for the discharge to pass in it, E becomes dark; but by continuing the exhaustion further, till no discharge passes in B, E becomes

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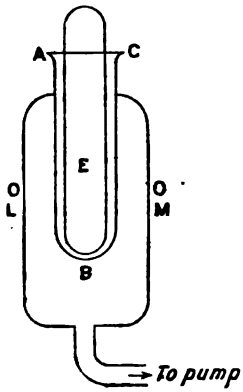
luminous again. When B is exhausted so that the discharge passes in it, the spark length can be adjusted so that on introducing a conductor or strong electrolyte into A C, the discharge in B is stopped; under certain conditions a large stick of sulphur or glass rod has the same effect. A tube of air at less than 1-10th mm. pressure has as strong an effect as a tube containing 50,000 times as many molecules of ammonium chloride. The greater the E.M.F., the greater does the ratio between the effects of the two latter become; the conduction through air does not, therefore, follow Ohm's law. This great conductivity of rarefied air is in striking contrast with the infinitesimally small values deduced for experiments with electrodes. The discharge is strongly affected by the neighbourhood of conductors connected to earth or having large capacity. Two primaries were then joined in series: in one of them, α, was placed an exhausted tube, A, and the spark length so adjusted that a small difference in the primary current made a great difference in the effect on A. A highly exhausted tube placed in the second primary, β, glowed brightly, and extinguished A; an iron bar or tube in β also extinguished A; but a brass one, or a short-circuited coil of copper wire, rather increased its brightness; a bulb filled with dilute electrolyte had little or no effect. The effect on A depends on the amount of work done on the secondary introduced into β. This is 0 if $R = 0$ or $R = \infty$, and is a maximum if $R = Lp$; R being the resistance of the secondary, L its coefficient of self-induction, and p the frequency of the primary current. The R of the weak electrolyte, the brass tube or the copper wire, was too low for much work to be done on it; if, instead of short-circuiting it, the ends of the last were joined through a variable external resistance, it could be made to nearly extinguish the A tube. An iron rod in β puts out A completely; slip a very thin platinum tube over the rod, and A lights up again. Platinum has practically the same specific resistance as iron, so it is not R in this case which makes the difference, but the magnetic properties of the iron, which are therefore shown to still exist even at this high rate of alternations. By magnetically saturating an iron core so as to greatly diminish its permeability, its effect on the A tube was very much diminished. Another curious fact is that a very thin tube of metal has more effect than a thick one; a mathematical explanation of this is given. The same apparatus was also used for comparing the conductivity of two electrolytes for rapidly alternating currents, and the ratio between them was found to be the same as for a steady current.



J. BROWN—ON THE THEORY OF ELECTROLYTIC CONDUCTION OF CLAUSIUS, AND ON SOME RECENT EVIDENCE OF THE DISSOCIATION THEORY OF ELECTROLYSIS.

(*Phil. Mag.*, *Vol.* 33, *No.* 200, *p.* 82.)

The author combats the objections of Clausius to the Grotthus hypothesis.

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Faraday's modification of which he considers to be probably near the truth; he also attacks the alternative theory which Clausius proposed. The small current which will pass through an electrolyte, say hydrochloric acid, when the E.M.F. is below that required to effect decomposition may, he considers, be conducted by decomposition of impurities, hydrates, or complex molecules, whose parts have but minute attractions for each other; or even that H Cl may itself be decomposed and the effect masked by the effect of dissolved oxygen or other depolariser. An experiment which Prof. Ostwald considers as proving the existence of free electrically charged atoms of chlorine and potassium in potassium chloride, the author holds as of a purely negative kind; as also another of Prof. Ostwald's to show that a semi-permeable wall interposed between two electrolytes in the path of a current will permit certain ions to pass, but not others. This he attacks experimentally, and shows to be explicable in other ways.

Prof. A. GRAY—THE COILS OF ABSOLUTE ELECTRO-DYNAMOMETERS.

(*Phil. Mag.*, *Vol.* 33, *No.* 200, *p.* 62.)

It is shown mathematically that (under the subjoined conditions) the couple θ turning the suspended coil in an absolute dynamometer is for unit current,

$$\theta = -8\pi^2 n n' a^2 \frac{x}{r} \xi,$$

where n, n' are the turns on the two coils;
a radius of moving coil;
2ξ length of moving coil;
a radius of fixed coil;
$2x$ length of fixed coil.

If $2\xi = \sqrt{8}\,a$, and $2x = \sqrt{8}\,a$, then for coils wound in a single layer, and perfectly even, the above is true to 0·02 per cent. if a is $> 2a/3$, or to 0·004 per cent. if a is $> a/2$.

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LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Journals during the Months of MARCH and APRIL, 1892.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

— Hrabowski—Lantern for Indoor Arc Lighting.—*E. T. Z.*, vol. 13, No. 12, p. 149.

F. Geraldy—Electric Lighting of Grand Hotel, Paris.—*Lum. El.*, vol. 43, No. 10, p. 451.

Anon.—Table of the Heaviest Day's Output for various German Stations, 1886–1891.—*E. T. Z.*, vol. 13, No. 10, p. 128.

J. P. Anney—The Embrun Water-Power Installation (I.).—*Lum. El.*, vol. 44, No. 17, p. 151.

J. Sahulka—The Strength of Field in Two-Phase Motors.—*E. T. Z.*, vol. 13, No. 10, p. 119, No. 11, p. 135.

R. Ulbricht—Alternating-Current Networks.—*E. T. Z.*, vol. 13, No. 12, p. 147.

G. Richard—Details of Dynamo Construction (S. I.). (Ferranti, Lahmeyer, Pyke & Harris, Poleschko.)—*Lum. El.*, vol. 43, Nos. 12 and 13, and vol. 44, No. 17.

G. Pellisier—Windmills as Sources of Power.—*Lum. El.*, vol. 44, No. 15, p. 65.

G. Richard—Electric Lifts (S. I.). (Herdman, Leonard, Neuburger, and Otis.) —*Lum. El.*, vol 44, No. 15, p. 57.

— Rechniewski—The Radiation of Heat from Dynamos.—*Bull. Soc. Int.*, vol. 9, No. 86, p. 92; *Lum. El.*, vol. 43, No. 11, p. 539.

— Thury—Automatic Regulator (I.).—*Lum. El.*, vol. 44, No. 15, p. 84.

MAGNETISM.

T. Moureaux—Note on the Magnetic Disturbances of March, 1892.—*C. R.*, vol. 114, No. 10, p. 564, No. 11, p. 616.

C. Fromme—Magnetic Investigations.—*Wied. Ann.*, vol. 45, No. 4, p. 798.

J. Trowbridge—Wave Propagation of Magnetism.—*Phil. Mag.*, vol. 33, No. 203, p. 374.

TELEGRAPHS AND TELEPHONES.

Anon.—Telegraphic Statistics for 1890 (finished).—*Jour. Tel.*, vol. 16, No. 4.

Complete List of the Submarine Cables of the World, with Lengths, Dates of Laying, &c.—*Jour. Tel.*, vol. 16, No. 4.

W. Œsterreich—Multiple Telephone Commutator (I.).—*Lum. El.*, vol. 44, No. 14, p. 17.

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FURTADO-ANDERS—Synchronised Telephone (I.).—*Lum. El.*, vol. 43, No. 13, p. 620.

K. STRECKER—Protection of Telephone Lines from Induction.—*E. T. Z.*, vol. 13, No. 10, p. 128; *Lum. El.*, vol. 44, No. 16, p. 122, No. 17, p. 181.

J. ANIZAN—The Measurement of Capacity on Telephone Lines.—*Lum. El.*, vol. 43, No. 13, p. 601.

G. CARRÉ—The Telephone in Military Operations.—*Lum. El.*, vol. 44, No. 16, p. 117.

C. RAVEAU—New Scientific Applications of the Telephone (S. I.). (Wien, Waitz Elsas, Palaz.)—*Lum. El.*, vol. 43, Nos. 12 and 13.

Commandant HENRY—The Cryptophone (Buried Microphone).—*Lum. El.*, vol. 44, No. 14, p. 13.

ELECTRIC CHEMISTRY AND METALLURGY.

Dr. G. GORE—Chemical Equilibrium in Mixed Electrolytes.—*Phil. Mag.*, vol. 33, No. 203, p. 342.

— KELLNER—Bleaching Process.—*Lum. El.*, vol. 43, No. 11, p. 526, vol. 44, No. 15, p. 75.

— HŒPFNER—Electrolytic Extraction of Silver and Copper.—*Lum. El.*, vol. 43, No. 12, p. 572.

— MARX—Electrolytic Apparatus.—*Lum. El.*, vol. 43, No. 10, p. 481.

INSTRUMENTS AND MEASUREMENTS.

Dr. FEUSSNER—New Alloys for Standard Resistances.—*E. T. Z.*, 19 Feb., 1892; *Lum. El.*, vol. 43, No. 10, p. 485.

Prof. W. WEILER—A Lecture Apparatus for Alternating Currents and Drehstrom. —*E. T. Z.*, vol. 13, No. 11, p. 139.

— ABRAHAM—Standard Air Condenser.—*C. R.*, vol. 114, No. 12, p. 654; *Lum. El.*, vol. 44, No. 16, p. 139.

H. BAGARD—On a Thermo-electric Standard of E.M.F.—*Jour. de Phys.*, March, 1892, p. 128.

W. HIBBERT—On a Permanent Magnetic Field.—*Phil. Mag.*, vol. 33, No. 203, p. 307.

J. LEFÈVRE—On the Attraction of Two Plates Separated by a Dielectric.— *C. R.*, vol. 114, No. 14, p. 834.

— LINDECK—The E.M.F. of the Clark Cell.—*Zeits. Inst.*, Jan., 1892; *Lum. El.*, vol. 44, No. 14, p. 37.

— DUCRETET—"Tesla" Apparatus.—*Lum. El.*, vol. 44, No. 16, p. 122.

STATIC ELECTRICITY.

CH. ANDRÉ—Negative Electrification of the Atmosphere in Fine Weather.—*C. R.*, vol. 114, No. 12, p. 659; *Lum. El.*, vol. 44, No. 15, p. 91.

— ZENGER—Reproduction of the Solar Prominences, &c., by Electric Discharges (I.).—*Lum. El.*, vol. 43, No. 11, p. 501.

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THEORY.

— Poincaré—Report on M. Blondlot's Memoir on the Propagation of Oscillations.—*C. R.*, vol. 114, No. 12, p. 645; *Lum. El.*, vol. 15, No. 15, p. 89.

L. Arons—Lecture Experiment with Electrical Oscillations.—*Wied. Ann.*, vol. 45, No. 3, p. 553.

Lucas and Garrett—A Detector for Spark in Hertz's Resonator.—*Phil. Mag.*, vol. 33, No. 202, p. 299.

K. Ångström—On the Intensity of the Radiation of Gas under the Influence of the Electrical Discharge.—*Phil. Mag.*, vol. 33, No. 203, p. 387.

— Cohn—The Propagation of Electric Vibrations in Water.—*Wied. Ann.*, vol. 45; *Lum. El.*, vol. 43, No. 13, p. 627.

Hutin and Leblanc—The Second Law of Thermo-Dynamics with reference to the Tesla Experiments.—*Lum. El.*, vol. 43, No. 10, p. 451.

E. Bouty—On the Co-existence of Specific Inductive Capacity with Electrolytic Conductivity.—*C. R.*, vol. 114, No. 10, p. 533.

A. Berget—Electro-capillary Phenomena.—*C. R.*, vol. 114, No. 10, p. 531; *Lum. El.*, vol. 44, No. 14, p. 34, No. 15, p. 92.

G. Meyer—Theory of the Capillary Electrometer.—*Wied. Ann.*, vol. 45, No. 3, p. 508.

F. Exner—Experiments on Theoretical E.M.F. of Batteries.—*Wiener Monatshefte*, No. 12, p. 216, 1891; *Lum. El.*, vol. 44, No. 16, p. 143.

Riecke and Voigt—The Peizo-electrical Constants of Quartz and Tourmaline.—*Wied. Ann.*, vol. 45, No. 3, p. 523, No. 4, p. 759.

RAILWAY WORK.

O. Saal—The Telephone for Railway Working.—*E. T. Z.*, vol. 13, No. 10, p. 125.

F. Geraldy—The Paris Electrical Tramways.—*Bull. Soc. Int.*, vol. 9, No. 86, p. 102.

G. Richard—Electric Rail and Tramways (S. I.). (Sergent, Nuttall, Reed, Munsie, Dewey, Sissach-Gelterkinden.)—*Lum. El.*, vol. 43, No. 11, p. 519.

— Kolle—The Project for the Berlin Underground Railway.—*Lum. El.*, vol. 43, No. 10, p. 472.

VARIOUS.

E. Branly—A New Unipolar Conductivity Property of Gas.—*C. R.*, vol. 114, No. 14, p. 831.

— Ascoli—The Relation between E.M.F. and Specific Heat.—*Rend. Lincei*, 1891, p. 397; *Lum. El.*, vol. 43, No. 11, p. 545.

G. Pellissier—The Physiological Effects of Alternating Currents.—*Lum. El.*, vol. 44, No. 16, p. 103.

P. Mottelay—History of Electricity, &c. (S.) (continued—1753-66-75).—*Lum. El.*, vol. 43, No. 11, p. 518, No. 13, p. 613.

Koch and Wüllner—Polarisation of Small Electrodes.—*Wied. Ann.*, vol. 45, No. 3, p. 475.

E. Englisch—The Resistance of a Bismuth-Lead Amalgam.—*Wied. Ann.*, vol. 45, No. 3, p. 591.

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JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXI. 1892. No. 99.

The Two Hundred and Thirty-seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 28th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on April 7th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

William Bathurst. | F. H. Francis.

Walter B. Lock.

Mr. J. S. Fairfax and Mr. E. B. Vignoles were appointed scrutineers of the ballot for new members.

The PRESIDENT: I will now ask Mr. Trotter to read his paper.

Mr. TROTTER: The Council of the Institution of Civil Engineers, to whose generosity we are indebted for the use of this hall, have recently decided to lay on the electric current here from the mains of the Westminster Electric Supply Company; but owing to several reasons, one being the sudden illness and

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regrettable death of our Member Mr. Charles Hortsek, it has been found impossible to complete the arrangements by this evening. The Secretary of the Institution of Civil Engineers very properly objected to any temporary work, and consequently I must ask your indulgence for the absence of those experiments with which I had proposed to illustrate my paper. It is not quite complete enough to be called a paper, and I have therefore given it a less pretentious title.

NOTES ON THE LIGHT OF THE ELECTRIC ARC.

By ALEXANDER PELHAM TROTTER, B.A., Member.

It is apparent to intelligent observers, and well known to all electric light engineers, that the light of an arc lamp is not emitted uniformly in all directions. Photometrical measurements of arcs are generally expressed by a polar curve, the length of the radius vector representing the candle-power. Although a good deal of work has been spent on this subject, the real meaning of the shape of these curves has not attracted much attention. They all exhibit the same characteristics, and it is easy to notice two distinct types of variation. One variation is found with arcs employing a large current, and consists in the emission of a small quantity of light in a direction above the horizon, the curve rising a little above the horizontal axis. Another type of variation is the narrowing of the whole curve, and the concentration of a large proportion of the light at an angle of about 40 degrees or 50 degrees with the vertical.

A rather complicated treatment of the subject has been made by M. Rousseau* with the object of finding a formula for the distribution of the light. His treatment appears to be empirical, and, having arrived at a formula, he does not appear to have recognised the practical meaning which it contains.

It has been assumed by many persons that the hollowing of the crater of the positive carbon tends in some unexplained manner to concentrate and throw the light downwards. It is

* *La Lumière Electrique*, vol. xxxvii., p. 415.

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evident that the lower, or negative, carbon intercepts a good deal of the light; but there speculation appears to have stopped. A little consideration will show that the effect is precisely and identically the same as though the end of the positive carbon were flat. No tilting of an incandescent or other luminous surface can make it brighter; and, on the other hand, if it is covered with a thin, imperfectly transparent layer, as in the case of the atmosphere of the sun, the edge will appear less bright than the middle of the disc. The quantity of light emitted by an incandescent disc in any direction is proportional to the amount of surface visible from that direction. That is to say, candle-power varies, then, as the cosine of the inclination.

Cosines plotted as a polar curve give a circle passing through the pole. This theorem is not to be found in mathematical works, being much too easy and simple for those students who have got so far as polar curves. The candle-power of the crater of an arc lamp should, then, if plotted as a polar curve, coincide with part of a circle. Any deviation from the circle must have some cause. Two such deviations are observed, and their causes are easily recognised.

An ideal continuous-current arc—in fact, any good one with first-rate carbons—has a uniform horizontal crater, and this gives no light in a horizontal direction, though a little may come from the hot sides of the carbon, especially if it is carrying a rather large current. No brightly incandescent surface is seen. The pointed negative carbon is seen in profile, and some light is emitted by it. From what Professor S. P. Thompson and others have told us of the physics of the arc, it is very probable that the seat of the dissipation of energy is primarily at the surface of the crater. The negative carbon only becomes hot by being cooked in front of the crater. It is wasted, probably, by mere combustion; and even this waste is reduced by the deposition, under some circumstances, of carbon transferred from the positive pole. The waste of the positive carbon is undoubtedly due to volatilisation. Professor J. J. Thomson and others have shown that electrolysis is not necessarily confined to bodies in the liquid state, and it seems probable that the volatilisation of

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carbon at the crater, and its deposition, under certain conditions, on the negative carbon, is closely allied to electrolysis, the positive pole behaving as an anode, both in its wasting and in the fall of volts at its surface. The absorption of energy appears to be twofold. A certain quantity of heat is required to produce the change of state from solid to vapour, and a certain difference of potential must be required to produce the electrolysis.

In one of the very interesting articles by M. Palaz* in which he abstracts and compiles the work of numerous Continental writers, he states, without giving his authority, that 85 per cent. of the light of an arc is emitted by the positive carbon, 10 per cent. by the negative carbon, and 5 per cent. by the flame of the arc. It seems probable that in most cases the proportion of light emitted by the positive carbon is even greater than this. The word "arc" will be used in this paper as an abbreviation for arc lamp, and will not be used to denote the flame which plays between the carbons. The word "crater" will be used to denote that portion of the positive carbon which is at the highest degree of incandescence. This portion is generally well defined, the colour being uniform. The word "crater" will not be used to signify a hollowing of the carbon.

During the Antwerp Exhibition in 1885, M. Wybauw† made a number of photometric measurements of arcs by different makers. The full curve in Fig. 1 represents the mean of a large number of observations made, no less than 26 different arcs having been tested. The cosine of 60 degrees being one-half, the area of the crater seen from this direction is one-half of that of the full circle; the candle-power is one-half of that emitted by the crater; and the length of the radius vector corresponding to 60 degrees may be taken as the radius of the circle. The light due to the negative carbon is clearly shown as an excess above the circular curve; there is, indeed, nothing else to which it can be due, except the red-hot walls of the crater.

At about 60 degrees the curve of candle-power begins to fall

* *La Lumière Electrique*, vol. xxxvii., p. 410.

† *La Lumière Electrique*, vol. xxxvii., p. 414, and vol. xxvi., p. 58.

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off, and this is due to nothing else than the shadow of the lower carbon, which intercepts more and more of the light as we pass to smaller angles, until, if the carbons be of the same diameter, no light is thrown in a vertical direction.

In considering the real meaning of the latter part of the curve, I drew a number of views of a pair of imaginary carbons, projected at different angles. The elliptical area of the crater in each view was calculated, and I found that these areas, plotted as radii of a polar curve, gave a curve closely resembling the well-known candle-power curve of the arc. It follows that, if this be proved to be true by experiment, the candle-power per square millimetre of the crater is constant.

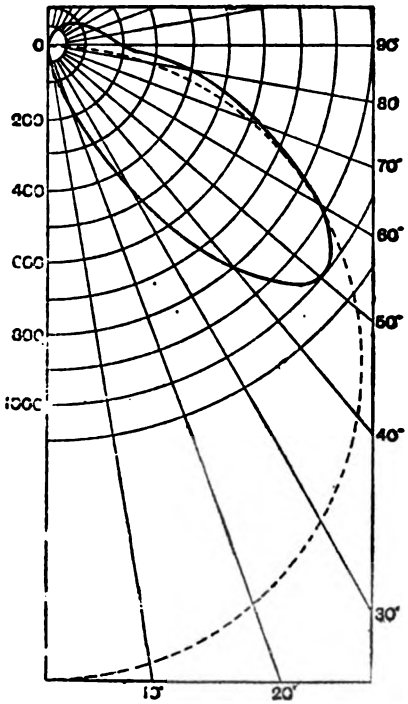

FIG. 1.

I communicated this result to Professor S. P. Thompson, and asked if he would see whether actual experiment would confirm it.

A series of very interesting experiments have been carried out at Finsbury Technical College by Mr. C. F. Higgins, senior student. A "Planet" lamp, taking 8 or 9 amperes, was first employed, and the first experiments showed that the light was undoubtedly proportional to the area of the crater. In order to carry out the experiments with greater accuracy, a larger lamp was required, and Messrs. Johnson & Phillips kindly lent a Brockie-Pell lamp, taking 25 amperes. The following apparatus (Fig. 2) was constructed by Mr. Higgins at the workshops of

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Finsbury College, for the purpose of projecting an image of the arc:—

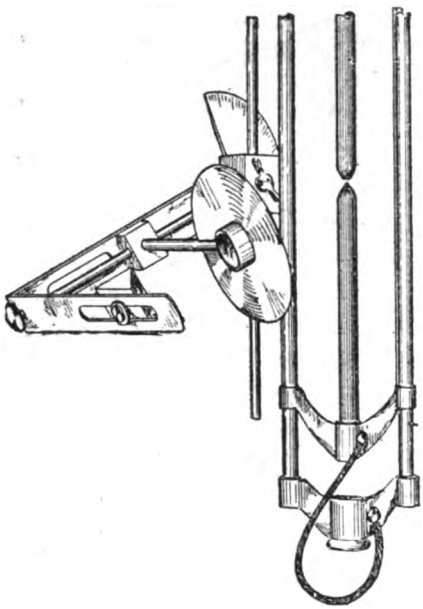

Fig. 2.

An arm adjustable on a horizontal axis, and provided with a clamping nut and graduated arc, carries a lens and a mirror set at an angle of 45 degrees, with the direction of the ray of light falling on it, and reflecting the ray in a direction parallel with the axis of the radial arm. An image of the arc may thus be projected on a screen. As the radial arm is moved the image turns round, but this does not interfere with the observations. An Ayrton-Perry photometer, slightly modified, was used to measure the light with a standard candle, and this was done by removing the projecting lens, and allowing the reflected beam to fall on the mirror of the photometer. When the lens was replaced, for the purpose of projecting the image, the mirror of the photometer was removed, the rest of the photometer remaining undisturbed. The image was received on a sheet of drawing paper, and was amplified about 14 times. The outline of the incandescent crater was traced in pencil, and in some cases an outline of the carbons was drawn. The diameter of the carbons formed a standard for absolute measurements. Readings were taken at every 10 degrees inclination. The area of the tracings was measured by a planimeter.

In the first experiments the absorption of the two mirrors was neglected, and the results are not, therefore, to be taken as candle-power. The mirror on the radial arm was of platinised glass, since an ordinary mirror gave a double image. The

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reflecting power was not good, though the image was clear. The areas were also measured to an arbitrary scale at first. Figs. 3 and 4 give polar curves of two sets of observations. The

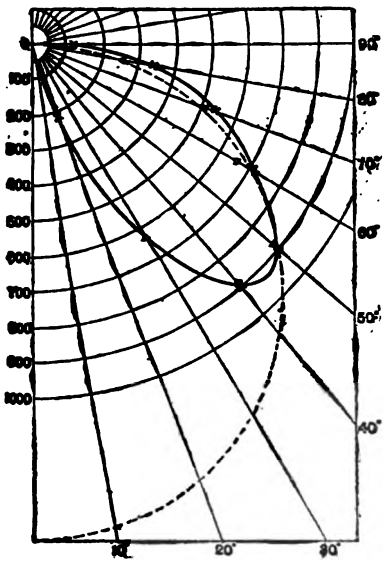

FIG. 3.

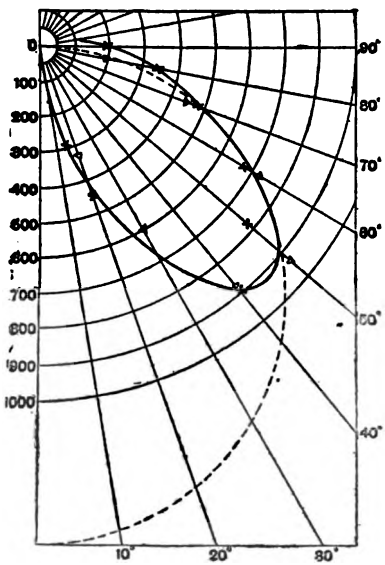

FIG. 4.

Curve of Areas of Crater and Candle-Power of Short Arc. Δ = area, and × = candle-power. The scale of radii is arbitrary.

photometer readings are represented by circles, and the areas by triangles. A general coincidence of both sets of observations with the well-known curve is evident. Figs. 5 and 6 are reproductions of the tracings.

It is rather difficult in many cases to estimate the relation between two polar curves by mere inspection, partly perhaps because they are so seldom used in practice. Two different curves may be drawn, the one through the photometer readings, and the other through the areas of crater; but, owing to the difficulty of arriving at accurate results, on account of changes in the length of the arc, which greatly affect the inner part of the polar curve, and the occasional indistinctness of outline of the crater, the errors of either set of readings are probably as great as their departure from the curve drawn freely among the two sets.

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FIG. 5.—Tracings of Short Arc. 1½ full size.

FIG. 6.—Tracings of Short Arc. 1½ full size.

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Useful as it is to plot as a polar curve observations which relate to measurements taken at different angular directions, the relation between the two sets of readings may be examined more easily when they are plotted with rectangular co-ordinates (Fig. 7). A straight line cutting the axis at 100 C.P. seems

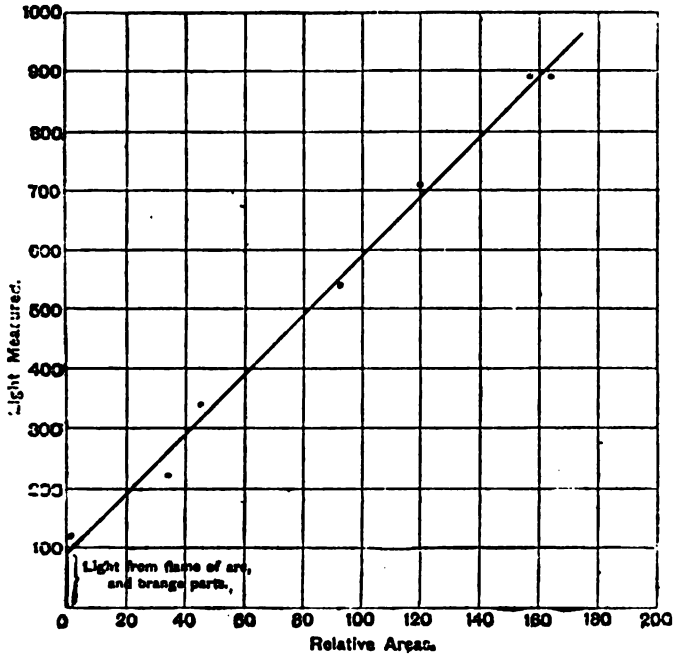

FIG. 7.

to fit the results. This may be explained by the light which is emitted by the red-hot and glowing parts of the carbon. These were not included in the measurement of area; the true crater only was measured. I have been unable to carry out any complete experiments on the photometry of arcs; the present paper is only a collection of notes on the subject, which is treated qualitatively, and not quantitatively. The measurements, for various reasons, were relative, and attempts were made on one occasion only to take direct readings in candle-power. The image of the crater at 60 degrees was projected and traced, and measured by a planimeter. The mean of several fairly concordant readings was 20·8 square inches. The diameter of the

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image of the carbon was 17 inches. Its actual diameter was 0·6 inch. The image was therefore magnified 28·3 times. The real area of the crater was 0·025 square inch. The candle-power readings were taken at the same angle, and immediately after the tracing of the image in each case, being measured directly from the arc without reflection. They were fairly concordant, and gave a mean of 1,065 candles. At the maximum position (about 45 degrees) this would give about 1,400 candles. It follows, therefore, that the crater gave 42,600 C.P. per square inch, or 64 C.P. per square millimetre. The amperes were 26, and volts 51.

It is as impossible to raise carbon above the degree of incandescence of the crater of the arc as it is to raise water above boiling point or ice above melting point.* No substance has yet been suggested for "improving" arc lamp carbons which is less volatile than carbon. Even the core of cored carbons, while it serves a very useful purpose in steadying the arc, is less brilliant than the rest of the crater. If a lower temperature than the normal incandescence be found at the crater, it is because the positive carbon is too large for the current. Under these circumstances the arc generally flickers irregularly, and a highly incandescent patch travels about over the surface of the crater.

The large amount of light intercepted by the negative carbon raises the question, What becomes of it? It is evident that it strikes the lower carbon, and is probably converted into heat; but since only about 10 per cent. of the whole radiation of an arc is in the form of light,† most of the "cooking" of the lower carbon is done by mere heat.

It is not difficult to reconstruct the shape of the carbons which must have given rise to the curve given in Fig. 1, but to do so is nothing more than a geometrical exercise, and leads to no useful result. A similar relation obtains between the candle-power curve of an alternating-current arc and the shape of its carbons, but the use of alternating arcs is not common: the light is not,

* S. P. Thompson, Society of Arts, March 6, 1889, *Electrician*, vol. xxii., p. 534.

† Prof. Marks, American Inst. Electrical Engineers, vol. vi.; *The Electrician*, vol. xxv., p. 150, *et seq.*

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for outdoor purposes, thrown in a useful direction; it is difficult to prevent the arc from flickering round the carbons, and they give so much trouble from other causes that they are seldom worth using.

Two other kinds of arc are the lighthouse and the search light. A very elaborate set of experiments was carried out in 1878 at Chatham, under the direction of the Royal Engineers Committee, by Major R. Y. Armstrong, Lieutenant G. Bowker, Lieutenant P. Cardew, Lieutenant L. Darwin, Lieutenant G. A. Carr, assisted by Lieutenant R. White and Captain Abney. Ten dynamos, six lamps, six projectors, and five kinds of carbons were tested; photometric measurements were made, and photographs were taken both facing the crater and at right angles to it. From the photographs the area of the crater could be determined. The candle-power per square inch seemed to vary in an indefinite way between 76,000 and 25,000 candles per square inch.

The photographs taken from the side give a very good idea of the position of the carbons and the small obstruction of the negative carbon when a search light is properly arranged. Finding that so complete an examination of search lights has been made, I have not proceeded with the experiments which I had intended to carry out on this kind of lamp. I had some difficulty in finding this report, which was intended for Government use only. I was allowed by Major R. Ruck to examine a copy at the Horse Guards, and on making application through him to the Inspector-General of Fortifications the report was allowed to be made public. Instead of attempting to abstract this very interesting research, I have presented the copy to the Library of the Institution.

By permission of Mr. Inglis, secretary to the Trinity House, I was allowed to examine the working of the St. Catherine's Point Lighthouse at the beginning of the present month, and to have the machinery run during the daytime. The magneto machines and lamps are the same that were used at the South Foreland in 1884 and 1885. Alternating currents from 180 to 300 amperes are used; the volts at the lamp are only about 35 to 38. Sir James Douglass's fluted

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carbons, of 50 mm. and 60 mm. diameter, are used, the smaller size being employed during clear weather. Fig. 8 gives sections of these carbons full size; they have a graphite core. The conditions are altogether different from the continuous-current arc which has been described. It was found during the South Foreland experiments that a short arc gives more light than a long one. It must be remembered that the horizontal light is the most useful, although in a large lantern a considerable angle is utilised by the lenses. A large alternate-current arc is very unsteady: the flame burns away the carbon in a very irregular manner; and when round carbons were used a hollow crater was formed at the end of each.

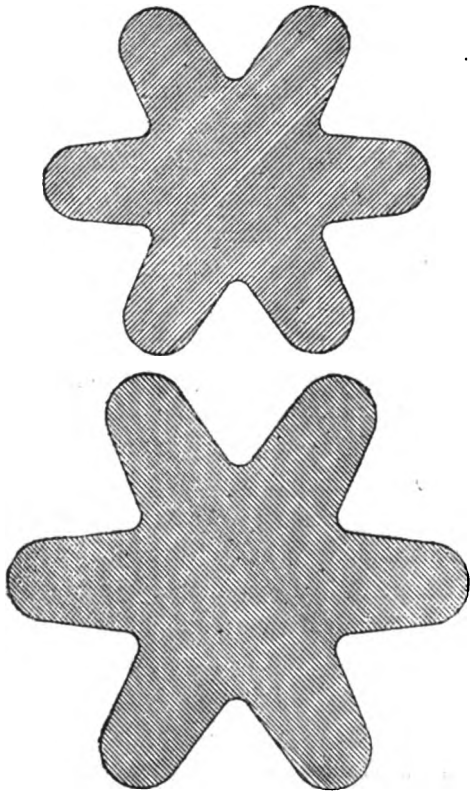

FIG. 8.—Section of Sir James Douglass's Fluted Carbons, 50 mm. and 60 mm. Full size.

The walls of the crater would give way, and cause great variation in the light. By the use of fluted carbons little, if any, hollowing takes place, and the light is more uniformly emitted. It seems probable that, since each carbon is only at a high state of incandescence during half a period, a short arc gives more light because the carbons keep each other warm. The same amount of radiant energy may be given off with a long arc and a short one, but when the chilling of the negative is reduced a greater proportion of the radiation will be in the form of light. With a long alternating arc the flame wanders round, and is blown violently sideways, and the crater therefore tends to be shifted to one

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side of the carbon. The distance between the carbons is only 1-16th inch to 1-8th inch, and as they are by no means flat it is the exception that any interval can be seen between them.

I understood that an optical apparatus was provided for projecting the image of the arc on the side of the lantern, for the

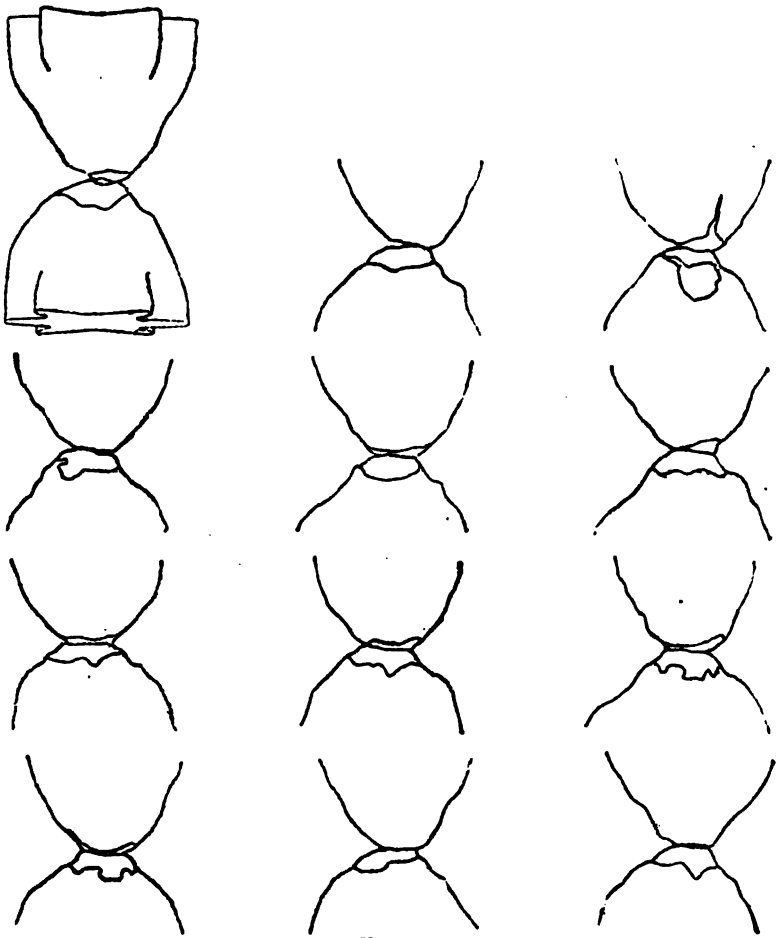

FIG. 9.

purpose of keeping the carbon points in the focal plane of the lenses. I found, however, that no such instrument was in use at St. Catherine's Point, but a very satisfactory image was thrown by the object glass of a telescope. The distances were arranged so that the image was full size. The shape changed so quickly that

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it was impossible to draw the image with great accuracy, and attention was paid only to the outline of the carbons and of the crater. The outlines in Fig. 9 are reproduced from a series of tracings made at intervals of half a minute. Fig. 10 is an inte-

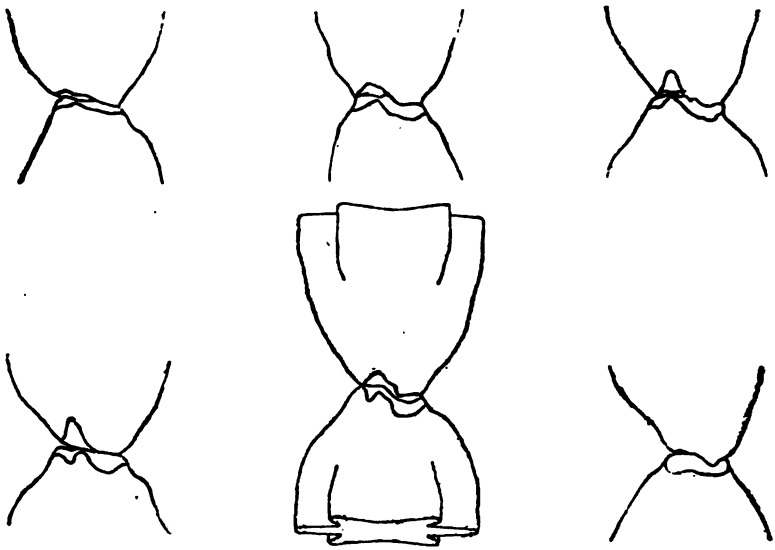

Fig. 10.

resting set, showing the gradual development and disappearance of an irregular interlocking of the two carbons. This does not appear to have any connection with the flutings of the carbons, and I have noticed it on several occasions, both at the light-house at the recent Naval Exhibition and at St. Catherine's Point. A glance at these tracings shows that only a very small portion of the true light-giving surface is visible, owing to the shortness of the arc. Fig. 11 is a pair of tracings showing the most regular and the most irregular form of crater.

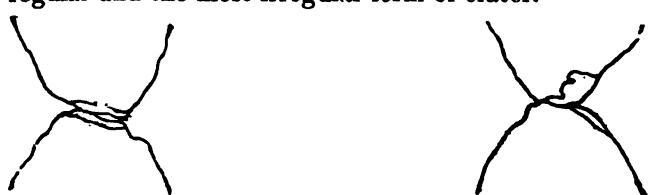

Fig. 11.

The areas of the crater on the tracings were measured by a

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planimeter, the tracing point being taken ten times round. The mean area of Fig. 9 is 0·2087 square inch, and the mean of Fig. 10 is 0·146 square inch. The interlocking form of crater appears, therefore, to give about 30 per cent. less light. The candle-power for 240 amperes, according to the measurements made at the South Foreland in 1884 and 1885, is about 16,000 C.P., giving about 75,000 candles per square inch if the light were entirely due to the crater. The area of the yellow- and orange-coloured parts of the carbons was very difficult to estimate, but it was very much greater in proportion to the true crater than in a continuous-current lamp taking 10 to 20 amperes. Taking these parts as exposing about four times the area of the crater, and giving one-eighth of the light per square inch, the crater alone may be taken as giving two-thirds of the whole, or about 50,000 candles to the square inch. This agrees fairly well with the result already given for the measurements at Finsbury.

It was found during the South Foreland experiment that 40 mm. cylindrical carbons became red-hot throughout their whole length when 300 amperes were passing. The fluted carbons, measuring 60 mm. over, have a sectional area equal to that of a cylinder about 46 mm. diameter—that is, 2¼ square inches; but the cooling surface is about 50 per cent. greater. Larger carbons would probably burn yet more irregularly, and yet it seems very desirable to increase the light during foggy weather. If the ends of the carbons could be maintained conical, all the light-giving surface would be usefully employed. A carbon which is more and more refractory towards the centre suggests itself; but even at present the consumption is from 1½ to 2½ inches per hour, and modification can only be made in the direction of less refractory material. The flame might be made to spin round the carbons under the influence of a magnetic field; but the simplest plan seems to be the revolution, or gyration, of the carbons about a common axis, their centres being slightly displaced. In order to obviate sliding contacts, a gyratory motion would be the best. The power of the St. Catherine's Point Lighthouse has been called 6,000,000 to 7,000,000 C.P. This would require an area of crater of about 1 square foot.

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The following measurements of red and green light from different sources have been made by Professor L. Weber, of Breslau,* taking incandescent platinum as unity. Taking the Violle platinum unit at 18·5 standard candles, I have converted them into candles per square inch and candles per square centimetre, and have added six other sets, with white light only, from my own observations.

	C.P. per Sq. In.		C.P. per Sq. Cm.	
	Red.	Green.	Red.	Green.
Platinum (Violle standard) ...	120	120	18·5	18·5
Sun's disc	487,000	1,000,000	75,500	155,000
Sky, near sun	120	120	18·5	18·5
Albo carbon on edge	73·5	60·7	11·4	9·4
White paper, horizontal, exposed to summer sky, noon	16·5	35·2	2·56	5·45
White paper, sun 60° high, paper facing sun	8·25	17·2	1·28	2·67
Albo carbon flat	10·5	8·7	1·63	1·35
Argand	6·8	5·24	1·05	0·82
Black velvet, summer sky, noon	0·0833	0·07	0·0052	0·0109
White paper, reading without straining	0·0018	0·0024	0·00028	0·00037
	White.		White.	
Sperm candle	2		0·31	
Moon, 35° above horizon	2		0·31	
,, high	3		0·46	
Batswing (whole flame)	2·25		0·35	
Methven standard	4·3		0·666	
Crater of electric arc	45,000		7,000	

The foregoing considerations about continuous-current arcs point to the value of a long arc and a small and, if possible, a pointed negative carbon; but these must without hesitation be sacrificed to perfect steadiness. For outdoor work the uniformity of distribution is not much affected by the shadow of the lower carbon; indeed, the latter may perhaps be advantageous, for there is a fair amount of stray light immediately beneath an arc lamp;

* *Journal für Gasbeleuchtung, Electrician*, vol. xxv., p. 404.

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and if the lower carbon *could* be dispensed with, there would be an overpowering illumination, which would make street lighting much more irregular than at present, unless very high posts were used. But for railway stations and for such places as the British Museum reading room, some improvements might be made in this direction. Experiments have been made with a carbon rod surrounded by a carbon tube, arranged pointing downwards, and the arc has been made to travel round by magnetic rotation. The rod should be positive, as the end surface of the tube would be too large to keep incandescent, unless with a very large current. The Jamin candle, a modification of the Jablochkoff, and the Rapieff and Hedges lamps, are examples of efforts in this direction.

Several attempts have been made to improve the arc by adding volatile substances, or by introducing gas through a hollow carbon. The most successful of these appears to have been the Saunderson method of using hydrocarbon by means of a wick, but nothing has been heard of this for some time. The only good effect that can be expected is the production of a long arc which will reduce the shadow of the lower carbon; and it is likely that the temperature of the crater will be reduced by the presence of any substance less volatile than the best carbon.

With a scientific but misguided regard for truth, the candle-power of arc lamps has been reduced to its "mean spherical" value by many authorities. An easy way to arrive at this is to cut out a piece of card to the shape of a candle-power diagram, such as Figs. 1, 2, or 3. By balancing this on a pencil, on a line parallel with the perpendicular axis, the distance of the centre of gravity from the axis may be found. The area may be measured with a planimeter, or by comparing the weight of the card with that of the rectangle out of which it was cut, or by treating the curve as being composed of half an ellipse and part of a parabola.* Then, measuring the distance of the centre of gravity from the axis, and multiplying this by 2 π times the area, the solid contents of the figure of

* Area of ellipse = minor axis × major axis × $\pi/4$. Area of parabola = base × $\frac{1}{3}$ height.

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revolution of the curve about its axis are obtained. The mean spherical candle-power of the arc is equal to the radius of a sphere having the same solid contents. As so little light is emitted above the horizontal, it would seem quite as useful to take the mean hemispherical candle-power. But we not only learn nothing fresh, but are likely to be misled, for a light giving the same mean spherical candle-power would not be nearly so useful as an arc. The mean spherical candle-power has been found by M. Rousseau by plotting to rectangular co-ordinates.* But the method is not much easier, and it seems better to retain the polar curve, which has a geometrical meaning.

Since there is little or no difference in the light-giving properties of arc lamp carbons, the various qualities differing mainly in homogeneity and rate of burning, and since there must be a definite relation between the watts expended and the area of the crater, or the candle-power, it seems preferable to denominate the size of arc lamps by the number of watts expended. There is no reason why the maximum candle-power at the best angle should not be given, if the candle-power be mentioned at all; but the carbons should be very carefully centred, and the test should be made either with several photometers simultaneously, or with a lamp revolving like a meat jack. The nominal 2,000 candle-power which 10-ampere arc lamps are sometimes supposed to give, is a perfectly unjustifiable convention, and has been very properly characterised as "a fine old crusted lie."

It might be imagined that, since an ordinary opal globe surrounding an arc lamp does not appear to differ greatly from a uniformly luminous sphere, the candle-power at different angles would be very much more uniform. This, however, is not the case. Fig. 12 has been derived from the measurements of the illumination

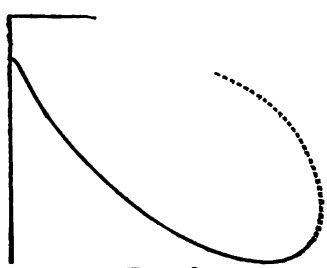

FIG. 12.

* Eric Gerard, "Leçons sur l'Électricite," vol. ii., p. 298.

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on the surface of the street by an arc lamp in Cornhill, an opal globe being used; the dotted line is assumed. Fig. 13 is a pair

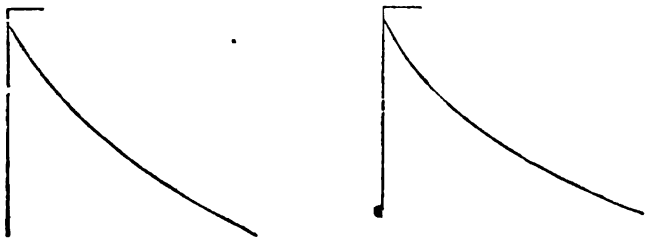

FIG. 13.

of similar curves, derived from measurements made in Queen Victoria Street with arc lamps enclosed in moulded glass.

Owing to the difficulty of comparing the light from an arc lamp with the light of a candle, on account of the difference of colour, it has been a common practice to make photometric measurements with red and with green glasses, and elaborate tests have been made by Professor E. L. Nichols and others throughout the whole range of the visible spectrum. The red and green glasses allow rather more definite readings to be made; but by a little practice with a photometer, which allows very free and rapid changes to be made by the oscillation of a lever or a handle, very much greater accuracy may be obtained than with an instrument in which a screen or a lens has to be moved on a slide until a balance is effected. But the green and red measurements having been taken, no one has yet suggested what is to be done with them; neither the mean, the sum, nor the product has any physical meaning, and the two readings are generally given side by side. For purely scientific work, where the light is treated as radiant energy, either the whole spectrum should be measured and compared with a standard, or the luminous rays should be carefully separated from the dark heat rays, and the radiation measured as a whole. Such measurements have been made by H. Nakano and by Louis B. Marks,* and form a very valuable contribution to our knowledge of the efficiency of the arc.

* American Institute of Electrical Engineers, May 21, 1890, *Electrician*, vol. xxv., p. 150.

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It has been shown by M. A. Crova* that the portion of the spectrum lying near $\lambda = 582$ gives a true measure of the total candle-power of a light. Professor E. L. Nichols† finds $\lambda = 600$ to be the position. For a considerable range of temperature the ratio between the intensities of this part of the spectrum is practically identical with the ratio of the candle-power of two lights.

The use of the green and red measurements, besides facilitating the work of the photometrist, is to compare, in a rough way, the whiteness of the light under observation with a standard light. The measurements of M. de Nerville‡ afford some information as to the colour of the arc lamps at the Hippodrome, the glow lamps at the Opera House, and the gas lighting at the Post Office in Paris. But this information can be obtained only in the arbitrary terms of the ratio of the red reading to that taken with the green, when these two are equal, and the ratio is unity. This was the case with the Edison lamps at the Opera House, but it tells us nothing. A complete luminosity curve for each glass throughout the spectrum would be required to explain these readings; but even then the resultant luminosity curve could not be derived. Some idea of the quality of the light thus measured may be determined by comparing the ratio of the red and green components of some standard light. Such a comparison may be effected by measurements made by M. de Nerville on the illumination produced by bright sunlight shining on a window with white blinds. The ratio of green to red in this particular photometer was about 3·5 to 1 for sunlight at four o'clock in the afternoon on May 1, 1890.

Captain Abney has made many investigations on the colour of daylight, and a chapter is devoted to this in his popular book on colour measurement and mixture. In searching for a standard light of uniform colour he found nothing to equal that which is given from the crater of an arc. This is very satisfactory to electrical engineers, but it gives no definite information as to

* Electrical Congress, Paris, 1889.

† American Institute of Electrical Engineers, 1890, *Electrician*, vol. xxiv., p. 225.

‡ Société Internationale des Electriciens.

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the real quality or degree of whiteness of the light. The popular idea, and one which is very firmly fixed, is that arc lamps give a bluish light, sometimes rather violet; that it shows up certain colours in a sickly or unnatural manner; and there is a widespread belief that it not only acts injuriously on the complexion, but pierces cosmetic embellishments. These ideas are of so serious a nature that I venture to discuss the quality of the light of the electric arc; since, although it is not a strictly electrical matter, it has a rather important aspect, in so far as it is a more or less unpopular feature of electric lighting.

The only standard of pure white light which we have, is that of the diffused light of a summer day. Artists are familiar with the fact that direct sunlight is yellowish,* even on the clearest days; and that a blue sky, on the other hand, gives distinctly blue light; and that both of these must be avoided, or a picture painted under such lights will appear too cold or too warm when hung in a diffused light. Captain Abney and Dr. Russell have found that at different seasons of the year and at different times of the day very considerable changes occur in the colour of sunlight, owing to the absorption of bluish light by the atmosphere. It does not, however, appear impracticable to me to fix on a fairly typical degree of whiteness, such as that of chalk, whiting, or alumina exposed to diffused light in summer time. A luminosity curve of the spectrum of such light would be a standard. Compared with such a white, or, indeed, by a very rough comparison with ordinary daylight in clear weather, the arc gives a distinctly pale primrose light, rather warmer than straw-yellow, and, to my eye, distinctly yellowish, in spite of the pale violet flame which with inferior carbons occasionally flares up at irregular intervals. It is no use telling an unscientific person that the light is not blue, but pale yellow; he will answer, "It appears to

* Dr. W. J. Russell and Capt. Abney, on the other hand, say: "The light from "a sky which is cloudy has very much the same composition as sunlight itself, as "we have repeatedly proved." "The proportion of blue rays in sunlight near "mid-day in May is very nearly the same as the standard light" ("the white-hot "crater in the positive carbon pole of the electric light").—"Report on the "Action of Light on Water Colours," c. 5453, 1888, pp. 23 and 69.

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" me to be blue or violet, and, therefore, to me, and to 99 people " out of a hundred, it *is* blue or violet."

Three reasons can be brought forward to explain this optical illusion. After dusk we are accustomed to the use of strongly yellow and even orange-coloured light of gas, lamps, candles, or glow lamps, and our own idea of whiteness is lowered. The whitest thing that we can see is a sheet of white paper, which is, of course, no whiter than the yellow light of the lamps; we imagine that it *is* white, and thus a false standard of white is obtained. When a really whiter, though not perfectly white, light is introduced, it naturally seems to be bluish compared with the false standard of white. Whether our idea of white really becomes altered is a psychological question which it would be out of place to discuss in the present paper. Secondly, after dusk, the blue-seeing nervous elements of the eye (on the Young-Helmholtz theory) are allowed to rest, while most of the work falls upon the red- and green-seeing elements. These latter become wearied, while the former are in a more highly receptive condition than during the day. As soon as a light containing a tolerable proportion of blue light is seen, it naturally appears to be more blue than white. The third suggestion is based upon a result of Captain Abney's researches, which show that for the most feeble illuminations no colour can be distinguished, and that the first colour which can be discovered is blue. He finds, and is supported in this by Lord Rayleigh, that the faintest light appears of a greyish-green colour; and Captain Abney attributes the apparent colour of moonlight to this cause. I do not attach much importance to the application of this phenomenon as an explanation of the colour of the light of arcs, but it is worthy of record in this connection. When the eye passes gradually from daylight into electric light without seeing any distinctly yellow light, the sensation of blueness is rarely, if ever, observed.

It would be easy to humour this optical illusion, as is done at the British Museum reading room, by the use of screens of yellowish glass; but a much more important consideration is the value of the arc light for the proper discrimination of colours;

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and though it is found in dye works that most colours can be matched with a good 15- or 20-ampere arc, there appears to be no reason why a perfect reproduction of a standard daylight quality of whiteness should not be obtained—at all events, with arc lamps, if not with glow lamps. The public would not be ready to believe it: they would call such a light, bright blue; but that does not matter much. As the idea did not occur to me until the autumn of last year, I have been unable to make experiments with summer daylight; but as it would be a misfortune if it were patented, I take the present opportunity of describing the principle.

The full line in Fig. 14 is the luminosity curve for the light

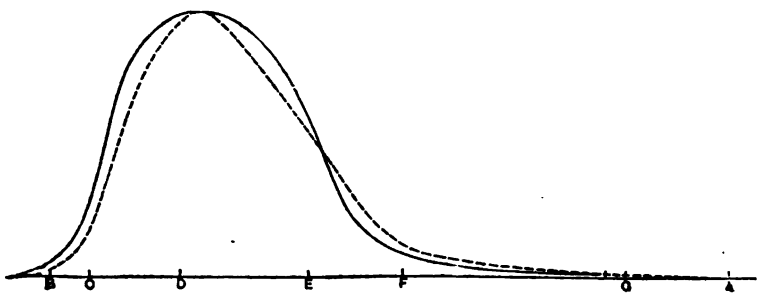

FIG. 14.—Arc and Sun.

of the positive pole of an arc as determined by Captain Abney. The horizontal scale is the length of the spectrum, and the letters refer to the well-known line. The ordinates are a measure of the brightness of each part of the spectrum. The dotted line is the luminosity curve for sunlight in May, from the same authority. Fig. 15 shows the difference between these two curves.

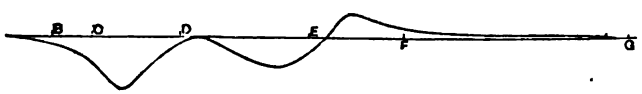

FIG. 15.—Arc and Sun—Difference.

The portions which lie below the horizontal line represent the excess of orange and greenish rays in the arc, and the portion which lies above the line represents the deficiency in blue rays. Such a deficiency has the same effect on the eye as an excess of yellow, and the whole effect is distinctly more yellow than

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sunlight. The light of a glow lamp at ordinary incandescence is almost exactly the same as that of gas. Fig. 16 gives the

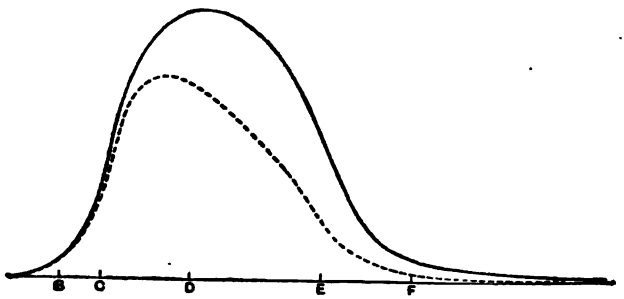

FIG. 16.—Arc and Gas.

luminosity curves of arc and gas; and Fig. 17 shows the difference

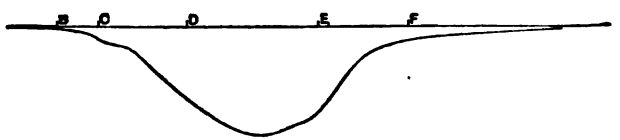

FIG. 17.—Arc and Gas—Difference.

which, when the curve of gaslight is reduced so as to fall wholly within the arc curve, as in Fig. 16, consists of a deficiency which increases rapidly towards the blue.

Assuming ideal daylight to be rather less yellow—that is, more blue than sunlight—the dotted curves in Fig. 18 have been

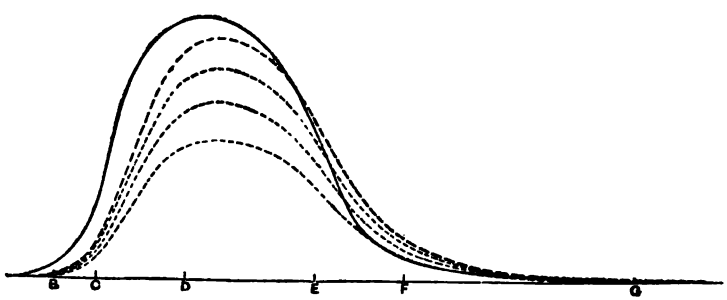

FIG. 18.—Arc and Daylight of different Strengths.

drawn from Captain Abney's curves of sunlight and the light of blue sky. One-third of the difference between sunlight and blue sky has been added to the sunlight. The dotted curves are of

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the same kind, but the ordinates have been reduced. Fig. 19

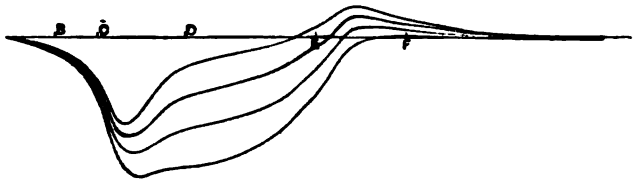

FIG. 19.—Arc and Daylight—Differences.

shows the differences. Compared with the strongest daylight curve, the arc has a marked excess of orange, and a deficiency of greenish blue. With weaker daylight there is a greater excess of yellow; and with the weakest light shown, since it lies almost entirely within the arc curve, there is practically no deficiency in the blue.

There are two different ways in which this result may be practically used to produce artificial daylight. A glass or other medium may be tinted with stains, or dyes, which will absorb the proper amount of yellow and red light, and the lamp may be surrounded by such a medium; or a reflecting screen may be painted with such a colour that this yellow light may be absorbed, and the white light alone may be reflected. Blue glass chimneys are used for microscope work, and are occasionally used for reading lamps with the view of giving a more agreeable light. The lowest curve but one in Fig. 19 has a small deficiency of greenish blue: this would affect the eye as a very faint yellowish tinge. If the excess of yellow in the rest of the spectrum could be corrected, a very close approximation to daylight would be attained. About one-third of the useful power of the light would have to be sacrificed. With gaslight or glow lamps more than two-thirds of the light would have to be absorbed in order to reproduce the effect of daylight.

Quite apart from considerations of colour-matching, the use of a white light ought to be good for the eyes, since all the nervous elements would be equally excited as in normal daylight. I have not yet completed my experiments on coloured shades, but have produced colour screens which, when illuminated by lamplight, reflect a light which cannot be distinguished from white when compared with daylight. It does not follow that the

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light is identical with daylight; but I hope to follow up the matter during the summer.

Mr. Swinburne.

Mr. J. SWINBURNE: I must congratulate Mr. Trotter on bringing forward a paper on a subject that has not before been dealt with in anything like the same exhaustive manner. The importance of studying arc lamps in a really scientific way cannot be over-estimated. The efficiency and output of an arc lamp are, as a matter of fact, even more important than those of the generators supplying it, as electrical energy costs more at the lamp than at the station.

The obvious fact, first pointed out by Dr. S. P. Thompson, that the crater is at the temperature of volatilisation of carbon, appears to me to give us a standard of white light. Kirchhoff and others have shown that the colour of light from incandescent bodies depends on the temperature only, and not on the emissivity or any other quality of the radiating surface; that is to say, at any given temperature the proportion of rays of different wave lengths is the same, though the light emitted per square centimetre depends on the emissivity as well as the temperature. If the arc is at a definite temperature, it thus gives light of the same colour always. There is probably little difference in the emissivities of different samples of arc carbon, as they are, doubtless, all about equally black. The light per square centimetre of crater will therefore also be constant. A standard of light can therefore be constructed as follows. A search light, or other good lamp, would be used. A shade, with a pinhole of known dimensions, would be placed in front of it, throwing an inverted image of the arc on a screen placed a certain distance from the pinhole shade. This screen would thus have an image of the arc of the standard colour, with the standard illumination. A hole of standard size is cut in this screen, and the shade is adjusted so that this hole falls well within the uniform part of the image. A standard quantity of light then passes through this hole. For photometric work the screen would be replaced by the photometer screen itself; the image of the arc being so large that the whole of the screen would be well within it. Such a standard of light would be used for reference

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only—that is to say, as a primary standard. It has many advantages. It can be easily reproduced, without any complicated measurements; and it affords a definite standard both of quantity and colour, the colour being approximately that of daylight; as the spectrum of carbon is continuous, it can also be taken as a standard of light of any colour as well as of white light. Mr. Swinburne

I wish Mr. Trotter had given us more information about alternating-current arcs. The lamps can be more easily steadied, and can be run off parallel circuits more economically, as choking coils or constant-current transformers can be used instead of wasteful resistance to secure the proper relations between the current and pressure. I hope those who have had extensive experience of alternating-arc lamps will tell us something more about them.

Mr. Trotter refers to lamps with gas passed into the arc. I tried this in 1885. Gas was passed through a hollow carbon. It merely made the arc unsteady, or put it out. Mr. Saunderson, however, is said to have been more successful.

There is some discrepancy in the light of the sun and of the arc, as given by Mr. Trotter. They appear to be of the same colour, therefore the sun must be at the boiling point of carbon. The sun, however, gives ten times as much light per square centimetre, so it must have ten times the emissivity of carbon; that is to say, it must be ten times as black, which is incredible.

Major-General FESTING: I agree with the writer of the paper in his view that the apparent blueness of the light of the arc lamp is due to the effect of contrasts, and in support of this I may remark that the effect of an arc lamp running in full daylight is that its light appears distinctly yellow; on the other hand, if the shutters of a ballroom be opened in the early morning, the daylight admitted will appear of a very ghastly hue to those who have been dancing all night, although to one who has had a good night's rest early morning light does not appear so. Major-General Festing.

I do not consider that there can be any abstract standard of whiteness of light. The diffused daylight to which we are accustomed is what we call white light; and if this were altered

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Major-General Festing.

by any change—for instance, in the absorption by the atmosphere—we should very soon get to consider the thus altered light as white.

Absorption of the blue rays by the atmosphere has been mentioned in the discussion. I venture to point out that in ordinary states of the atmosphere there is very little actual *absorption* of the blue rays, but that they are *scattered* by small particles suspended in the atmosphere. Lord Rayleigh has investigated this matter, and has propounded a formula which has been found by Captain Abney to agree closely with the results of his experiments. The consequence of this scattering is that the sky appears blue, and the direct sunlight more or less yellow. In confirmation of this view, I may mention the case of a photograph taken with a low sun shining through a London fog, in which, although the sun was focused directly on to the plate, it had made no impression, yet there was sufficient scattered light to give a picture of houses, trees, &c.

Dr. Sumpner.

Dr. SUMPNER: Mr. Trotter has brought before us a great number of interesting points. Perhaps one of the best is his explanation of the shape of the characteristic curve of the arc light. According to Mr. Trotter, the incandescent surface of the carbon gives out light in any direction proportional to the apparent area of the surface seen from that direction, so that the arc acts like any other bright surface. Something has just occurred to me which confirms this view. There is a well-known formula for finding the mean spherical candle-power of an arc lamp from the measurement of the horizontal candle-power and the maximum candle-power. The formula is this: The mean spherical candle-power, S, is equal to half the horizontal candle-power, H, plus a quarter of the maximum candle-power, M. Thus,

$$S = \frac{H}{2} + \frac{M}{4}.$$

The mean spherical candle-power of an arc lamp is not perhaps of great importance, but it has a very definite meaning; and if we try to account for this formula, we are led to the explanation Mr. Trotter has brought forward. This formula can be explained by assuming that the characteristic curve is made up of two

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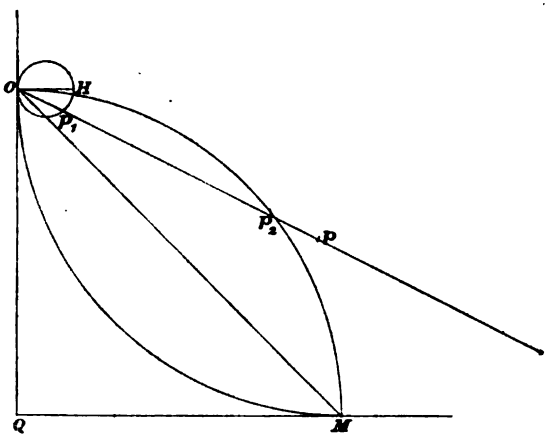
parts, the first part being a circle of the diameter equal to H, and the other part of the curve being composed, as Mr. Trotter suggested, of a semicircle cut off by the shadow cast by the lower carbon. Assume the shadow caused by the lower carbon to cut off the lower half of the semicircle, so that the characteristic curve consists of two quadrants, O M, as in the figure, Dr. Sumpner.



where O denotes the position of the arc light, O M represents M, the maximum candle-power of the arc, and O H represents H, the horizontal candle-power. The resultant characteristic curve will be such that the radius vector, O P, is equal to the sum of the two corresponding radii, O P_1 and O P_2, of the two component curves, and will be very like those which Mr. Trotter has sketched. The mean spherical candle-power of this resultant curve will come out to be half the horizontal intensity, plus the quarter of the maximum intensity, in accordance with the formula. The physical meaning seems to be that the light is due to two causes. The circle O H is due to the carbon stick being more or less incandesced round the surface, owing to the heating of the carbon; and the other curve, O M, is due to the upper carbon, considered as a bright horizontal disc, the light of which is cut off more and more as the vertical direction is approached by the shadow of the lower carbon. From tests which have been published on various arc lights, I have found by calculation that the formula may be relied upon to give results correct to

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Dr Sumpner. 4 or 5 per cent., and the explanation of this formula appears to strongly support Mr. Trotter's contention.

The PRESIDENT: Mr. Swinburne has referred to alternating-current arc lamps. It is an important question whether they do cause so much trouble as not to be worth using. Perhaps some member would like to speak on that subject.

Mr. Mordey Mr. W. M. MORDEY: I have not had the opportunity of studying the paper beforehand, but I think Mr. Trotter has given us a very carefully considered paper, and that he has given the true explanation of what Dr. Sumpner has called the "characteristic" of the arc, and has thrown a good deal of light on the subject. I should like to question Mr. Trotter on one or two points where my experience does not quite agree with his. He states that a good arc has a uniform horizontal crater. One arc, of course, is as good as another; it is simply a matter of current and carbon. With the same carbon you have the same arc. I am not speaking now of the regulation of the arc. I have never come across a uniform horizontal crater; but have always found craters of arcs, where the carbons were not out of proportion to the current, quite concave. I hope Mr. Trotter will explain what he means by the goodness of the arc to give the horizontal effect. No doubt the horizontal effect is better than a concave crater if you can get it; but I do not know how to get rid of the concavity except by a flaming arc. Then, further on, it is stated that in the case of cored carbons the cored part is less brilliant than the rest of the crater, and I gather that Mr. Trotter intended us to understand that the core was made of something other than carbon. I believe many makers now use in the cores certain substances not carbon; but a good many carbons are made with simply soft graphite for cores, and in such carbons the arc is steadied, but there is no difference in the density, in the colour, in the degree of light over the crater. No doubt with impure cores, made with foreign matter, the core would be cooler. With regard to alternate-current arcs, I should have been very glad if Mr. Siemens had spoken—I shall be very glad if he is going to speak in the discussion—because I think he has had greater experience with large alternate-current arcs than any of us. At the Naval

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Exhibition we all admired the very large arc run on the lighthouse, which was visible over the whole of London. That was an alternate-current arc, and seemed to burn very steadily whenever I saw it; it certainly was steadier than any direct-current arc of equal power I have ever seen. We know how difficult it is to run a direct-current arc without a large steadying resistance in the circuit; and, by the way, was not the function of that resistance first pointed out by Mr. Siemens at the Society of Arts, about nine years ago? I entirely disagree with the author of the paper as to alternate-current arcs not being worth using, and as to their giving trouble. I am perfectly indifferent, commercially, whether people use direct-current or alternating-current arcs, but I must say I have got better results with alternating current than with direct; and I put it down to the fact, first, that you can use longer arcs with the same E.M.F., and the small difference of length due to the feed does not affect the whole result of the arc as with the short direct-current arc; and, secondly, to the fact that the alternating current causes a slight vibration in the mechanism of the lamp, and that assists the feeding of the carbon. Of course I recognise that this paper deals with scientific questions, and not with such matters as the mechanism of arc lamps; but I think Mr. Trotter will find that with any ordinary arc lamps, if skilfully made for alternate-current working, those two causes will enable him to get better results for the same type of lamp, with alternate currents than with direct currents. Then we are told: "A large alternate-current "arc is very unsteady. The flame burns away the carbon in a very "irregular manner; and when round carbons were used a hollow "crater was formed at the end of each." That may be the case, but I am sure Mr. Siemens can set that at rest by telling us how the big arc burnt at the Exhibition. My Company will, in the course of a day or two, have a very large alternate-current lamp running at the Crystal Palace Exhibition; it will take 200 amperes, and be automatically fed. You will have an opportunity of seeing whether that lamp can be made as steady as the ordinary search light arc of equal power; I think you will find it steadier. We will also have hung up in the Palace an ordinary alternating

Mr. Mordey.

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Mr. Mordey. arc, run in the ordinary way, so that anybody may see whether the arc is steady, and whether it justifies its existence. It shall be put in a dark lantern, so that you may study the arc itself. There is a reference in the paper that I do not quite understand as to whether an arc can change its temperature during half a period of the generation of an alternating current. Well, that is a physical matter that I am not competent to give any opinion upon; but I should think that with any ordinary rate of change of alternating currents—anything from 40 to 150—such as we have in practice, the mass of the carbon would be too great to allow of any visible fluctuation in temperature. It must surely act as a kind of thermal fly-wheel and have a fixed mean temperature during the whole period. I quite agree with Mr. Trotter's criticism of the lighthouse arc lamp shown in the wall diagram; I think that is a very bad arc indeed. The arc is far too short; the light cannot get out; it will not burn steadily. I suppose it would not burn with the modern machines at all, unless it had something like 30 per cent. of the total fall between the dynamo terminals in the resistance; and if it is possible to burn with such a very short arc, I suppose it is on account of the large drop in the curve of the dynamo. I think there should be a length of clear arc at least one-quarter of the diameter of the carbons, to get good results. We are told the South Foreland experiments showed that a short arc gave more light than a long one. I think that statement ought to contain some reference to the energy absorbed in the arc. A short arc with the same terminals, and, of course, a lower E.M.F., a lower difference of potential on account of its shortness than a long one, I think would be found to give less light than a long arc; and does not Mr. Trotter's characteristic show us that we ought to have long arcs—that we ought to let the light get out? I would suggest that it might be possible to get a better illumination curve from the arc lamp, and for many purposes an improved result, by putting one of the carbons horizontally and the other vertically, and forcing out the arc by means of an electro-magnet, so as to get the whole surface exposed and throw light down. Something of the kind has been proposed to a limited extent, I believe, for search lights, to deflect

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the light a little to one side in order to get it into the reflector, Mr. Mordey
It is unfortunate that Dr. Hopkinson is not here to tell us of the result of experiments in introducing volatile substances into the hollow carbon. At one time we were led to expect some improvement in that direction. Now, Mr. President, if my memory is not at fault, I feel that I may appeal to you to justify the custom of the trade in calling the 10-ampere 50-volt lamp a 2,000-C.P. lamp. At the last Crystal Palace Exhibition, I think in 1882, or perhaps it was at the Paris Exhibition the year before, you and others made some photometric measurements of the Brush arc lamp, and brought it out at 2,300 C.P. I am speaking from memory, but I think those were the figures. The old practice in America, followed at one time by one of the large companies—the Weston Company—and seen in the original lighting of Queen Victoria Street here, of using very short arcs, has not been followed of late years: it was claimed that they were better, steadier, and more efficient than the long arc; but my experience does not confirm that. There was a very interesting arc lamp, both mechanically and in a physical way, that Mr. Trotter has not mentioned. I refer to the Varley lamp at the Inventions Exhibition, in which a carbonised rope was used. The carbon was a piece of charred rope, coiled upon a drum, I believe, and fed out at a very slow rate by some kind of mechanism gearing with the rope; the arc was of an enormous length—something like an inch or an inch and a quarter with 20 volts. I have not heard of that since the Exhibition. I should like to know if Mr. Trotter has tried experiments with lamps in air-tight globes. I once made some tests, the idea being to get rid of the rapid consumption of carbon; and the consumption of carbon in an air-tight globe is very much less than in the air, and as far as one can see the lamp burns just as well. It shows, of course, what the effect of combustion is. As soon as you get the oxygen out of the air the carbon is not burnt, and the arc takes place in an atmosphere of carbonic oxide and nitrogen. It, however, blackens the globe a good deal, the carbon being thrown down on the globe.

The PRESIDENT: Possibly by the next meeting, which will be

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held on May 12th, the current may be laid on, and Mr. Trotter will then be able to show the experiment which he had contemplated showing us this evening. Probably, also, by that time some other speakers may be better prepared to contribute to the discussion. Under those circumstances I think the best thing is to adjourn it until May 12th. We shall perhaps then have time also to take one of the other papers intended to be read during the present session. Before adjourning, I have to announce that the scrutineers report the following candidates to have been elected:—

Members:

John Henry Holmes. | D. Codrington Selman.

Associates:

James Russell Allport. | John T. Hardy.
Henry M. Bayly. | Harry Currie Marillier.
Max Binswanger. | Mannsell Mercier.
Robert Jamieson Brown. | Edwyn Lonsdale Pope.
Charles Richard Cormac. | William Thomas Pressland.
William Henry Whitehouse.

Students:

Percy Walcott Dowse. | Sydney Stuart Galsworthy.
Montague Herbert Galsworthy. | Louis Emil Jost.
Guy Percival Morrish.

The meeting then adjourned.

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The Two Hundred and Thirty-eighth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 12th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the ordinary General Meeting held on April 28th, 1892, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

E. Celso Mozzoni. | H. W. Sullivan.

From the class of Students to that of Associates—

F. C. Raphael.

Messrs. W. H. Blakeney and P. Dawson were appointed scrutineers of the ballot.

The PRESIDENT: We will now resume the discussion on Mr. Trotter's paper, "Notes on the Light of the Electric Arc."

Dr. J. A. FLEMING: There are several points in Mr. Trotter's valuable paper which interest me particularly, because they cover ground which I have also, to some extent, gone over in experiments of my own at various times. Some of these, upon which I should like to make a few remarks, were concerned with the result of introducing various hydrocarbons into the arc. Mr. Trotter mentions the Saunderson method of using a hydrocarbon in the arc. I happen at one time to have made a great many experiments with Saunderson's arc lamp, and the figures, which I have here, with reference to those experiments are interesting. I may perhaps explain, for the benefit of those who do not know Dr. Fleming.

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Dr. Fleming.

the nature of that arc, that Saunderson used for the bottom carbon a hollow carbon with an asbestos wick up it, instead of the usual soft core, and at the bottom of this tubular carbon there was a metal vessel in which he placed some paraffin, vaseline, or easily fusible hydrocarbon, which was conducted by the asbestos wick up the bottom carbon, and thus introduced into the arc. I made a good many experiments with such an arc lamp in Mr. Apps's workshop in March, 1890. The experiments were made with a photometer of the usual type, having a special Bunsen disc adapted for such purposes, and the standard of light was a 5-C.P. incandescent lamp, which was carefully standardised before and after the experiments against a Vernon-Harcourt standard lamp. The arc lamp used was a hand-regulated arc lamp: we found that was the best on the whole, as an assistant, with his hand on the slow-motion screw and his eye on the voltmeter, could regulate the arc better than any automatic mechanism. The experiments consisted in measuring the photometric intensity of the arc in various directions, first for a pair of ordinary carbons, and then for exactly the same-sized carbons in the same arc, with the bottom carbon hollow and supplied with oil. The size of the carbons was in all cases 13 mm. carbon, and the lower carbon was either good cored 13 mm. carbon, or else an improved tubular carbon supplied with oil. The results of these observations on the photometric intensity at various angles of incidence were then plotted down in the usual polar curve. The red rays were selected by a good ruby-red glass, tolerably monochromatic, and the green by a good signal-green glass. Taking the red rays first, it was found with the ordinary carbons we got the usual polar curve of certain dimensions; and on substituting for the bottom carbon the hollow Saunderson carbon with the paraffin in it, and drawing the same polar curve for the red rays, it was found to lie outside the other altogether: there was not only more light in any direction, but more light in all directions. And the same was true for the green rays. The angle of greatest intensity of light for the red rays was somewhere about 30° or 40° below the horizon in that lamp. Taking the intensity of the red ray for the ordinary carbon in the horizontal

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direction as unity, then the maximum intensity of red rays for the ordinary carbons in the direction 30° below the horizon, was 3·44; the intensity in the case of the improved carbon in the horizontal direction was 1·21, and in the direction of maximum intensity it was 4. The results were then converted into a figure which Mr. Trotter says has no very definite meaning—the mean spherical candle-power; and the result of that was to show that the mean spherical red light was improved 26 per cent., and the mean spherical green light was improved 24 or 25 per cent., by the introduction of the hydrocarbon into the arc. It was noticed that the introduction of this vaponr into the arc greatly lengthened the arc, even for the same volts and current. The length of the arc was measured by projecting it with the lens on a piece of cardboard, and measuring the length of the arc compared with a piece of metal an inch long projected at the same time. It was found that the arc was about twice as long with the same voltage and current when using the hollow carbons with oil, compared with its length with the ordinary carbons. Moreover, it was noticeable that there was certainly much less cratering of the positive carbon when the prepared carbon was used. Those results at the time were interesting, but I have not heard much since of the application of this lamp in practice. The device seems a very simple and practical one, for the oil vessel was very easy to fill, and the wick up the hollow carbon was certainly an improvement over attempts made in the same direction before: the wick seemed to have the effect of supplying the hydrocarbon to the arc very regularly and uniformly. I should like to say a word or two on the table in which Mr. Trotter gives the intensity of the light emitted per unit of surface from different sources of radiation, quoted from the results of some work by Weber, of Breslau, taking incandescent platinum as the unit. The table is a very interesting one, showing the very wide range over which this luminous surface intensity extends for different sources of illumination. There is no mention in that table of the surface intensity of illumination of carbon at the state of incandescence at which it is in an incandescent lamp. I can perhaps supply the figure which is missing. The light emitted per

Dr. Fleming.

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Dr. Fleming.

unit of surface from the incandescent carbon filament is about the same as that from melting platinum. The standard of surface intensity which is there taken is the surface intensity of illumination from platinum not very far below its melting point, which is taken as the unit, and it gives light at the rate of 120 candles per square inch. In the case of the incandescent carbon in a lamp it is very nearly the same number, namely, 120. Otherwise we may say that the surface intensity is at the rate of one-eighth of a candle per inch mil. Taking Mr. Trotter's figure in the table for an arc lamp, it comes out at 40 candles per inch mil. Therefore, comparing the incandescent carbon of an incandescent lamp with that of an arc lamp, it is in the ratio of 40 to $\frac{1}{8}$, or 320 to 1. It would be interesting to know what ratio exists between those surface intensities and the true temperature of the surface—in other words, what function of the temperature of the surface is the surface intensity. It must be something like the cube, or a fourth power, for we know the temperature of a filament of an incandescent lamp is something not very far removed from that of melting platinum—say 2,000° C.—and if the arc crater has a temperature of 8,000° C.—that is to say, four times the temperature of the filament of an incandescent lamp—then the temperatures of these surfaces are in the ratio of 4 to 1; but the surface intensities are in the ratio of 320 to 1, which is more than the fourth power. That point is one which certainly needs further investigation. Some time ago I commenced an investigation, which has never yet been completed, on a comparison between continuous- and alternate-current arc lamps. I had an idea that there was an intrinsic inferiority in the alternate-current arc. I began by obtaining a good arc lamp, adapted for working either with an alternating current or with continuous currents, and determining the mean spherical candle-power of the arc taken from the polar curve, comparing that with the watts expended in the arc, and drawing a curve—or, rather, a series of curves—representing the variation of candle-power with watts expended in the arc: that was done for different rays for continuous- and alternate-current arcs. For the red rays of a continuous-current arc the curve was very

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nearly a straight line. The curves are here, and if anyone wishes to see them after the meeting, I shall be happy to show them. I only stopped for want of better means of measuring the power taken up in an alternating arc, for I suspected that under some conditions an alternate-current arc behaved like an inductive resistance—a result since verified by Professor Ayrton. Hence my experiments remained on this subject incomplete. Dr. Fleming.

Sir DAVID SALOMONS: Mr. Trotter, in his very interesting paper, uses, near the commencement, these words: "A little "consideration will show that the effect is precisely and "identically the same as though the end of the positive carbon "were flat." This is not strictly true, and in the remainder of his paper all the arguments adduced are those applicable to the carbon rod end being cup-shaped, and he, therefore, arrives at the truth. A flat surface emitting light does not illuminate equally in all directions, and, considering for a moment a point upon such a luminous surface, the greatest intensity will be in a direction normal to that surface, and less and less as it becomes the more inclined to this normal, varying as unity to the cosine of the angle included between the surface and the ray under consideration. The refractive index of the medium need not be taken into account in this particular case. It is evident, therefore, that, if the positive carbon burned with a flat end, for practical purposes this method of illumination would become impossible, since the direction in which most light would be given would be cut off from sight by the lower carbon. The fact of the positive carbon burning hollow renders the arc light a useful illuminant; and it this circumstance I am anxious to point out, as it is one that few persons appear to have considered. Sir D. Salomons.

Mr. R. W. WEEKES: When at the Central Institution, Mr. G. H. Thornton, Mr. A. O. Kolkhorst, and I worked for a considerable time on original research as to the nature of the electric arc. Mr. Weekes

Amongst the various points we investigated was the variation of the polar curve with the length of arc and size of carbon, when the current is kept constant and the volts are increased with the length as required.

The present method of measuring the actual candle-power at

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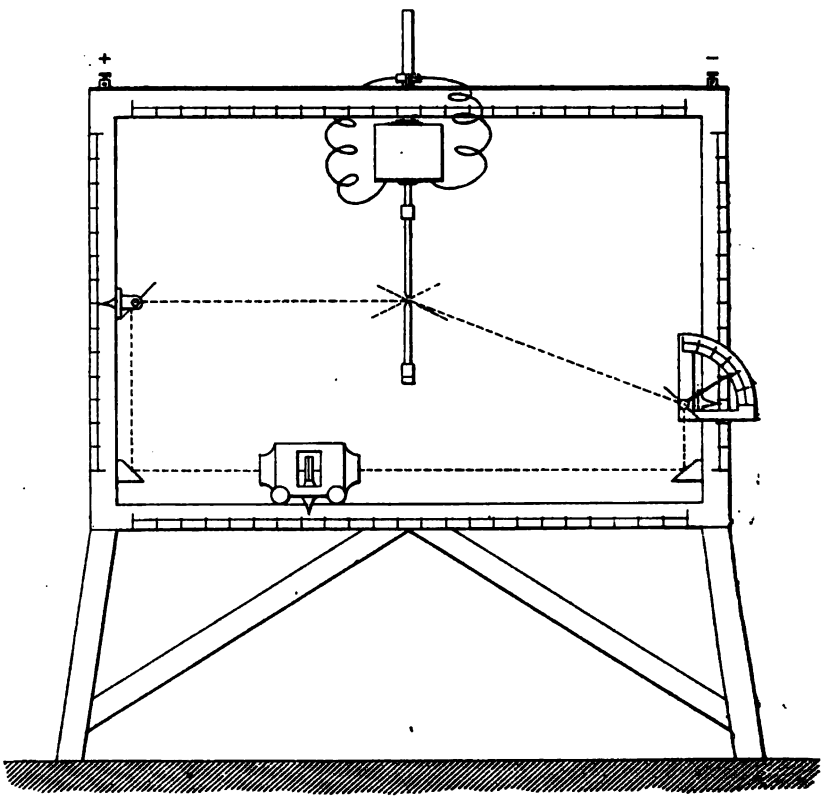
Mr. Weekes. each angle by means of an Ayrton and Perry dispersion photometer was found to be always tedious, and in the case of hard carbon it was also inaccurate unless a very large number of readings were taken. The individual readings when both the current and voltage were constant varied by 25 per cent., and the mean of four readings gave anything but the true mean candle-power in any assigned direction.

This led us to design the apparatus shown in the accompanying figure, for obtaining polar curves, which I trust may be of



interest to the members of this Institution. The principle of the method is that of measuring the ratio between two rays coming from the arc at different angles, and as this ratio does not vary so much as the actual candle-power, more concordant results can be expected. These ratios give the polar curves, and the actual value

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of the radii in candle-power can be obtained from the mean of a large series of readings of candle-power in one direction only. The frame, and the method of reflecting the rays, can be readily understood by the aid of the adjoining diagram, which shows the horizontal ray being compared with one at about 20° to the horizontal. As three out of the four mirrors need not be moved, the correction for absorption becomes simple. The lamp connections are made by two knife edges resting on copper rails, and this also ensures that the lamp shall hang vertically. As all the sides of the frame are graduated, the various distances can be easily read off. I regret that we were unable to complete the research, but it is being carried on by present students, and the whole results will be published when complete. Mr. Weekes.

Mr. E. T. CARTER: I should like to occupy your attention for a few minutes to make a remark or two on some observations I have been making since the paper was read. Let me call your attention in the first place to a sentence in the paper in which is a quotation from a paper read in 1889 by Dr. Silvanus Thompson, on "The Arc Lamp and its Mechanism." The sentence is this: "It is as impossible to raise carbon above the degree of incan-descence of the crater of the arc as it is to raise water above "boiling point or ice above melting point. No substance has "yet been suggested for improving arc lamp carbons which is "less volatile than carbon." Dr. Fleming has already mentioned one method of increasing the efficiency of an arc lamp by the introduction of what I my call foreign material. My remarks will be of purely theoretical interest, I think, but it has occurred to me that there is another method of raising the temperature of the arc. According to the theory of heat, we know that the boiling point of a liquid depends upon the pressure which the liquid supports—*i.e.*, the pressure of the vapour; and if the arc were placed in an atmosphere subjecting the vapour of the carbon to a higher pressure, the carbon would necessarily boil, as it were, or vaporise at a higher temperature. Consequently, we should have the surface of the carbon at a higher temperature, and we should, therefore, have a higher efficiency, for the efficiency of any light source is a direct function of the temperature: raise the Mr. Carter.

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Mr. Carter. temperature and you increase the efficiency. For instance, the temperature of an incandescent lamp, 2,000° C., gives an efficiency of luminous rays of 6 per cent.; and the temperature of the arc, 3,000° C., gives an efficiency of from 15 to 20 per cent. Now, if we assume—and I think we have a perfect right to assume—that the saturation curve of carbon vapour resembles the saturation curve of steam, or CO_2, within a small percentage of difference, then we can calculate approximately what the rise of temperature would be with a certain increase of pressure of the vapour. I have estimated that if we subject the arc to a pressure of eight atmospheres, we shall get a rise of temperature of the carbon of, approximately, 1,200°. Consequently, without the introduction of any foreign material into the arc, we can get a higher degree of illumination from the arc; and the limit is only fixed by the possible limit to the pressure to which we can subject the arc. I do not say that this is of any practical value for ordinary work: it may be of use in lighthouse work, or for experimental work; I merely suggest it for theoretical reasons. Thus the efficiency of the lamp can be increased, and the intensity of light from each square inch of incandescent carbon also increased; the lamp giving more light with smaller carbons and less power. There would be less screening by the lower carbon, and it is probable that a longer arc would be produced.

Speaking on another point, I have made some calculations on the probable amount of energy consumed in overcoming the back E.M.F., which, although useful energy, involves a very high E.M.F. to work the arc. Dr. Thompson states that the minimum volts required to work an arc steadily is 39, and gives strong ground for our believing that this represents a back E.M.F. due to the vaporisation of carbon. Assuming all the loss of positive carbon to be due to vaporisation in this way—electro-vaporised, as it were—then we have a latent heat of carbon vapour of 400,000 w.g.d. C. Whereas, if we assume that only the difference between the positive and negative carbon is thus vaporised by direct electrical action, we have a still higher latent heat of 1,030,000 w.g.d. C. These figures, which I have worked out this

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week, give at best only the approximate value of the latent heat of carbon vapour. By all analogy, they seem too high; and it is possible that they represent more energy than is used in the mere evaporation of carbon. Of course it is difficult to say how much of the carbon is really vaporised by direct electrical action, as some of it may be re-deposited on the negative carbon. I do not think that much of this condensation goes on under ordinary conditions, and I am disposed to attribute the peak on the negative rod to another cause than the piling up of carbon upon it. It seems to me that the absence of any oxidising agent in the centre of the arc would be sufficient to account for the negative carbon there burning away so slowly, while all around the edge of the hot carbon the air would cause a rapid combustion. Thus the peak would be formed. I am now about to make some experiments to ascertain what is the exact amount of carbon, if any, which is deposited on the negative rod; and I hope at some future time to be able to bring the results before you. Mr. Carter.

Mr. J. N. SHOOLBRED: Not having had the advantage of being present when the paper was read, I will not attempt to discuss this interesting and valuable communication. I rise more for the purpose of asking the author a question. He refers to two communications published in *La Lumière Électrique*—one by M. Rousseau, and one by M. Palaz—as being interesting, and containing some information with regard to polar curves—I think those represented in Figs. 1 and 3. May I ask him if he remembers the date of those communications? He simply refers to certain volumes of *La Lumière Électrique*. The reason I ask this is, that some interesting curves of the "luminous intensity" of arc lights, the result of experiments carried out by Messrs. Sautter, Lemonnier, & Cie., in Paris, were noticed first before the Society of Arts in 1878, and subsequently published in a little book entitled "Electric Lighting in its Practical Application" (Hardwicke & Bogue, 1879). The description also included some experiments on the arc-light curves from alternate-current machines, from experiments which had been specially carried out by M. Allard, of the French Lighthouse Department; the results being also added of some experiments by Mr. (now Sir Mr. Shoolbred.

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Mr. Shoolbred.

James) Douglass at the South Foreland, in 1876 and 1877, with a Siemens continuous-current dynamo machine, on the difference between the "condensed" and the "diffused" beam. Photometric curves were given resulting from the use of a 6-arc alternate-current Lontin machine (a rare machine in those days of single-arc machines). Furthermore, the results of the illumination given off from the Jablochkoff candle, carried out by the Municipality of Paris, were described. All of the above experiments (excepting those of Sir James Douglass) were necessarily carried out in Paris (and most of them for the little book above named), as there were no such facilities in this country at that time. A table is also added as to the luminous efficiency of different electric machines. The information just referred to attracted much interest at the time, and the late Sir William Siemens mentioned that, as far as he knew, it was the first time the peculiarities of these curves of luminous intensity, especially of the continuous-current machine, had been printed. Had that early information been referred to by the author (though probably he was not aware of its existence), several of the inquiries made during the discussion would thereby have been answered. The latter part of the author's paper, referring to the arc lighting now being carried out in the City of London and elsewhere, contains much which is not merely of interest, but which deserves careful and serious study.

Professor Thompson.

Professor SILVANUS P. THOMPSON [*communicated*]: Mr. Trotter's paper is so complete and satisfactory that it leaves little room for discussion, beyond congratulating him upon the very admirable way in which he has straightened out the crooked problem of the distribution of light from the arc lamp. It has been long known in a general way that the peculiar curves of distribution of light were occasioned by the shadow of the lower carbon. But Mr. Trotter's investigation has put the matter upon a new and sound basis of observation. His intensely geometrical mode of viewing all matters has led him to discuss the arc lamp from a novel standpoint, and to give us the true solution of the problem. It might be worth while to re-examine by the light of these researches the curves given some 12 or 14 years ago

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in the Journal of this Institution for the distribution of light from the Jablochkoff candle. Doubtless these curves would also prove amenable to the same sort of geometrical treatment. Professor Thompson

The starting-point of Mr. Trotter's investigation is the recognition of the fact that the intrinsic brightness, per square millimetre, of the carbon surface of the crater is a fixed quantity, being due to the incandescence of a determinate substance—carbon—at a temperature which is also determinate, being fixed by the physical conditions of its production. It also necessarily follows that, so far as the actual crater is concerned, the light emitted is always of precisely the same composition; that is to say, the various kinds of waves emitted—red, green, blue, &c.—are always present in the same proportion. In the recognition of this fact the name of Captain Abney should not be forgotten, since he has made it the basis of much of his photometrical work during recent years. I would now throw out the suggestion whether, in view of the unchanging quality of the light of the crater of the electric arc, it is not worth while to reconsider the question of a standard of light, and adopt *as a unit of light* the light emitted by a square millimetre of crater. It is certainly a more practical unit than the square millimetre of molten platinum used by Violle; and it is preferable, by reason of its greater whiteness, to the flame standards (such as the pentane flame, the amyl-acetate flame, and the Carcel lamp) that have been adopted from time to time. I omit the sperm candle and the Methven gas standard as being too unscientific to be put into comparison.

It is to be hoped that Mr. Trotter will be encouraged to complete his experiments on coloured shades, so as to give us a real daylight illumination. It is obvious that such shades can be used in conjunction with electric lamps (whether arc or incandescent) with far greater facility than with any flame lamps. Certainly, artists and all lovers of art will welcome such a means of lighting rooms. The introduction of such combinations will extend the use of electric illumination.

The PRESIDENT: Before calling on Mr. Trotter to reply, I will make one or two observations, partly in reference to some The President.

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The President.

remarks made on the last occasion. Mr. Mordey considered that he was justified in calling the light given off by a 10-ampere Brush lamp 2,000 and something candles, because the jury at the Crystal Palace in 1882 had reported that such was the efficiency of the Brush lamp. He coupled my name with that jury, and wished, therefore, to imply that the results were immaculate. However, I was not on that particular jury, and therefore am not responsible for the accuracy or inaccuracy of their results.

Mr. Carter has assumed in his remarks this evening that the back E.M.F. of the arc is a perfectly definite and known number. I wish just to point out in passing that I am not aware of any tests whatever that have proved that there is a back E.M.F., still less that it has the value of 39 volts. It is undoubtedly frequently stated that there is a back E.M.F. in an arc, but I am not aware of any tests that substantiate this fact.

Mr. CARTER: I merely took the figure given by Dr. Thompson.

The PRESIDENT: He has not, as far as I am aware, made any test himself on this subject, and I fancy he is merely quoting the popular opinion on the subject.

Dr. Fleming's remarks with reference to the Saunderson lamp open out a wide subject, about which I should like to say a good deal; but in view of the other paper which we are looking forward to hearing from Dr. Gladstone in a few minutes, I refrain from doing so. May not, however, a possible explanation of the greater light, red and green, given off by the arc when the paraffin oil was introduced, be simply looked for in the greater length that could be thus given to the arc? for this would diminish the shading of the top carbon by the lower.

As regards the curves at the end of the paper, they are of very great interest indeed. The question of producing artificially, by means of arc or incandescent lamps—especially with arc lamps—lights of particular colours, is a subject which has interested me for some years past. On two occasions at the Royal Academy I have urged on Dr. Russell, whose name is mentioned in this paper, and he has kindly undertaken (but I do not know whether he has yet had time to do so), to bring my views before Sir Frederick Leighton as to methods of grouping the pictures in the Royal

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Academy and illuminating them with light of a particular colour for each particular group of pictures, so as to produce not merely as good effects as are obtained in the daytime, but, I think possibly, far better effects than the uncoloured sun gives. The President.

The question as to whether arc light is or is not blue with reference to white light, is a subject which has been discussed backwards and forwards, and the advocates on each side consider that they are both right. The explanation of that is that they *are* both right. The arc light is and is not blue relatively to white light. All depends upon what you mean by white light. People talk about white light as though it were some definite thing. If by white light you mean sunlight during the day—the unclouded sun—or if you mean the light given off from the white clouds in the daytime, then undoubtedly the arc light is red, and not blue, compared with that standard. This you can easily prove by using what Mr. Trotter appears to rather despise—red and green glasses. It is perfectly true, as he says, that you cannot combine the results obtained with red and green glasses: you cannot use the arithmetic mean, or the geometric mean, or the square root of the mean square; but I do not think that constitutes any objection to making experiments with red and green glasses. In the same way you may speak about the experience of a person and the ability of a person, but you cannot take the arithmetic mean, or the geometric mean, or the square root of the mean square of the experience and ability of that person; yet, when judging of the value of a person, you think of his experience as compared with his ability. In the same way, when you want to speak of the arc lamp as of so many candles, it all depends upon what you want to use the lamp for—whether to illuminate green leaves or red roses. The value of the arc light is very different in those two cases. It may be two or three times as valuable for the one purpose as for the other. Therefore, surely, it is perfectly right to separate the two measurements. If you use the despised glasses, you can at once prove that the arc light is red compared with the sunlight. All you have to do is to get a balance with the photometer looking through green glass, and then look through the red

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The President. glass, when you will find that balance is no longer produced, and that the arc light, as seen through red glass, is the brighter of the two. But arc lights are, as a rule, used, not in the daytime, but at night, and during the night white light is not sunlight. That, of course, brings us to the consideration of what is the definition of "white light." I believe the proper definition is, *white light is what you see most of*. Simply that. Now, up to the present time, we have not seen most of arc lights during the evening. Therefore there is a particular standard during the evening, and in accordance with that standard, which we have adopted by experience, the arc light is undoubtedly blue during the night. Also, as General Festing said, the sunlight of the early morning gives a very bad and imperfect impression of white light if you have been up during the night. If you take the daylight standard, then the early morning light appears quite right; but if you take the night standard, then the early morning light appears wrong. Of course you may say that is introducing physiology and psychology, but you cannot help introducing them if you desire to understand this point. Helmholtz some years ago made a very simple experiment. It was this: In order to see what was your estimation of distortion, he put a pair of distorting spectacles on a willing subject, and found that in a comparatively short time the person wearing them ceased to have any feeling of distortion: when looking through those glasses he saw everything perfectly correctly, and he only saw things distorted when he took off the glasses. That is to say, the eyes quickly adapt themselves to a particular standard at a particular time. Well, now, during the night your standard is the gas and candle standard, and it is perfectly right to say the arc light is blue compared with white light at night. But, on the other hand, the standard of white light during the daytime is sunlight, or the light from the clouds, and compared with that light it is equally right to say that the arc is red.

Mr. Stothert. Mr. J. K. STOTHERT [*communicated*]: It may interest Mr. Trotter and the members of this Institution if I mention some experiments performed at the Electrical Standardising and Test-

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ing Institution, for some clients; not with the object of making measurements and analyses of light of an ultra-scientific nature (as that has already been done by Captain Abney and others), but of finding by practical methods, which artificial light is most similar to diffused light from clouds. Mr. Stothert.

I am not at liberty to do more than indicate the nature of our experiments. The artificial lights used were—

1. Incandescent lamps.
2. ,, ,, with various coloured glasses.
3. ,, ,, over-incandesced.
4. Arc lamp.
5. Sun lamp (kindly lent by inventor for the purpose).

It was found that light from the Sun lamp, with suitable reflectors and diffusing screens, was as nearly as possible the same as diffused daylight in a room. There was very little difference between the spectrum of the light from this lamp and that of diffused daylight.

We put the lamp to another practical and, I venture to think, severe test. We asked some competent artists, exponents of both oil and water colours, to paint an object by the light of the Sun lamp, afterwards comparing the pictures and the object by daylight. The results were most satisfactory. The pictures had all the appearance of having been painted by daylight.

I ought to mention that I am not financially interested in the Sun lamp, and that the experiments were not carried out for the inventor.

Mr. A. P. TROTTER: I have to apologise to the meeting for not being able to show the experiments which have been a second time promised. The terminals are in that glass box, and have been there for ten days, but the insurance inspector has not been there. I hoped to have shown one or two other experiments with reference to what Mr. Swinburne said, and what Professor Silvanus Thompson has also suggested, about using a square millimetre of the crater as the unit of candle-power. Mr. Higgins drew my attention to the fact that the core of cored carbon is distinctly duller than the rest of the carbon—it is not such a good carbon, but it has a valuable function in centring the arc. I suggested Mr. Trotter

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Mr. Trotter. to Mr. Higgins to take a double carbon, as good as could be got, and also a piece of battery carbon; to tie them together side by side; and to see if one half of the crater was brighter than the other. He has tried the experiment, but found that the soft carbon flared and flamed so much that it was difficult to make a test. This suggests that differences may be due not only to emissivity, but also to the volatility of the carbon. The temperature must go down if the carbon is more volatile, and therefore I think it could hardly be depended upon for a unit of light.

The question has been raised about the comparative merits of continuous- and of alternate-current arcs. I have had very little experience indeed with alternate-current arcs. I am glad to hear Mr. Mordey has a high opinion of them, for that must mean that there is a great deal in them. Mr. Mordey suggested that I had described the crater as being a horizontal disc, where it is really concave; and Sir David Salomons has given his views on the subject: I venture to disagree altogether with them. I would like to have shown an experiment to demonstrate this point. I prepared a cylinder of cardboard about 6 inches in diameter, one end flat and the other end a concave hemisphere, and these were painted with luminous paint and exposed to bright light. If I had it here, and showed you first the flat end and then the concave hemisphere in a darkened room, both ends being self-luminous, I venture to say no one within 6 feet could have told which was which.

Sir DAVID SALOMONS: I did not say that. At the angle you made with the normal you could not tell the difference, looking at it straight.

Mr. TROTTER: I say in the paper that no tilting of a luminous surface can make it brighter or less bright. Where light falls on a white reflecting substance it is not quite the same thing; the law of cosines is not strictly followed. But I am sure that I am correct in saying that with a self-luminous surface it is a mere question of how much you can see of it at any angle. Mr. Mordey thought that I should not have said that the alternating arc had time to get cool, and suggested that there was, as it were,

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Mr. Trotter.

a thermal fly-wheel, owing to the mass of carbon behind the crater, which would have enough heat to keep the arc going. I should like to have shown on the screen the effect in the arc of reducing the current by a very small quantity. If you have the crater well focused on the screen, and reduce the current, you will see the surface of the crater instantly curdle all over in a sort of flocks, and in an instant after the crater will diminish and shrink in size: the current cannot support so large a crater. I think that, since volatilisation takes place actually at the surface, the luminous heat is actually on and at the surface alone. Truly white-hot carbon is actually at the surface alone. An incandescent lamp carbon is different. The heat is all the way through the filament, and, indeed, the temperature must be greater inside. Be that as it may be, if the crater is being heated to the highest point only for half a period or less, it is difficult to see how the luminosity could be the same as if it had the full current on one crater upon which we depend for the light. I should like to have heard that Professor Ayrton could have supported the 2,000-C.P. fable. I do not know anybody else who has obtained that measurement, and I should imagine it could only be done by measuring with a photometer on each side, and adding the results together.

Dr. Fleming has mentioned the Saunderson carbon, and he says the maximum was given out at 30 degrees to 40 degrees, I think he said, below the horizon. The maximum is therefore rather higher in the ordinary short arc. But the Saunderson arc ought to give a longer arc. Well, Dr. Fleming says that it does give a longer arc. But the chief point of my paper is to show, if the lower carbon were drawn away, the lower part of the curve would be fuller. Therefore I should have thought that the longer the arc, you would have got the maximum shifted to a greater angle with the horizon. I am surprised in these good results that nothing more has been heard of this. I think about 10 months ago some American patented the leading of a wick up a groove outside, instead of in the middle—a more palpable infringement it is impossible to conceive—but we have heard nothing more of it.

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Mr. Trotter. My table does not contain a glow lamp. I mentioned the subject to Dr. Fleming, and there is no greater authority upon glow lamps; but one does not like to press him for details about Edison-Swan lamps. We shall know sooner or later, very likely. I have mentioned in the paper the method of Rousseau, in which rectangular co-ordinates are used, but I found that I could calculate spherical candle-power in the way I described, taking the area as bits of parabolas or bits of ellipses. These can be measured out in about one-fifth of the time it would take to plot it out and measure it with the planimeter. Dr. Fleming also said you must either tilt the arc or raise it to a great height in order to measure the angular rays. The method I have adopted in the instrument which Mr. Higgins constructed at Finsbury, gets over that difficulty: you merely swing round the arm, and allow for the reflective power of a single mirror, which is always at the same angle. The apparatus described by Mr. Weekes employed four mirrors, one of which had to be adjusted at different angles. The horizontal ray of an arc is a bad standard. The smallest irregularity of the edge of the crater causes great variation in the light.

The question of raising the temperature of the arc has been mentioned by Mr. Carter. I am rather surprised to hear that even eight atmospheres would be enough to give any useful effect, but it does not appear to be at all practicable. That reminds me about Mr. Swinburne's remarks as to the curious discrepancy about the light given by the sun per square inch, and that given by carbon. He suggested that a piece of the sun cut out and allowed to cool would be 10 to 20 times blacker than carbon. It is difficult to imagine anything blacker than carbon, but there is something. A hole cut out in the side of a box lined with black velvet is much blacker, and is sometimes used in colour measurement. But that will not account for the discrepancy. In the carbons I have been using, the core is distinctly more gray than the rest. The carbon is as black as jet. But there is this to be said—that the pressure on the sun may be many tons per square inch, and it is very possible that that might raise the temperature sufficiently to account for the discrepancy. I have checked that

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figure by some different experiments, and these bear out the figure of Weber. Mr. Shoolbred asked for dates. Allard and Sauttier-Lemonnier were no doubt long before those of Wybauw. These measurements were made in connection with the Antwerp Exhibition of 1885; they were taken as the mean of a large number of cases. No doubt those Mr. Shoolbred refers to were the first polar curves made. Mr. Trotter.

I fully expected to have heard some remarks to-night from a gentleman, whose name I have not even heard, on some experiments made at the Standardising Institution with coloured shades. I alluded when reading the paper to the fact that someone else was working at the same thing. There was a short letter in *Lightning*, and it alluded to the fact that some coloured shades had been used in experiments. I understand that some experiments were made there with a gray screen—not so very blue, but gray—and with Sun lamp, which is the Lampe Soleil resuscitated, and that some very good results were obtained.*

Professor Ayrton's remarks about back volts are, I think, interesting. I hunted well through all the records I could get to find anything about back volts. I believe Professor Ayrton has some experiments which are not quite completed yet. Then Professor Ayrton has given a very good reason for showing why you may talk about red and green, and his analogue of ability and experience is most excellent. He says white light is what you get most of; but that is, unfortunately, not the case with ability and experience.

The PRESIDENT: Before proceeding to the next business, I will move a hearty vote of thanks to Mr. Trotter for his interesting paper, which has given rise to an extremely interesting discussion.

The motion was carried unanimously.

The following paper was then read :—

* See Mr. J. K. Stothert's communication, p. 406.—ED.

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ON THE CAUSE OF THE CHANGES OF ELECTRO-MOTIVE FORCE IN SECONDARY BATTERIES.

By J. H. GLADSTONE, Ph.D., F.R.S., Member, and WALTER HIBBERT, F.I.C., Associate.

In 1882, Dr. Gladstone and the late Mr. Tribe sent to *Nature* four letters on the "Chemistry of Secondary Batteries." The main point established in these papers was the all-important functions of sulphate of lead. They showed, in fact, that when a lead lead-peroxide cell is discharged, "sulphate of lead is the "ultimate product on both plates;" and when it is charged again, "this lead sulphate is oxidated on the one plate, and reduced on "the other."

In 1883, another letter appeared, in which, among other things, the effect of different strengths of acid when the cell is being charged was investigated, and the existence of occluded hydrogen, ozone, and hydrogen dioxide was considered.

These letters were subsequently published in book form.* In a paper of the same authors in the *Journal of the Chemical Society* for 1883, p. 345, reference was made to the production of persulphuric acid in the electrolysis of oil of vitriol.

In 1883, Professor Frankland† obtained substantially the same results, and proposed to ascertain the state of a cell during charge or discharge by observing the density of the acid.

In 1889, Messrs. Duncan and Wiegand‡ investigated the rate of diffusion of the acid out of the pores of the spongy masses in the plates of a secondary battery.

In 1889, Heim§ investigated the change in capacity of secondary batteries when used with different strengths of acid, and gave the E.M.F.'s observed when the strength varied from 10 to 35 per cent. H_2SO_4.

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* "The Chemistry of Secondary Batteries." Macmillan, 1883.

† *Proc. Roy. Soc.*, vol. xxxv., p. 67.

‡ *Electrical World*, June 15, 1889.

§ *El. Zeit.*, 1889, p. 88.

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In 1890, we published experiments* which led us to regard the "abnormal initial E.M.F. of a secondary battery as due to "inequality of acid strength, and its gradual disappearance as due "to equalisation of strength produced by diffusion."

Shortly after this, an elaborate and valuable series of observations on accumulators was published by Messrs. Ayrton, Lamb, Smith, and Woods,† who have given curves showing the changes in potential difference under many conditions of working. They allude to our suggestion, and conclude on various grounds that it is insufficient to explain some of the phenomena.

In the discussion which followed this paper, Mr. Hibbert urged that curves given by the authors afforded confirmation of our hypothesis.

Subsequently, two papers were sent to the Royal Society by Mr. G. H. Robertson and Professor Armstrong,‡ of which only the abstracts have been printed. From these, as well as from a lecture by Mr. Robertson at the Society of Arts,§ we gather that they attribute much to the formation of persulphuric acid and hydrogen dioxide.

We have lately been making additional experiments on the matter, which have led us to the conclusion that *variations in the strength of the sulphuric acid are the main cause of the variations in electro-motive force.*

We propose to consider—

1st. What variations of strength of acid actually occur during charge, repose, and discharge.

2nd. Experimental determination of the change of E.M.F. produced by changing the strength of acid.

3rd. How far this is capable of explaining all that is known about the changes of E.M.F.

4th. Confirmations, experimental and theoretical.

5th. Other suggested causes.

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* "Notes on Secondary Batteries," *Phil. Mag.*, 1890, p. 168.

† *Jour. Inst. Elec. Engineers*, 1890, pp. 539 and 560.

‡ *Proc. Roy. Soc.*, 1891, pp. 105 and 108.

§ *Soc. Arts Journal*, Dec. 4, 1891.

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PART I.—WHAT VARIATIONS OF STRENGTH OF ACID ACTUALLY OCCUR DURING CHARGE, REPOSE, AND DISCHARGE.

(*a.*) *Changes during Charge.*—If we start with a properly formed cell which has been discharged, we have to deal with two leaden supports, on one of which there is a mixture of lead sulphate, $PbSO_4$, with more or less lead peroxide, PbO_2, and on the other a mixture of lead sulphate, $PbSO_4$, with more or less spongy metallic lead. Each of these mixtures is a porous layer the interstices of which may penetrate to the leaden support.

If these be charged, the chemical action consists of the conversion of the lead sulphate on the one plate into PbO_2, and on the other into spongy lead, and the electrolytic change may be expressed thus:

$$PbSO_4 + H_2O \ldots . H_2O + PbSO_4 = PbO_2 + H_2SO_4 \ldots . H_2SO_4 + Pb.$$

It is evident that during this process sulphuric acid is formed in the pores of both plates, whilst at the same time an equivalent amount of water disappears.

Apart from this electro-chemical result, which is equal on both sides, it is well known that during an electrolytic decomposition there is a gradual heaping up of the acid at what is now called the positive electrode, and a drawing of it away from the other. This increase of strength of acid at the peroxide plate may be seen by the descent of a denser layer of acid through the clear space under the plate. Indeed, it is well known that there is a circulation in the cell, the acid becoming stronger at the bottom and weaker at the top.

But to remove any doubt as to the strength of acid in the pores of the peroxide being greater than in those of the other plate, we made a direct experiment.

A cell was made of two small fully formed plates, each in a porous pot containing about 30 cubic centimetres of acid. These porous pots were placed in a large dish of the same acid and a current of 0·2 ampere sent through for two hours. After that time, the whole of the acid in the pot containing the PbO_2 plate had increased 3 per cent., whilst the whole of that round the Pb plate had diminished 1 per cent.

Of course, during the whole process of charging, diffusion is

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tending to equalise the strength of the acid, but it is much impeded by the capillary nature of the passages through which it must take place. It is quite conceivable that towards the end of charging, a film of the strongest acid—that is, H_2SO_4 itself—covers the working surface of the PbO_2 plate.

All these actions account for the well-known fact that during the charge the whole body of the liquid in a working cell rises in density by somewhere about 0·04.

(*b.*) *Changes on Repose.*—At the end of charging, the PbO_2 plate consists of porous peroxide attached to the lead support, surrounded by strong sulphuric acid. This sulphuric acid will diffuse out into the intermediate liquid at a rate which Messrs. Duncan and Wiegand's results show to be rapid at first. Complete equalisation is, however, a very slow process, to be reckoned by hours rather than by minutes.

But there are other actions reducing the sulphuric acid in the pores at the same time. The PbO_2 and its supporting lead are in a condition to set up energetic local action with the formation of sulphate of lead, and consequent absorption of sulphuric acid from the liquid. The chemical change is as follows:—

$$PbO_2 + H_2SO_4 \ldots . H_2SO_4 + Pb = PbSO_4 + H_2O \ldots . H_2O + PbSO_4.$$

(It must be remembered that the Pb in this equation is the lead support for the peroxide. Common experience shows that it is corroded during use.)

This action clearly will absorb sulphuric acid from the liquid in the pores and replace it by water.

In a well-charged plate, there is always at first an evolution of a little oxygen gas, which has been attributed* to the reaction of hydrogen dioxide on peroxide of lead.

$$PbO_2 + H_2O_2 = PbO + H_2O + O_2.$$

If this be the case, the oxide of lead (PbO) formed must also abstract its equivalent of sulphuric acid from the liquid.

Of the three causes of weakening—diffusion, local action, and reduction by H_2O_2—the first goes on till the acid in the pores is brought down to the same strength as that in the inter-

* "Chemistry of Secondary Batteries," p. 51.

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mediate liquid. But the local action may still continue for many days, and will tend to keep the acid in the pores a little weaker than that in the external liquid, notwithstanding the return diffusion of acid towards the plate.

Turning now to the Pb plate in repose, we find none of the above actions taking place, except the slow equalisation of strength produced by diffusion.

But there is an action peculiar to this plate—that is, a direct, slow chemical action of the sulphuric acid on the lead, producing lead sulphate and hydrogen gas.* The equation is—

$$Pb + H_2SO_4 = PbSO_4 + H_2.$$

This must gradually produce a weakening of the acid in the pores, and it is important to notice that diffusion, which is always slow, will be almost entirely prevented by the choking up of the capillary passages by the gas evolved.

We have indicated the probability that towards the end of a charge there is a film of the strongest acid against the working surface of the PbO_2 plate. If this be true, it is evident that as soon as repose begins, such a film will almost immediately disappear, owing to diffusion into adjacent liquid, whether in the pores or out of them.

(*c.*) *Changes during Discharge.*—As soon as the discharge begins, a still more rapid reduction of the strength of acid may be expected. Diffusion, local action, and reduction by H_2O_2 will still take place on the PbO_2 plate, and the direct chemical action on the Pb plate, but on each plate there is superadded the ordinary discharge reaction of the cell. The form of the equation for discharge is the same as that already given for local action, namely—

$$PbO_2 + H_2SO_4 \ldots . H_2SO_4 + Pb = PbSO_4 + H_2O \ldots . H_2O + PbSO_4.$$

(In this case the Pb represents spongy lead on the Pb plate.)

But while the absorption of sulphuric acid and production of water thus goes on in both plates, electrical transference of the

* Gladstone and Hibbert, *Phil. Mag.*, 1890. p. 161; Ayrton and others. *Jour. Inst. Elec. Engineers.* 1890, p. 680.

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$H_2 SO_4$ now takes place from the Pb O_2 to the Pb plate, causing an additional weakening of the acid in the pores of the first.

If discharge has commenced immediately upon the cessation of charging, these different causes will combine to produce a very rapid decrease in the strength of the acid at the Pb O_2 plate.

In any case, a period must soon arise in which the great excess of sulphuric acid originally about the Pb O_2 plate has disappeared through these various agencies, and the acid on both plates will be reduced to pretty nearly the same strength as that of the intermediate liquid. After this, there will be a gradual withdrawal of acid from the liquid in the pores, more or less compensated by diffusion inwards from the intermediate liquid. This, of course, brings about the reduction in the strength of the whole acid, which is well known to take place during discharge.

The strength of the acid in the pores will be determined by the relative values of the rate of withdrawal and the rate of diffusion. But while the rate of withdrawal continues constant for a given discharge current, the rate of diffusion rapidly diminishes.* The rate of weakening of the acid is therefore a constantly increasing one, and may finally become so rapid that the acid strength of the liquid against the working surfaces of the plates is very low or almost *nil*.

In such a case we may expect the formation of the white compound described by Gladstone and Tribe,† which when analysed seemed to be a basic compound of the composition $2 Pb SO_4 . Pb O$.

(*d.*) *Changes on Repose after Prolonged Discharge.*—If through prolonged discharge the acid against the working surfaces has become very weak, and the discharge is then stopped, it is evident that the acid in the pores will quickly increase in density, and uniformity of strength in and outside the pores will be restored after awhile; but the general strength will always be lower than the original value. A renewal of the discharge will,

* This might be expected from the partial clogging of the pores by the $Pb SO_4$ formed on both plates, and it has been experimentally shown to be the case by Messrs. Duncan and Wiegand.

† "Chem. of Sec. Batts.," p. 46.

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however, soon lead to exhaustion of the acid against the working surfaces.

PART II.—EXPERIMENTAL DETERMINATION OF THE CHANGE OF ELECTRO-MOTIVE FORCE PRODUCED BY CHANGING THE STRENGTH OF ACID.

It is a matter of general knowledge that the electro-motive force of an accumulator rises slightly when the strength of the acid is increased. Incidental determinations of the rise occur in several papers, of which we may mention one by Preece* and another by Heim.†

In 1890, thinking it probable that the chief cause of the high E.M.F. was "the great inequality in strength of acid produced by "the charging current," we made some experiments which were described, with tabulated results, before the Physical Society.‡ Instead of reprinting these, we now throw the results into a diagram of curves (Fig. 1).

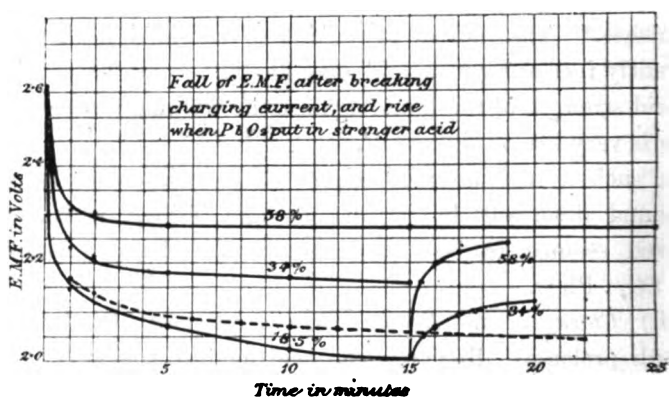

FIG. 1.

For each case, the ordinates represent E.M.F. of the cell in volts, the abscissæ represent time in minutes. The figures attached to the curves show the percentage strength of the acid

* *Proc. Roy. Soc.*, 1883, p. 460.
† *El. Zeit.*, 1889, p. 88.
‡ *Phil. Mag.*, 1890, p. 168.

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round the Pb O_2 plate. The Pb plate was always in 18·5 per cent. acid.

The curves show—First,—That in each experiment the E.M.F. at the commencement of discharge is about the same value, 2·58 or 2·6.

Secondly,—That the fall after stopping the charging current was initially more rapid in the case of the weaker acid. The subsequent and more permanent E.M.F. also depends on the strength of the acid, the value for 58 per cent. acid being 2·27 volts; for 34 per cent. acid, about 2·15 volts; for 18·5 per cent. acid, about 2·01 volts. When the cell has been in repose 15 minutes, the weakest acid gave 0·26 volt less than the strongest, and 0·14 volt less than the 34 per cent. acid.

Thirdly,—That if, when the E.M.F. has fallen, the strength of the acid be increased, the E.M.F. quickly rises, and, as the acid soaks into the pores, eventually attains about the same value as if that particular strength of acid had been maintained throughout.

This seemed to justify our conclusion, but we were desirous of obtaining more direct evidence of the effect of varying strength of acid. In our old experiments we measured E.M.F. by the condenser method, and have again found it useful in many experiments. But we have now made more accurate determinations by the potentiometer, and in one series of experiments by observing the current obtained through a high resistance.

Throughout the new experiments we have employed the same two plates. The supports consisted of thick lead wire doubled on itself. These were pasted with red lead and then "formed" in the usual way, until the paste on one was pure lead, and on the other pure peroxide of lead. The active part of each was about 3 inches long and half an inch diameter. The containing vessel and acid varied according to requirement, as described below for the particular experiments.

We found that on transferring the plates from a weaker to a stronger acid the E.M.F. begins to go up, at first rapidly, afterwards more and more slowly. But the final value is not reached for some hours. It was impossible to allow so much time

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for each observation, and in our earlier experiments we made our determination of E.M.F. at the end of 20 minutes or so. Consequently in these our figures do not represent the ultimate value of the E.M.F. for that strength of acid, but they do not fall much short of it.

In all experiments the plates were fully charged, and before being used were washed free from the liquid products of electrolysis.

1st Series.

In the first series both plates were immersed in acids of the same strength. When one observation had been made, the plates were quickly transferred to another cell containing stronger acid, and the E.M.F. occasionally tested for 15 minutes, when it was finally observed, and the plates again removed into a cell containing still stronger acid, and so on. The following table gives the results:—

Table I.

Acid in Cell round both Plates.		E.M.F. of Cell in Volts.
Density.	Percentage Strength.	
1·045	6·5	1·887
1·065	9·5	1·898
1·080	11·5	1·915
1·115	16·2	1·943
1·157	21·7	1·978
1·217	29·2	2·048
1·254	33·7	2·088
1·333	43·0	2·170

The last acid was found to act vigorously on the spongy lead, so that this series was discontinued.

2nd Series.

The same procedure was followed, with the exception that the Pb plate was kept throughout in acid of density 1·098 = 14 per

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cent. The peroxide was transferred from one to another of a series of porous pots filled with acids of the strength given below :—

Table II.

Acid round Pb O_2 Plate.		E.M.F. in Volts.
Density.	Percentage Strength.	
1·045	6·5	1·926
1·065	9·5	1·932
1·080	11·5	1·939
1·115	16·2	1·949
1·157	21·7	1·963
1·217	29·2	1·986
1·254	33·7	2·013
1·335	43·0	2·061
1·53*	63·0	2·22
1·75*	81·0	2·33

The results of the two series are plotted in the following diagram :—

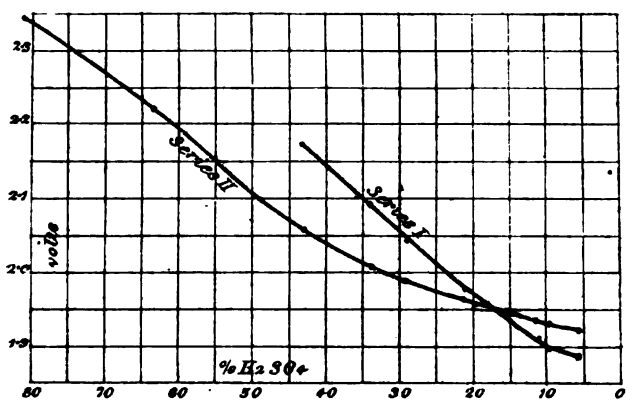

FIG. 2.

* The two last observations really belong to another set of experiments, where the acid round the lead = 23 per cent.

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It will be seen—First, that in both cases the E.M.F. increases with the strength of the acid, although in Curve II. there is no change in the acid at the Pb plate; secondly, there is no coincidence of the curves except where the conditions of the experiments are practically identical; thirdly, that for any given abscissa the E.M.F. in Curve I. is smaller than that in Curve II. when the acid about the Pb plate is less than 14 per cent., and greater when it is more. Hence it follows that the E.M.F. depends on the strength of the acid at both electrodes.

3rd Series.

Not being satisfied with the range or the accuracy of the preceding series, we endeavoured to obtain a curve representing the relation between E.M.F. and strength of acid within the widest limits.

After preliminary trials and considerations, we adopted the following plan. The experiments were divided into two parts, in consequence of the action of strong acid on spongy lead.

In the first part, we commenced with an acid of 5·6 per cent., and worked upwards to 49 per cent., allowing the plates to stand half an hour in each acid before final observation of E.M.F. The process was then reversed, working downwards through the same acids to 5·6 per cent. again, and then continued through weaker acids to a mere trace—less than 0·05 per cent.—and the cycle finished by returning once more to 5·6 per cent. As even half an hour or an hour is scarcely sufficient for equalisation of the strength of the acid within and without the pores of the plates, it may be expected that the ascending series would scarcely represent the full effect of the rise, whilst the descending series would scarcely represent the full effect of the fall. We therefore give in Table III., not only the numbers actually obtained, but also the means of the ascending and descending series, which cannot be far from the true values. We have proof of this in the case of the 5·6 per cent. acid, where the mean is 1·89 volts, while the true E.M.F., as very carefully determined previously for the same strength, was 1·88 volts. This agreement also proves that no appreciable change had come over the plates during the operations.

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Table III.

Time of Soaking.	Density of Acid.	% H_2SO_4.	Electro-motive Force in Volts.		
			Ascending.	Descending	Mean.
1 hour	...	Trace	...	1·507	1·507
„	1·008	1·2	1·747	1·777	1·762
	1·013	2·0	1·792	1·825	1·808
„	1·020	3·1	1·835	1·867	1·851
„	1·037	5·6	1·875	1·908	1·891
30 minutes	1·071	10·4	1·922	1·953	1·937
„	1·154	21·3	1·993	2·016	2·004
„	1·233	31·2	2·055	2·066	2·060
15 minutes	1·388	49·0	2·149	...	2·149

In the second part of the experiments of this series, the Pb plate stood throughout in 27 per cent. acid. We commenced with the Pb O_2 plate in 43·5 per cent. acid, ascended to 88·5 per cent., and then returned to 43·5 again. A longer time was here allowed for diffusion, as the strong acid is very viscid. Notwithstanding this longer soaking, the differences between the ascending and descending series are greater than before. The results are given in the following table:—

Table IV.

Time of Soaking Peroxide Plate.	Acid round Peroxide.		E.M.F.		
	Density.	% H_2SO_4.	Ascending.	Descending.	Mean.
50 minutes	1·338	43·5	2·106	2·163	2·135
50 „	1·446	54·8	2·179	2·233	2·206
50 „	1·569	66·0	2·259	2·298	2·277
60 „	1·605	69·0	2·279	2·342	2·310
60 „	1·723	79·0	2·354	2·398	2·376
60 „	1·814	88·5	2·442	...	2·442

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The mean results of Tables III. and IV. are given in the following diagram :—

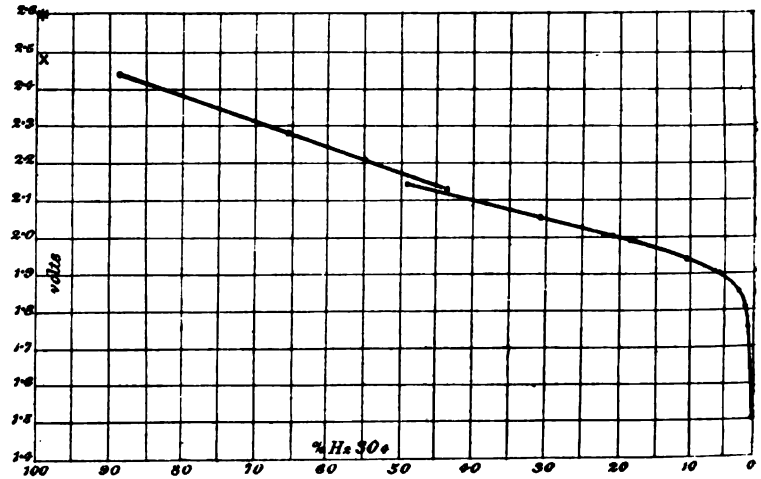

FIG. 3.

The two curves very nearly join, and when it is considered that the highest E.M.F. of the lower curve is almost certainly too low, and the lowest E.M.F. of the upper curve too high, the coincidence is striking.

An attempt was made to get an observation in very strong acid. The fully charged Pb O_2 plate was washed in water, and dried at 100° C. It was then soaked in 95 per cent. acid. After 34 minutes' soaking, the E.M.F. was observed, and the plate then placed in weaker acid. In consequence of absorption of water from the adjacent liquid of the cell, as well as from the atmosphere, the acid round the Pb O_2 plate fell to 91·5 per cent., the Pb plate standing all through in 27·5 per cent. acid. Results are given below, and are fairly confirmatory of the previous figures :—

Table V.

Time of Soaking.	Acid round Pb O_2 Plate.	E.M.F.
34 minutes	91·5 %	2·44
63 ,,	77·5 %	2·37
67 ,,	65·5 %	2·28
97 ,,	53·0 %	2·16

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The highest E.M.F. here obtained (2·44) is very nearly the same as that given in Table IV. for 88 per cent. acid.

A still further effort was made to get an observation in the strongest acid. A $Pb\,O_2$ plate was soaked all night in 80 per cent. acid, and in the morning transferred to a jar containing 99 per cent. $H_2\,SO_4$. This was kept under a closed bell jar for four hours, after which the E.M.F. between the $Pb\,O_2$ in this strong acid and a Pb plate in 25 per cent. acid was measured by potentiometer. The value was 2·47 volts, which, after 30 minutes' further standing, rose to 2·48 volts nearly. This is represented in Fig. 3 by the small cross x.

It was expected that a still higher voltage could be obtained if both plates were immersed in the strongest acid, undiluted $H_2\,SO_4$. The difficulty was that this acid acts pretty readily on the spongy coating of the ordinary Pb plate, so we made an experiment with a strip of ordinary clean sheet lead. Placing this in the same $H_2\,SO_4$ in which the $Pb\,O_2$ plate had been standing for some hours, the E.M.F. was found to be as high as 2,607* volts. This is indicated in Fig. 3 by *.

PART III.—HOW FAR ARE THESE RESULTS CAPABLE OF EXPLAINING ALL THAT IS KNOWN ABOUT CHANGES OF ELECTRO-MOTIVE FORCE?

At the very beginning, it is evident that the experiments described in Part II. more than cover the range of variation in E.M.F. during ordinary work. For these limits are: a maximum P.D. of 2·5 volts at the end of a charge, and 1·8 volts at the end of a discharge—figures which come within the variations produced by strong and weak acid.

But our results go much farther than this; and for the purpose of detailed consideration we shall compare them (as shown in Parts I. and II.) with the valuable observations of Professor Ayrton and his collaborateurs. Their papers in the Journal of the Institution† contain the most elaborate and trustworthy account of working variations of P.D. which has yet been published.

* The same E.M.F. was obtained whether the lead was cleaned by scraping with a knife, or by washing with acetic acid.

† *Jour. Inst. Elect. Eng.*, pp. 545, 661.

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In order to make the comparison more evident to the reader, we give the two following time curves of E.M.F. in charge and discharge. They are deduced from the curves of P.D. values given by Prof. Ayrton and others in the figures opposite p. 661, by making allowance for the resistance as exhibited on pp. 590 and 592. We indicate for certain marked points in the curves the percentage strength of the acid in the general body of the cell as deduced from the specific gravities given on pp. 672 and 673.

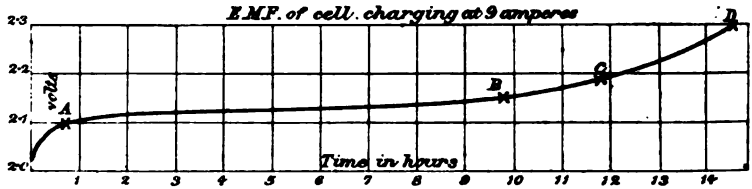

FIG. 4.

Acid in General Body of Cell during Charge.

At Point	Density given.	% H SO_4.
A	1·178	24·5
B	1·198	27·0
C	1·201	27·4
D	1·206	28·0

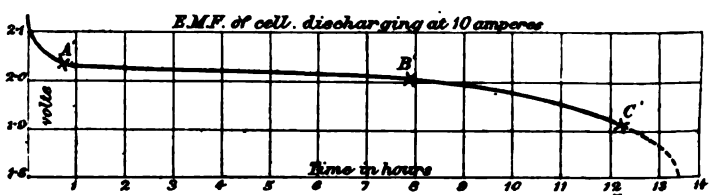

FIG. 5.

Acid in General Body of Cell during Discharge.

At Point	Density given.	% H_2 SO_4.
A′	1·205	27·8
B′	1·189	25·9
C′	1·180	24·8

As far as charging is concerned, we have already shown that sulphuric acid must be continuously formed by the decomposition of the sulphate on each plate, and that the acid must become more and more concentrated, especially against the Pb O_2 plate. If

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our views be correct, this must give rise to an immediate rapid increase of E.M.F.; but as the action proceeds, the tendency of the heavy acid to sink towards the bottom of the vessel, or to diffuse into the weaker intermediate acid, will become greater, and a point will be reached when the production of sulphuric acid against the plate will be nearly counterbalanced by its dispersal. The E.M.F. will still rise slowly, because the intermediate acid is gradually increasing in strength.

This evidently is the tale that is told by Fig. 4. Beginning with an external acid of 24·6 per cent., and a corresponding E.M.F. of about 2·03 volts, there is a rapid rise—so rapid, indeed, that by the end of half an hour the E.M.F. has become 2·1 (which is about equal to what would be given by a 40 per cent. acid at each plate—see Fig. 3), whilst the intermediate acid has not risen, but is only 24·5 per cent. The increase of E.M.F. due to increase of strength of acid against the plates then becomes very slow; but after about nine hours it is found to have risen to what we may look upon as 45 per cent. or more, against the $Pb\,O_2$ plate, while the intermediate acid in the cell has risen to 27 per cent. It subsequently rises against the working surfaces of the plates to 55, and eventually to at least 66 per cent., or probably much more, though the strength is little increased in the body of the cell.

When discharge commences, we have shown that there must be a very rapid weakening of the acid from diffusion and formation of lead sulphate, till the losses are fully counterbalanced from the intermediate acid. There will then be no material reduction till the intermediate acid is considerably reduced or prevented diffusing freely into the pores of the plates. This is the explanation of Fig. 5. We see the rapid fall of E.M.F., the bulk of which takes place in the first few minutes (see Ayrton and others, pp. 545 and 546, and our experiments, Fig. 1). After about half an hour the E.M.F. is reduced to about 2·03 volts, indicating about 25 per cent. $H_2 \cdot SO_4$ against the plates, while the intermediate acid is 27·8 per cent. This state of things lasts for some hours, but the absorption of the free acid that is in the pores of the plates gradually reduces

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the strength there below that of the intermediate liquid, so that after eight hours the internal acid is about 22 per cent., while the external strength has sunk only to about 26 per cent. The acid against the plates then declines more rapidly, so that in four hours more the E.M.F. is equal to that which would be given by 7 per cent. at each plate, while the intermediate acid has scarcely sunk below 25 per cent.

We have already mentioned in Section I. that when the acid is very weak the compound 2 Pb SO_4 . Pb O may begin to form. Should this be the case, the acid absorbed per ampere-hour would be less than before by about one-third. This is about what was found by Dr. Frankland, and described by him in the discussion on the paper by Professor Ayrton and his colleagues (Jour. Inst., xix., 698). Dr. Frankland shows that in some prolonged discharges there was some such reduction of the absorbed acid, and that the diminished rate of absorption began when the voltage fell below 1·8—a figure indicative, on our hypothesis, of a weak acid in the pores of the plates.

We have also shown that if through prolonged discharge the acid in the pores has become very weak, a stoppage of the discharge ought soon to bring up the acid towards what it is outside. The E.M.F. ought therefore quickly to increase to the normal, or nearly so. This rise after rest was observed in very early days, and has been frequently discussed, as, for example, by Messrs. Gladstone and Tribe ("Chem. Secondary Batts.," p. 28), and by Professor Ayrton and others.

We have already seen that if a charged cell be allowed to rest without discharge, the acid in the interstices of the Pb plate is slowly weakened by its action on the lead with the evolution of hydrogen gas. The gas will tend to clog the interstices and impede the stronger acid outside from diffusing inwards.* The lower strength of internal acid will produce a lower E.M.F. than would be obtained if the internal acid were of the same strength as the external. In other words, a discharge after long repose ought to commence with a somewhat low E.M.F. But one effect of

* There may also be a slow formation of lead sulphate on either plate, due to the local action described further on (p. 429).

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discharging will be to remove the impediment to free diffusion produced by the enclosed gas, and thus tend to increase the strength of the internal acid, with a consequent small rise in E.M.F. as the discharge goes on.

Now Professor Ayrton and his colleagues obtained these very results in two instances. The curves representing one of these we have reproduced (Fig. 6) from their paper, with two previous normal curves, *a* and *b*, for comparison.

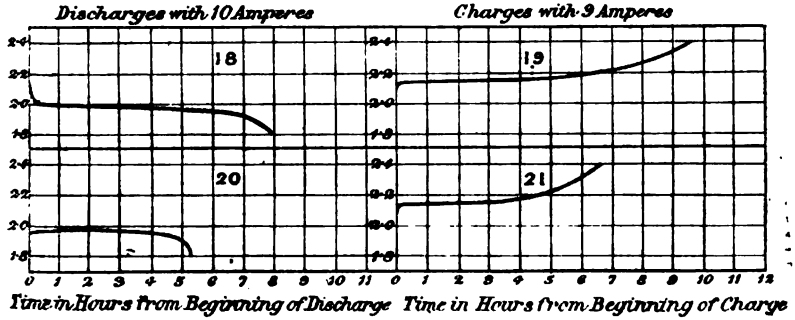

FIG. 6.

In both the instances not only had the P.D. sunk below the normal during repose, but it never quite recovered its normal value, while the fall towards the end of the action took place in about two-thirds of the normal time. On subsequent charging, the process of decomposing the Pb SO_4 lasted for only two-thirds of the usual time. All this points to an early clogging of the pores, and a consequent diminution of the total chemical action.

In this way we explain what the discoverers of the effect regard as a difficulty. The real problem that had to be solved was not, why did the P.D. go up during discharge? but why had it gone down during the many days' repose?

PART IV.—CONFIRMATIONS, THEORETICAL AND EXPERIMENTAL.

1. *Changes of E.M.F. with Two Similar Plates.*—In the discussion that followed one of our papers at the Physical Society, Mr. Hibbert mentioned that if two lead plates are put into acids of different strengths, separated by a porous diaphragm, a voltaic current is produced on completing the circuit.* We have

* *Electrician*, vol. xxvi., p. 154.

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performed this experiment on two lead plates quantitativ
reference to the E.M.F. produced, and have extended the
tions to two peroxide plates.

The method of experimenting was as follows:—A
cell was taken, in one compartment of which was placed s
acid of about 0·2 per cent., in the other an acid varyi
this strength upwards.

A pair of lead plates was tested in the weakest acid,
to ascertain that when immersed in the same liquid t
no appreciable E.M.F. One of the lead plates was then
a stronger acid, and the E.M.F. between it and th
determined by the condenser method. This was continu
a 98 per cent. acid, but, as the stronger acids act fr
spongy lead, the observations beyond 22·5 per cent. we
with two solid lead wires.

The results are given in the following table. The le
in the *weaker* acid behaved all through like a Pb O_2 plat
other, and it is therefore called the + plate.

Table VI.

Acid round + Lead Plate.	Acid round − Lead Plate.	E.M.F. in Volts.
Per Cent.	Per Cent.	
0·2	0·65	0·036
,,	1·35	0·047
,,	2·85	0·060
,,	5·5	0·066
,,	10·5	0·082
,,	14·5	0·094
,,	18·0	0·102
,,	22·5	0·109
,,	36·5	0·150
,,	48·0	0·164
,,	57·5	0·204
,,	85·5	0·247
,,	98·0	0·266

A similar series of experiments was made with two p
plates. It is not likely, however, that the 99 per cen
had completely soaked into the meshes of the Pb O_2
peroxide in the *weaker* acid behaved like a lead plate, a
therefore called the − plate.

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Table VII.

Acid round − Pb O_2 Plate.	Acid round + Pb O_2 Plate.	E.M.F. in Volts.
Per Cent.	Per Cent.	
0·2	0·65	0·054
,,	1·35	0·072
,,	2·85	0·095
,,	5·5	0·107
,,	10·5	0·134
,,	14·5	0·150
,,	18·0	0·158
,,	22·5	0·168
,,	36·5	0·215
,,	48·0	0·281
,,	57·5	0·359
,,	85·5	0·537
,,	99·0	0·643

The results are delineated in the following diagram :—

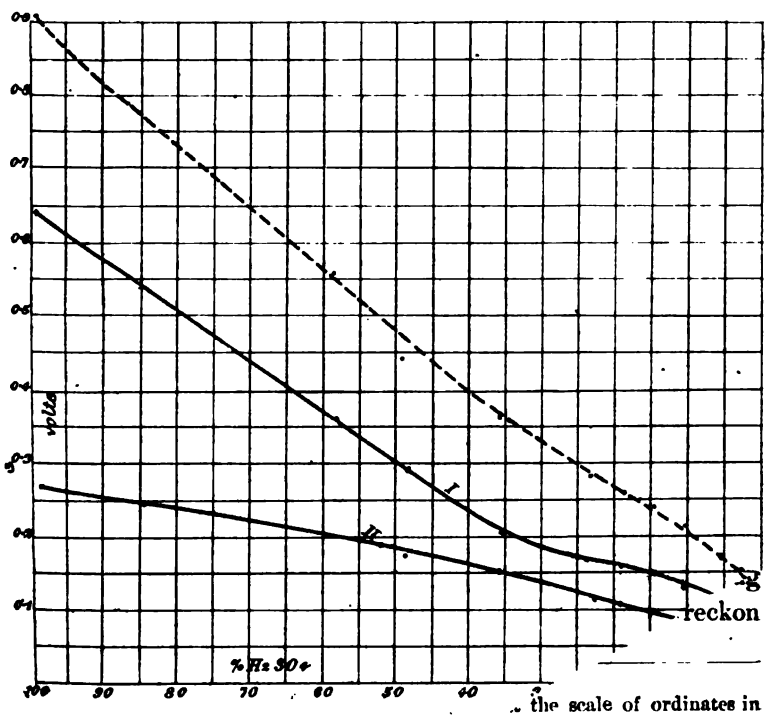

Fig. 7.

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If we consider Curve I. in Fig. 7—which shows the E.M.F. between two *peroxide* plates, one of which stands in sulphuric acid of only 0·2 per cent., and the other in strengths varying from that to 99 per cent.—we see at once that, while it rises through 0·359 volt. it is far from being a straight line. It rises very rapidly at first, then bends over and remains convex till about 18 per cent. is reached, when it becomes somewhat concave till about 57 per cent., after which it is tolerably straight.

If we consider Curve II., Fig. 7—which shows the E.M.F. between two *lead* plates, one of which is in the weak and the other in stronger acids—we find that it follows a similar course, but the ordinates are only about two-thirds as large up to about 30 per cent., after which they are relatively much less.

In Curve II., Fig. 2, we have an experiment in which the peroxide plate was placed in various strengths of acid, while the lead plate was retained in acid of uniform strength, namely, 14 per cent. The observations are only from 6·5 per cent. to 81 per cent., but on comparing it within those limits with Curve I., Fig. 7, it will be at once seen that the curves are identical in form. They are also identical in the absolute amount of the rise. The difference between 6·5 per cent. and 81 per cent. in Curve II., Fig. 2, is 0·404 volt; whilst in Curve I., Fig. 7, the difference is 0·401 volt.

In Fig. 3 we have also observations on a cell with the Pb O_2 plate in various strengths of acid from 43·5 to 88·5 per cent., while the Pb plate remains in acid of 27 per cent. On comparing this with the similar portion of the curve in Curve I., Fig. 7, we find that in both instances we are dealing with a line which is very nearly straight. The rise of E.M.F. in Fig. 3 between the points mentioned is 0·307 volt, and in Fig. 7 it is 0·308 volt.

The close similarity both of the forms of the curves and the amount of the rise in each case, shows that the causes of the phenomena represented in Figs. 2 and 3 are the same as those in Fig. 7 within the range of the experiments. There is, however, one striking difference in the experiments—that while in Figs. 2 and 3 with a peroxide plate and a lead plate, in Fig. 7 with two peroxide plates, and the results can

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be dependent only on the varying strength of acid. This, of course, is a strong confirmation of the theory we have propounded.

It is worthy of note that in the three experiments just compared together, the strength of the unchanged acid round the — plate was very far from being the same, varying, in fact, from 14 per cent. in Fig. 2, and 27 per cent. in Fig. 3, to 0·2 per cent. in Fig. 7. In other words, it does not matter what may be the starting point. This is just what might be expected if we are dealing merely with a differential result.

If we compare Curve I., Fig. 7, with the lower part of Fig. 3, we observe a general resemblance, but with well-marked differences.* In each case there is the rapid rise at commencement, with the subsequent bend over and convexity. But in Fig. 3 the concavity has nearly disappeared. There is also another difference. The rise in this case between 6·5 per cent. and 49 per cent. is 0·251 volt, instead of 0·183 volt in Fig. 7.

These differences are easily explained. It will be remembered that the curve of Fig. 3 represents the variation of E.M.F. due to increasing strengths of sulphuric acid about the peroxide and the lead plate at the same time; while Curve I., Fig. 7, represents the increasing E.M.F. due to increased strength of sulphuric acid solely at the + Pb O_2 plate. In Curve II. of Fig. 7 we have, however, the E.M.F. due to increasing percentages of sulphuric acid round a — Pb plate. Now it has already been stated that increasing the sulphuric acid round the two plates acts in opposite directions in the two cases, so that the potential difference between them in any particular acid will be the arithmetic sum of the separate effects. We have therefore added the two curves of Fig. 7 together, and expressed the result in the same diagram by a dotted curve. It will at once be seen that this combined curve closely resembles the experimental curve in Fig. 3, the concavity having almost disappeared. The rise from 6·5 to 49 per cent. has become 0·27 volt, only slightly exceeding that of the curve in Fig. 3, namely, 0·25 volt. If we reckon

* In such a comparison it must be remembered that the scale of ordinates in Fig. 3 is only half that in Fig. 7.

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the rise from 6·5 to 99 per cent., we get 0·73 volt from Fig. 7, as against 0·71 from Fig. 3.

We have thus resolved the rise and fall of E.M.F. during the charge and discharge of a cell into the two parts of which they are composed, and have determined them quantitatively.

2. *Confirmatory Evidence from Changes in Resistance.*—The general correctness of the conclusions arrived at in Part III. is supported, not merely by the known changes in E.M.F., but also by changes in resistance as given by Professor Ayrton and his colleagues.

It is a matter of common knowledge that sulphuric acid varies very much in resistance according to its state of hydration; that the resistance is least for acid of about 30 per cent., though not changing very widely between 15 and 50 per cent.; and that if the acid become either weaker or stronger than these, its resistance rapidly increases. The following figures, calculated from Kohlrausch's results, will give an idea of the variation:—

Resistance of Sulphuric Acid Solutions.

% H_2SO_4.	Relative Resistance.
2·5	6·73
15·0	1·33
30·0	1·00
50·0	1·35
71·0	3·79
95·0	7·29

Hence we should expect that if the acid against the working surfaces of the plates is being concentrated during charge, or greatly weakened during discharge, there would be a marked increase in resistance. This is exactly what is found to be the case. In the *Journal*, vol. xix., p. 590, is a diagram showing the very rapid increase of resistance during charge, the increase beginning when the E.M.F. (about 2·17) indicates, according to our theory, a strength of sulphuric acid against the working

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surfaces of the plates of somewhere about 50 per cent.; also showing that at the end of the charge "the resistance is five "times as great as its minimum value."

This is particularly worthy of remark, because every other chemical change would tend to diminish, instead of increasing, resistance. The $Pb O_2$ produced on the one plate, and the metallic lead produced on the other, are both much better conductors than the $Pb SO_4$ which they replace.

On page 592 of the same volume is another diagram, showing that when the discharge of this cell was begun the E.M.F. had fallen from 2·30 to 2·06 volts; and the resistance, which at the end of the charge was 0·0115 ohm, had fallen to 0·0038. We attribute both these falls to the same cause, namely, the reduction of the internal acid to about 30 per cent. Subsequently, the E.M.F. slowly diminished, while the resistance remained nearly the same for about five hours, when the E.M.F. more rapidly fell to 1·95 volts, and the resistance rose to 0·0055 ohm.

This is more than would be expected from the above table of resistances, but it must be remembered in this case that the other chemical changes—that is to say, the conversion of $Pb O_2$ and Pb into $Pb SO_4$ would also tend to increase, instead of diminishing, the resistance.

We cannot lay great stress on precise numerical relations in the case of resistance, as the change in the strength of the acid at the working surfaces is only one of the factors, though an important one.

3. *Confirmation from Mr. Crompton's Experiments.*—During the discussion on the papers by Professor Ayrton and his colleagues, Mr. Crompton described two series of experiments which have a bearing on this subject.* In the first series some cells were discharged at rates varying from 11 to 66 amperes, and Mr. Crompton found that the ampere-hours delivered with a given range in P.D. fell from 300 to 125. He adds: "In every case the "form of the curve is very definite, the turn-down as soon as the "electro-motive force falls to 1·8 being very marked."

* *Jour. Inst. Elec. Engineers*, vol. xix., pp. 691, 692.

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This follows naturally, because at the higher rate of discharge the absorption of the acid in the pores has become much quicker, while the diffusion is scarcely affected, so that the weakening process goes on much faster. The critical voltage of 1·8 simply indicates that the internal acid has become very weak. Mr. Crompton's curves and remarks show clearly that the fall is independent of the amount of Pb O_2 remaining on the plate.

In the second series of experiments thicker plates were used, and then he found a greater diminution of delivering capacity at the higher rates of discharge—in fact, it fell from 300 to 90 ampere-hours, and at the highest rate the fall began almost at once. This must be the natural result of the greater distance through which the entering acid has to diffuse.

4. *Confirmation from Thermo-Chemistry.*—It is possible to test the matter further by applying Lord Kelvin's law as to the relation between the E.M.F. of a cell and the thermal value of the chemical actions contributing to it. We hope to go into this matter more fully, and shall content ourselves at present with pointing out that the liquid in a secondary cell is a mixture, or a chemical compound, of two different liquids—sulphuric acid ($H_2 SO_4$) and water, in varying proportions.

The simple problem is to determine what would be the voltage of a Pb O_2 — Pb cell in which there was nothing but pure $H_2 SO_4$.

From the thermo-chemical data which are before us we arrive at the value 2·627 volts. Our own determination, by means of the closest approximation which we could make to absolute $H_2 SO_4$, is 2·607 volts.

With pure water only in the cell, the calculated value is 1·35 volts, whilst in an experiment we found 1·36 volts.

In determining the thermo-chemical values for mixtures of these liquids, it is necessary to subtract the heat of dilution from the available energy. On doing this, the calculated and experimental numbers do not agree so well as those already given, until we come to the most prominent part of the curve at about 6 per cent. acid. The theoretical value at this point, as determined from the known heats of combination and dilution, would give 1·901 volts, the experimental value being 1·89 volts.

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With lower figures than 6 per cent. it is evident that the change is not to be accounted for on thermo-chemical grounds, unless we admit a change in the chemistry as we approach pure water—a conclusion already drawn from other data (see p. 417).

It is now easy to understand the large P.D. required for charging an accumulator. The current has to do extra work in concentrating $H_2 SO_4$ at the $Pb O_2$ plate, and the energy equivalent to that work must be obtained from an increased P.D. For a dyad gramme equivalent of $H_2 SO_4$ concentrated from a 10 per cent. solution to 100 per cent. about 17,000 calories will be needed, and this = 0·37 volt. The calculated charging E.M.F. must therefore be at least 2·3 volts.

Part V.—Consideration of other Suggested Causes of the Variation of E.M.F.

As other causes have been suggested for the changes in E.M.F., it is desirable to consider how far they are in accordance with the known facts of the case. It is evidently possible that the phenomena may be due to a number of causes co-operating.

1. It might be supposed that the reduction of the E.M.F. in discharge is determined by the relative amount of lead peroxide, which is destroyed or covered over with sulphate of lead.

This is absolutely disproved by the experiments of Professor Ayrton and his colleagues, which are graphically represented in Figs. 1 and 2, given on p. 661, vol. xix. of the *Journal of Electrical Engineers*. It will there be seen, from the determinations of the percentage of $Pb O_2$ found on plugs removed, that the formation of the $Pb O_2$ in charging, and the decomposition of it in discharging, is a fairly regular and continuous action. It gives no indication of the rapid changes at the commencement and termination of charging and discharging; while during the intermediate period, when there is little change of the E.M.F. for many hours, the rate of variation in composition is steady. This is indicated, as far as the discharge is concerned, by the dotted line in our Fig. 4, which is reproduced from the paper by Messrs. Ayrton, Lamb, Smith, and Woods. (See also remarks on Mr. Crompton's experiments, *ante*.)

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2. Planté considered that the exceedingly high E.M.F. observed for the first few minutes on joining up a completely formed cell, immediately after its removal from the charging circuit, was due to the gaseous hydrogen found on the Pb plate. Gladstone and Tribe,* while considering that this was possible, drew attention rather to the hydrogen occluded by the lead as a possible cause, but stated at the same time that this occluded hydrogen was exceedingly small in quantity. Frankland showed by a totally different process that it was practically *nil*.

But hydrogen on one plate and oxygen on the other would not, under the conditions, account for as much as 2 volts, and this explanation is therefore inadequate.

3. Planté observed that a small quantity of lead peroxide is formed on the Pb plate during discharge; and Gladstone and Tribe found in this a reason for the state of electric equilibrium being approached before the peroxide on the PbO_2 plate is exhausted, and also for the fact that partly discharged accumulators give an increased current after repose ("Chem. "of Sec. Batts," pp. 27, 28). This last action is attributed to the extreme rapidity with which the lead peroxide formed on the Pb plate must be destroyed by local action.

Mr. Robertson has recently added to this the observation that the formation of peroxide of lead on the Pb plate does not take place till the E.M.F. has fallen much below the normal value.†

We have already attributed the resuscitation of the E.M.F. on repose to the inflow of the stronger acid to the acting surfaces of the opposed plates, but we must look upon this formation of peroxide of lead, where it does occur, as contributing both to the reduction of E.M.F., and its resuscitation.

4. *Presence of some Form of "Active Oxygen."*—It is well known that electrolysed sulphuric acid contains some hydrogen dioxide, which is probably due to the decomposition of persulphuric acid by water.

Gladstone and Tribe showed that the presence of hydrogen

* "Chemistry of Secondary Batts.," p. 48.

† Robertson's report to Professor Ayrton, &c., *Jour. Inst. Elec. Engineers*, vol. xix., pp. 670-675.

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dioxide in a cell must reduce the peroxide of lead—an observation confirmed by Robertson (*Proc. Roy. Soc.*, vol. l., p. 107).

Last June, both Mr. Robertson and Professor Armstrong communicated papers to the Royal Society, in which they attach great weight to the presence of peroxides in the electrolyte, as causing a loss of efficiency. As only abstracts of these papers are published as yet, we cannot enter into any criticisms of their experiments. Mr. Robertson, however, in a lecture to the Society of Arts (*Jour. S. A.*, xl., p. 44), states that the "variations " in E.M.F. appear to depend on which plate hydrogen dioxide is " found at. When present at the peroxide plate it causes a rise, " but when diffused through the acid and present at the lead " plate, it causes a lowering of the E.M.F."

We therefore made several experiments with the addition of hydrogen dioxide (both ordinary, and carefully purified from hydrochloric acid). Our arrangements admitted of adding the hydrogen dioxide to the electrolyte surrounding either the peroxide or the lead plate. Some of the determinations of E.M.F. were made by a condenser, and others by observing the current through an astatic galvanometer in series with a high resistance.

We generally obtained a slight reduction (about 0·02 of a volt), but only what might be fairly attributed to the dilution of the sulphuric acid.

It was still possible that some other form of "active oxygen" might accomplish what hydrogen dioxide had failed to do; and in order to determine whether these peroxidised products of electrolysis have really a different effect at the cathode and anode, we completed the charging of a pair of plates in a divided cell filled with 20 per cent. acid, and found that the "peroxides," or "active oxygen," existed only round the Pb O_2 plate. We then reversed the plates in the compartments, so that the Pb plate stood in the liquid containing the peroxides ($H_2 O_2$, persulphuric acid, &c.). One minute after stopping the charging current, and immediately before the reversal of the plates in the cell, the E.M.F. was 2·185 volts. After the reversal of the plates, the

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E.M.F. was measured by potentiometer at intervals, the results being given in the following table:—

Time after Reversal of Plates.	E.M.F. in Volts.
Minutes.	
1	2·163
6	2·081
8	2·078
10	2·066
12	2·063
15	2·055
22	2·044
45	2·031

There was evidently here no unusual fall in the E.M.F. The figures are embodied in the dotted curve of Fig. 1, and on comparing it with the fall for 18·5 per cent. acid they will be found so similar as to show that peroxidised compounds round the Pb plate had no sensible effect. In each case the voltage evidently fell very slowly to a uniform value, and that value agrees very fairly with what we find for a 20 per cent. acid in Fig. 3. Through the kindness of Dr. H. Marshall, we were able to test the effect of adding some persulphate of potassium to the sulphuric acid in a cell about the Pb O_2 plate, but without visible effect on the E.M.F. As far, therefore, as our own experiments are concerned, the E.M.F. appears to depend on the acid strength of the electrolyte, and not on the existence or the position of any peroxidised bodies dissolved in it.

ADDENDUM.

Although we did not undertake this investigation with the object of improving secondary batteries, there is one suggestion we should like to make—*i.e.*, the desirability of promoting diffusion as much as possible. We believe that this is becoming more and more the practice among those who make accumulators,

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and the previous considerations furnish three reasons for such a procedure.

It is well known that the accumulation of stronger acid at the lower part of a cell during its working is disadvantageous. This is believed to create differences of current-density in different parts of the plate, and we have shown that it will also give rise to potential differences of fairly large value on each of the plates, and thus produce local action and the formation of lead sulphate. This inequality would be diminished if the diffusion of the acid could be promoted.

The fall of E.M.F. at the close of discharge leaves a large fraction of the effective material not acted upon. This is mainly due to the weakness of the acid against the plates on account of the interstices being so much clogged; and it would be counteracted to a considerable extent if the diffusion could be increased.

When a cell has been discharged at below 1·8 volts, there occurs the destructive action called "scaling." We are disposed to attribute this to abnormal chemical action arising from the very weak acid; and if this be true, increased diffusion would in this case also act as a remedy.

Such increased diffusion might be obtained either by agitation or by heating, and we thought it interesting to try the effect of higher temperature on the output of our small cell. The following curve (Fig. 9) exhibits the result of one experiment:—

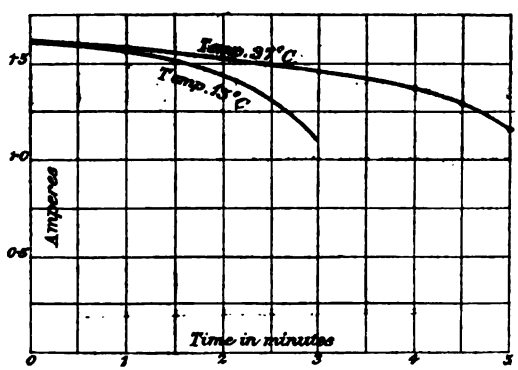

FIG. 8.

The discharge curves at temperatures 15 or 20 degrees higher

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than the ordinary, generally showed an output some 40 to 50 per cent. greater. But it is evident that the higher temperature would increase local action and the chemical action of the acid upon the spongy lead. This might be met by a reduction in the strength of acid, but we have not tested at what temperature and with what strength the advantage is at a maximum.

In addition to this suggestion, electricians will doubtless be able to make other useful applications of our conclusion that the changes of E.M.F. in a secondary battery depend on the strength of the acid that is against the working surfaces of the plates.

The discussion was postponed until the next meeting.

The PRESIDENT announced that the ballot had resulted in the election of the following candidates:—

Foreign Members:

Dr. R. Sissingh. | Tómas L. Taylor.

Member:

Conway Osborne Grimshaw.

Associates:

Frank Bryan. | Mark Ruddle.
Alexander Hunter Robinson. | Robert A. Smith.

Students:

Hubert Hallam Bigland. | Alfred R. Sillar.

The meeting then adjourned.

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The Two Hundred and Thirty-ninth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 19th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 12th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council, viz.:—

From the class of Associates to that of Members—

Alexander Mackenzie Williams.

Messrs. H. C. Donovan and Paul Dimier were appointed scrutineers of the ballot for new members.

The following paper was then discussed:—"The Cause of the "Changes of Electro-motive Force in Secondary Batteries," by Dr. J. H. Gladstone, F.R.S., Member, and Walter Hibbert, F.I.C., Associate.

Mr. G. H. ROBERTSON: On behalf of Dr. H. E. Armstrong and myself I beg to offer the following remarks:—Messrs. Gladstone and Hibbert's paper is undoubtedly an important contribution to the discussion of a very complex and difficult problem; but the verdict on it, we think, must be one of "Non-proven," as we are free to admit is the case as regards our own recent attempt to display the inner workings of the lead-sulphuric-acid lead peroxide cell. While accepting their results, we must, at present, decline to accept their interpretation of them; indeed, we think we are justified in claiming them in support of the explanation we have advanced in our Royal Society papers, which, it would seem, the authors in a measure seek to traverse. Mr. Robertson.

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Mr. Robertson.

Divested of all details, their contention would appear to be that, practically, action is limited to the occurrence of the series of interchanges pictured by the equation,

$$\mathrm{Pb} \,\Big|\, \underset{\text{aq.}}{\mathrm{SO_4\,H_2}} \,\Big|\, \underset{\text{aq.}}{\text{————}}\, \mathrm{SO_4\,H_2} \,\Big|\, \frac{\mathrm{O\,Pb\,O}}{\underset{\text{aq.}}{\mathrm{SO_4\,H_2}}} =$$

$$\mathrm{Pb\,SO_4} \,\Big|\, \mathrm{H_2} \underset{\text{aq.}}{\text{— — ————}} \mathrm{SO_4} \,\Big|\, \mathrm{H_2O} \,\Big|\, \frac{\mathrm{Pb}}{\underset{\text{aq.}}{\mathrm{SO_4}}} \,\Big|\, \mathrm{O\,H_2};$$

and that the E.M.F. developed is directly a function of the concentration of the acid. They do not deny that peroxides are formed, but appear to consider that the part which these play is so entirely subordinate that it need not be taken into account.

Variations in E.M.F. from about 1·5 to 2·6 volts have been observed by Messrs. Gladstone and Hibbert, according as the plates (sometimes both, sometimes only one) were in a liquid containing but a trace, or consisting all but entirely, of sulphuric acid.

The decision must turn on the question, Are these values pure or impure values? Is the variation due to the influence of varying amounts of water, present in acids of different strengths, on the energy developed in the formation of lead sulphate, as the authors appear to assume it is? or are the values throughout impure in consequence of the operation of "polarisation" effects to an extent which varies with the concentration of the acid?

It is almost inconceivable to us that variation in concentration should produce an effect such as is observed, if the chemical action consist merely in the direct formation of lead sulphate from acids of varying strength.

Messrs. Gladstone and Hibbert make no reference to the observations of Messrs. Duncan and Wiegand, confirmed by Professor Ayrton and his coadjutors, that the acid cools during discharge to a marked extent, which appears to us to have a most important bearing on the question at issue. We are at a loss to understand how they will explain this. It appears difficult at present to attribute this to any other cause than *dissociation* consequent on the withdrawal from the solution either of

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molecules of sulphuric acid itself, or of molecules which are hydrated to but a slight extent. In other words, we think it probable that the formation of lead sulphate takes place, not at the expense of the acid of the concentration in contact with the material undergoing change into sulphate, but at the expense of free molecules of H_2SO_4 present in the acid liquid, whose withdrawal conditions the appearance of a fresh supply, the separation of which from their association with water molecules necessarily involves cooling. We question whether lead sulphate is formed except by the agency either of sulphuric acid itself, or of its simplest hydrates. Mr. Robertson

In the next place, it is a highly noteworthy circumstance that, according to Professor O. J. Lodge (*The Engineer*, vol. liv., p. 30), not only is a certain strength of acid requisite for the formation of peroxide of lead, but also a certain E.M.F. is required. Moreover, visible evolution of oxygen attends its formation. As the electrolysis of sulphuric acid is always, it would seem, accompanied by the formation of peroxides, unless the acid be extremely dilute, it appears probable that there is a connection between the formation of the peroxide of lead on the plate and that of the peroxides in the acid, and, indeed, that they are inseparable. The dissolved peroxides appear to continually undergo spontaneous destruction, and therefore the amount present at any time affords no clue as to the amount actually produced.

Moreover, McLeod's experiments, published in 1886 (*Jour. Chem. Soc.*, 1886, p. 591), on the electrolysis of solutions of sulphuric acid of different strengths, show that the amount of "peroxide" in solution rapidly increases as the concentration is increased until a relative density of 1·25 is reached, after which it diminishes: whether this be due to a difference in the amount formed, or to a difference in the stability in the case of different acids, it is at present impossible to say.

F. Richarz (*Ann. Phys. Chem.* (2), 183-209) confirms these results, and agrees that the products of electrolysis vary with the strength of acid employed. According to this observer, in the case of acid containing 10 to 20 per cent. H_2SO_4 the proportion of oxygen as persulphuric acid is small, and no hydrogen

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Mr. Robertson.

peroxide is formed; with an acid containing 20 to 40 per cent. the proportion as persulphuric acid increases; while with acids containing 40 to 90 per cent. the proportion of oxygen as persulphuric acid decreases, whilst that of hydrogen dioxide increases.

The same author has also shown (*Zeit. Physikal. Chem.* (4), 18-30) that the E.M.F. corresponding to the formation of persulphuric acid is 0·57 volt.

Dr. Gladstone has had the kindness to inform one of us that—

"1. In every instance, after charging the plates we immersed "them in a fairly large quantity of dilute acid, and allowed them "to remain about a couple of hours for diffusion; sometimes "another washing in a second acid followed.

"2. The plates were used 'straight on.' There was no "recharging between the reading in one acid and the reading "in the next."

Judging from the great difficulty which was experienced in freeing plugs taken for analysis from acid (cf. this Journal, vol. xix., p. 663), the treatment described would scarcely suffice to eliminate "peroxides" formed during the previous charge. As Dr. Gladstone also says that there was no recharging, we think there can be no question that pure sulphuric acid solutions cannot have been dealt with, and that "peroxides" were necessarily included in the measurements, and to an extent which varied with the strength of acid used.

Two series of experiments are described by the authors in which the E.M.F. developed, on the one hand between two lead plates, and on the other between two peroxides plates, in acids of different strengths, is measured. They appear to regard these results as very important, as they go so far as to draw conclusions from a curve obtained by integrating the two sets of observations. If the origin of the E.M.F. measured in the two cases be considered, it appears to us that no real weight can be assigned to the results in the discussion of the question at issue (*i.e.*, whether variation in the dilution of sulphuric acid affects the energy developed in the formation of lead sulphate).

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Mr. Robertson.

In the case of lead, the action must be complicated by the effect of dissolved atmospheric oxygen. We imagine that it is impossible for a current to arise between pure peroxide plates in pure sulphuric acid, concentrated or dilute. We presume, however, that the peroxide plates were backed with lead, in which case there would be no difficulty in understanding the result; and also that although of two *lead* plates that in the weaker acid behaved like a peroxide of lead plate, of two *peroxide* plates that in the weaker acid behaved as a lead plate. The stronger acid would act more powerfully on lead, and therefore the plate in the stronger acid would be attacked; but in the case of peroxide on lead supports, local action would set in and be at a maximum in the stronger acid, and therefore the lead plate of this couple would be more protected, and the support opposite would be more active against the peroxide of this plate.

Our view that the support is capable of coming into action is confirmed by the fact that the highest reading of E.M.F. obtained by Messrs. Gladstone and Hibbert was observed with a plate which had been dried at 100° C. and then steeped in strong acid, and in which, therefore, there was the densest layer of protecting lead sulphate, the presence of which Messrs. Gladstone and Tribe have shown renders it possible to support peroxide of lead on a lead plate.

The values obtained were therefore, we venture to think, what we have termed impure values; *i.e.*, they correspond to no simple effect.

With regard to the experiments described as undertaken for the purpose of ascertaining the effect of the presence of some form of "active oxygen," Mr. Robertson would point out that (doubtless for the reasons given in their paper) Messrs. Gladstone and Hibbert did not follow his procedure in making the experiments. In the original experiments on the subject of the effect of H_2O_2 on a peroxide of lead-lead couple, the plates had not been formed by electrical action, as in the experiment described by Messrs. Gladstone and Hibbert; and in the case of the experiment of transferring the plates, not only must the acid next the lead plate be free from peroxides, but the plate must not be much formed, or the spongy material has sufficient power of

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Mr. Robertson.

reducing the peroxides to prevent the experiment being successful. In the case of a well-formed cell, the addition of a little more "active oxygen" to that already present in the electrolyte has very little effect.

With regard to the effect of heating the cell, to which reference is made in the addendum, it may be mentioned that this has already been made the subject of a patent. Messrs. Crova and Garbe, moreover, have pointed out the unsuitability of thick plates for a high rate of discharge. (*Compt. Rend.*, vol. ci., pp. 240-243).

The benefit arising from heating is perhaps due rather to the well-known effect which rise in temperature has in lessening polarisation in the electrolysis of sulphuric acid solutions (S. P. Thompson, *Jour. Soc. Arts*, vol. xxx., p. 35) (which has been ascribed to the difference in the products at different temperatures), than to the circulation produced. This has been rendered probable, we think, by Mr. Crompton's experience (*The Electrician*, vol. xxvi., p. 263) that during cold weather the efficiency of his batteries fell to 66 per cent., and rose at once to 79 per cent. on providing means for maintaining a higher *uniform* temperature.

In our Royal Society paper, after stating "that in all "probability sulphuric acid (H_2SO_4), and not the dilute acid "contained in the cell, is operative throughout," we said that "on the assumption that sulphating occurs at both plates in "circuit, and under the influence of H_2SO_4, the calculated value "is considerably too high—that is, compared with observed "working values—while, if sulphating occurs only at the lead "plate, the value calculated is far too low."

But we further added "that a counter E.M.F. of about 0·5 "volt [this was intended as a mere approximation] would account "for the observed departures from the highest calculated value. "As peroxides are always present in the electrolyte, it is "conceivable that such a counter E.M.F. may exist; moreover, "there is the possible influence of the lead support to be "considered."

Further consideration of the matter has since led us more and

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more to favour the view that a considerable counter E.M.F. is operative throughout discharge, which is conditioned by the "peroxide" in solution at the lead peroxide plate. Its presence cannot be denied, and if present, it would seem that it must be operative. Mr. Robertson

The explanation that we should give of the variations observed by the authors will be obvious from these remarks.

But the importance which we attach to "peroxidation" will be made still more obvious if we call attention to what we regard as the probable primary change during charge. In our Royal Society paper, after stating that "the properties of lead sulphate "are such that the chemist must necessarily hesitate to accept "the conclusion that it directly undergoes oxidation," we said, "it "appears to us probable that, either by the direct addition of "SO_4, or by the electrolysis of acid sulphate, the lead sulphate is "converted into persulphate, which interacts with water, affording "lead peroxide and sulphuric acid."

We may represent the change during charge in the following manner, assuming that acid sulphate is present at both plates:—

$$Pb\begin{matrix}SO_4\,H\\SO_4\,H\end{matrix}\Big|\,SO_4\begin{matrix}H\\H\end{matrix}\Big|\begin{matrix}H\,SO_4\\H\,SO_4\end{matrix}Pb = Pb\begin{matrix}SO_4\\SO_4\end{matrix}\Big|\begin{matrix}H\\H\end{matrix}SO_4\Big|\begin{matrix}H_2\,SO_4\\H_2\,SO_4\end{matrix}\Big|\,Pb\,;$$

and then

$$Pb\begin{matrix}SO_4\\SO_4\end{matrix} + \begin{matrix}H_2\,O\\H_2\,O\end{matrix} = Pb\,O_2 + \begin{matrix}H_2\,SO_4\\H_2\,SO_4\end{matrix}.$$

It is conceivable, however, that the peroxide is formed directly, and that persulphate is but potentially present.

The formation of persulphuric acid during both charge and discharge may, at least in part, be due to a secondary interaction of lead persulphate and sulphuric acid—

$$H_2\,SO_4 + Pb\begin{matrix}SO_4\\SO_4\end{matrix} = H_2\,(SO_4)_2 + Pb\,SO_4\,;$$

the formation of persulphate at the lead plate during discharge being, in like manner, possibly due to oxidation of acid sulphate.

We append these remarks especially with the object of emphasising our opinion that our knowledge of the chemistry of the cell under discussion is as yet far from complete.

Mr. R. E. CROMPTON: Dr. Gladstone's paper, as a further contribution to our stock of knowledge of accumulators, is of Mr. Crompton.

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Mr. Crompton.

great value. He has touched upon certain points of which I had previously a general knowledge, but which neither Mr. Howell or myself had worked out quantitively, as the author has done in his paper. So clearly did Mr. Howell and myself see how important it was that means should be provided to prevent the electrolyte becoming of unequal density, whether through the action of charge or discharge, that we, six years ago, took out a patent for various methods of introducing artificial means of circulation, so as to equalise the strength of the electrolyte throughout the cell. Although we found that the use of such artificial agitation was hardly practicable in secondary cells of ordinary size, yet we *did* then note and put into practical use electrolyte of the density mentioned in page 10. It is there shown that dilute sulphuric acid, containing 30 per cent. of acid, and answering to a specific gravity of about 1,245 to 1,255, has a lower electrical resistance than electrolyte of higher or lower density and specific gravity. We have during the past six years utilised this fact in order to make the electrolyte self-regulating and self-circulating. It will be easily understood that any increase or decrease of specific gravity above the normal, having the selfsame effect of increasing the resistance of the electrolyte, produces a self-regulating effect, as it prevents the action being intensified or weakened by the corresponding concentration or weakening of the electrolyte at the same points.

Although I feel, personally, grateful for Dr. Gladstone's paper, yet I do not wish to accept his results as fully proved until they have been borne out by further experiments carried out on a larger scale. Mr. Howell, Mr. Claus, and myself have been working for so many years at the subject, that we know too well how contradictory are the results, and how impossible it is to lay down any laws on those obtained from experiments on small cells. For this reason I am one of those who believe that no quantitive experiments are of substantial value until they have extended over some years. During the seven years we have been working on this subject, we have determined some facts with reasonable certainty, and on them we have based our rules for the manage-

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ment of accumulators, which it is now generally known have been very successful. One point we have determined with considerable accuracy is the space that ought to be occupied by the electrolyte between the plates in order to obtain the advantages of natural circulation. If we state that the space occupied by the plates themselves, without taking into account their porosity, is 100 cubic centimetres, then the space that should be occupied by the electrolyte immediately between them should be about 225 cubic centimetres. Mr. Crompton.

Another point that we have determined has been the degree of porosity we could safely allow in the material of which the plates are made, so that we might, on the one hand, obtain the advantage of each individual crystal of which the plates are composed being freely bathed by the electrolyte, and, on the other hand, avoid the difficulty of making the plates too weak from a mechanical point of view; as it is evident that as we increase the porosity by increasing the size of the crystals, so do we weaken the plates mechanically, so that they are liable to be damaged in handling or from other mechanical causes. This shows that the weaker the plates are mechanically, through excessive porosity, the better they are from Dr. Gladstone's point of view, as giving greater facility for equalising the density of the electrolyte, and hence maintaining the E.M.F.

Another point I wish to introduce is, how little is known by the world generally on the subject of how much the E.M.F. of a cell measured on open circuit varies with the strength of the electrolyte. At a recent trial, several scientific witnesses appeared to consider that the fact that a certain cell measured on open circuit gave 2·08 volts, showed that it was nearly fully charged; whereas the fact was that this E.M.F.—higher than that to which they were accustomed—was due simply to the use of electrolyte of greater strength than that to which they were used to work with.

With reference to the point raised by Dr. Gladstone, at what point of density the electrolyte is liable to attack the delicate crystals of spongy lead on the negative plates even on open circuit, we believe we have determined this as follows:—At

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Mr. Crompton.

ordinary working temperatures—that is to say, under 75 degrees Fah.—we find the plates are not so attacked so long as the electrolyte does not exceed a density of 1,260.

I am anxious to know if Dr. Gladstone, in his reply, can account for the very curious fact we have always observed, that whenever our own form of accumulator cells are discharged at a rapid rate, after the first rapid fall of P.D., on commencement of discharge the P.D. again rises somewhat as follows:—Say the fall is from 2·08 down to 2·01, there will be a subsequent rise to 2·03, followed by a gradual fall. I think this phenomenon is almost invariably noticed.

I wish to call attention to a foreign contribution to this subject which ought to be read attentively by those who are studying this matter—that is, a paper contributed to the *Electrotechnische Zeitschrift* of January 8th of this year by Dr. Krügel, in which he points out the cause of that bugbear to all accumulator users, namely, the self-discharge of the negative plates when on open circuit. Dr. Krügel's experiments appear to have been carried out on a very large scale, and I think he has proved his case in attributing this self-discharge entirely to the metallic impurities originally present in the sulphuric acid used for the electrolyte. I think that the reason why we English users have failed to discover this in the past has been that we have never analysed our electrolyte in order to discover these impurities at the right time of discharge. I believe that up to a very late period of the discharge the greater portion of these metallic impurities remain deposited on the negative plates, and it is only at a particular moment—practically that of the commencing of reversal—that it is possible to get them again into solution and so detect or remove them. The importance of preventing such self-discharge is very great; I believe it is one of the principal causes of the occasional troubles which private users of accumulators experience. Negative plates in a few of the cells suffer from this defect, and as a result the users find that they cannot discharge their battery to anything near its capacity without the danger of reversing the cells containing these plates. The reversals that do take place with these cells is

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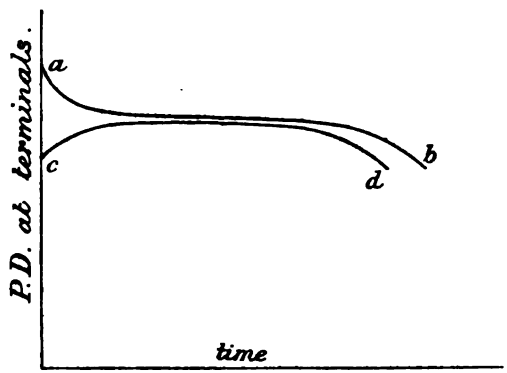
the principal cause of the sulphation and destruction of the positive plates in the same cells. Mr. Crompton.

Mr. E. WYTHE SMITH: A simple theory, such as Dr. Gladstone and Mr. Hibbert put before us, to explain what appear to be complicated phenomena, has such a fascination, that one hesitates in differing from it. But as the authors themselves start by taking certain modifications produced by alterations in density, we can go a little further. The first point which I wish to speak on is: In our Edinburgh paper we called attention to the fact that after the cells have rested charged, the potential difference rises during the commencement of discharge. Not only does it rise the first time, but, after charging up again, a similar effect in the next discharge is noticed, but to a less extent. The normal discharge curve has the shape *a b* (see figure). After Mr. Wythe Smith.



rest, the first discharge curve is of the shape *c d*; the next discharge is of a shape between the two, showing that the rest affects more than the first discharge.

With regard to the decay of the positive grid: This, I think, is not due to the formation of the sub-sulphate that the authors mention; but one must look to the negative plate to discover the cause. The effect of rest, we showed, was the same as would be produced by removing part of the lead plate from the battery, and so diminishing its capacity, the lead lead plate having sulphated owing to local action. Now, in practice, to get rid of this sulphate one has to overcharge the cells considerably and

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Mr. Wythe Smith.

continually. This continual overcharge of the positive plates bites further and further into the grid, and slowly turns the grid into peroxide, as in a Planté cell.

If, as Dr. Gladstone and Mr. Hibbert point out, there is a considerable amount of local action on the positive grid at the end of charge during rest, should there not be a great deal more during charge itself, when the sulphuric acid is of great density, the grid with the plugs forming a short-circuited battery?

On page 417 the authors say: "The rate of weakening of the "acid is a constantly increasing one, and may finally become so "rapid that the acid strength of the liquid against the working "surfaces is very low or almost *nil*. In such a case we may "expect the formation of" a substance which gives the analysis, $2\,Pb\,SO_4 . Pb\,O$. Now, if the discharge is carried beyond this point continually, discharge after discharge, a certain amount of mud comes from the positive plates and settles at the bottom of the cells. One would expect that, from what the authors say, this matter consists of the basic compound just mentioned. We tried the experiment, and collected the mud, but, upon analysing, we found that it did not consist of $2\,Pb\,SO_4 . Pb\,O$, but might be represented thus: $Pb\,O_2 . Pb\,SO_4$—one molecule of peroxide to one molecule of ordinary sulphate. It is interesting to note that this very nearly agrees with the analysis of the positive plugs at the end of discharge; and that in itself will account for the fact that we have to use a much larger positive plate than one would expect from electro-chemical data. Because, when the positive plate is discharged to this formula, the peroxide will no longer hold together, but will drop out of the plates.

Local action takes place at the end of discharge, when the acid is weak, much more rapidly than at the end of a charge, when the acid is strong, and also takes place in a different manner. Local action at the end of a discharge, if the cell is allowed to rest after discharging, has the effect of increasing the time of the next charge; allowing the cell to rest charged apparently diminishes the capacity of the negative plates only.

Dr. Gladstone and Mr. Hibbert mention the fact that, on

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calculating the E.M.F. from thermo-chemical data, they did not get results which agreed with their experiments. Now, we showed, as Professor Duncan and Mr. Swinburne have also concluded, that there is a distinct cooling of the cell during discharge, from which it appears that the energy required to separate the sulphuric acid from the solution is taken from the cell in the form of heat. Now, if this is the case, the authors should find by experiment the E.M.F. to be somewhat higher than calculated according to their theory. I should like to ask if this is not the case. Mr. Wythe Smith.

Professor Duncan found that the rate of diffusion of the acid into the plugs of discharged plates was less than one-half that of fully charged plates. If, as the authors say, the alteration in the E.M.F. is caused by change of density of the acid in the plugs, then, after breaking the circuit at the end of a discharge, the E.M.F. should rise at only one-half the rate at which it falls after breaking the circuit towards the end of charge. But this is not the case. In the two instances the rate of variation of the E.M.F. is almost identical, and if the discharge be continued until the P.D. is less than 1·8 volts, then the rise of E.M.F. after breaking the circuit is very rapid indeed. From this it appears that there are actions beside those which the authors mention.

Nearly two years ago, we had special plates constructed to enable us to see the effect of very rapidly changing the density of the acid in the pores of the plugs, but we have not yet had the opportunity of finishing the investigation. I mention this to show that we agree with Dr. Gladstone and Mr. Hibbert in thinking that the variations of the strength of the acid play a very important part in the actions of an accumulator, although we anticipate that all the phenomena are not directly accounted for by such variations.

Mr. J. SWINBURNE: With regard to one or two points in Messrs. Gladstone and Hibbert's valuable paper, I will ask pardon for introducing a little personal history. The effect of the liberation of acid at the plates or in the coatings was indicated by me as a cause of variation of electro-motive force as early as Mr. Swinburne.

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Mr. Swinburne. 1883.* The effect of impurities in causing local action, whose discovery Mr. Crompton credits to an eminent German, was also pointed out by me.† As to what Dr. Gladstone calls Sir William Thomson's law, I fear there is some confusion. As pointed out by Helmholtz, the electro-motive force of a cell does not correspond with the heats of formation unless the temperature coefficient is zero. This cannot be too much insisted upon, as heats of formation are used in the most reckless way. I pointed out in 1887 that a secondary cell ought to cool if discharged at a reasonable rate. This has been, I believe, verified by Professor Ayrton and his colleagues.

Mr. Dimier. Mr. PAUL DIMIER: With regard to the high E.M.F. occasionally obtained from secondary batteries of different forms and under varying circumstances, the following results were obtained by me about three years ago while carrying out a long series of experiments with a form of battery, of American origin, I believe, and known as the Woodward accumulator.

A particular feature of this battery is the formation of its plates. These are made up of honeycombed lead castings, produced by mixing soft lead with powdered crystals of common salt and casting the mixture in a mould. When the plates are cold, they are dipped in water and washed until all the salt crystals have been dissolved, leaving a porous mass of lead with a large number of interstices into which the ordinary paste can be forced. The active surface of the plates is by this process somewhat increased, and owing to the cavernous forms of the holes the paste is well retained when once pressed in.

A result which I thought somewhat unusual at the time, was the high charging E.M.F. per cell as compared to the ordinary E.P.S. cells, for whereas in the latter form of battery a maximum is reached at 2·5 volts, with the Woodward cells the effervescent or milky state was reached with an E.M.F. of 2·7 volts per cell at an average density of acid = 1,185, and when charging was stopped the E.M.F. remained steady, at first, at 2·1 to 2·15 volts per cell.

* "Practical Electrical Units." Spon, 1883.

† "Some Experiments on Secondary Cells," Journal, December, 1886.

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Mr Dimier.

In order to avoid any error of reading due to the voltmeter, it was calibrated against a Clark standard cell, and found correct.

Now it appears to me that such a high counter E.M.F. must have been due to the particular formation of the plates, and may be explained according to the theory expressed in Dr. Gladstone's paper as regards the concentration of $H_2 SO_4$ at the interstices of the plates; for inasmuch as in these plates the paste was distributed into the inner substance of the lead, diffusion of acid would be more impeded than in plates of the E.P.S. battery type, and consequently the density of acid collected at the pores of the plates would increase to a greater extent and produce the observed high counter E.M.F., while the density of the intermediate acid remained normal.

With regard to the relation between specific gravity of acid and the energy of charge or discharge expended, I found, on plotting curves of the latter, that these were approximately straight lines for certain ranges of specific gravity at the ordinary working currents, and that from such relations the state of the cells during charge or discharge could be determined for different values of the specific gravity.

For example, in these Woodward cells, the fall of specific gravity for current discharges varying from 5 to 28 amperes was found to be nearly proportional to the energy of discharge—*i.e.*, an average of 1 degree fall for every 4·75 ampere-hours, or for every 9·4 watt-hours, taken out.

A correct estimate of the commercial efficiency of a storage battery can be obtained by comparing the total energy of discharge, over a given working range of specific gravity, with the energy that has to be expended in charging the battery up to the initial value of the specific gravity before the discharge was commenced.

Thus, if the specific gravity of a cell falls from 1,188 to 1,134 while a discharge of 254 ampere-hours, or 503 watt-hours, is taken from it, and it requires a charging energy of 651 watt-hours to raise the specific gravity from 1,134 to 1,188, when the cell is supposed to be ready for another discharge,—then, under these

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Mr. Dimier circumstances, the ratio of 503 to 651, or 77 per cent., represents the commercial efficiency of the cell.

The following are some details of the Woodward 15 M cells referred to above :—

Capacity = 240 ampere-hours.

Discharge current = 1-25 amperes. Mean internal resistance = 1-140th ohm.

Number of plates = 15. Size = $7\frac{1}{4}$ in. × $7\frac{3}{4}$ in. × $\frac{1}{4}$ in. Distance apart = 5-16th in.

Total weight of cell = 98 lbs.

" " " plates = $61\frac{1}{2}$ lbs.

Mr. Hibbert. Mr. W. HIBBERT: In the paper, the section which deals with the relation of thermo-chemical values and the E.M.F.'s that correspond to them is professedly abbreviated. It is intimated that the matter will be postponed for further consideration. But it is shown that where the conditions are sufficiently simple—that is, at the two ends of the curve—the calculated values agree with observation. At the top of the curve, when the plates are in pure H_2SO_4, the calculated value is something like 2·62 volts, whilst the observed value is 2·607—a discrepancy of not more than 1-50th of a volt. Considering the difficulty of getting the plates really permeated with acid of the strength supposed to be there, that was an exceedingly good agreement.

It was because of the statedly imperfect nature of the consideration we gave to this relationship that no reference was made to the cooling during discharge, as shown by Messrs. Duncan and Wiegand, and also by Professor Ayrton and his colleagues. But those results were not overlooked. As suggested by Mr. Smith, cooling would lead to the observed values being higher than the calculated, and this was in accordance with experiment. It ought to be kept in mind, however, that the difference between the thermo-chemical values and observed E.M.F. is never more than about 1-10th of a volt; and even if the cooling property were shown to account for this, it would be insufficient to touch our experimental evidence that the changes in E.M.F. (extending over more than 0·7 of a volt) are determined mainly by changes in strength of acid.

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Mr. Hibbert.

With reference to the remarks read by Mr. Robertson, a good many suggestions and fancies are mentioned in that paper, but, unfortunately, experiment is directly against them. Professor Armstrong and Mr. Robertson hold that hydrogen dioxide, or some form of "active oxygen," has a great influence on E.M.F.

Dr. Gladstone and I tried the effect of adding hydrogen dioxide to the cell, and found no effect worth noting—nothing that could be dwelt upon; the change of E.M.F. on adding a fair amount of hydrogen dioxide was almost negligible. We also tried adding persulphate of potassium—which ought to have given us persulphuric acid—but in no case were these substances able to produce appreciable changes of E.M.F.

On the other hand, we find, as described in the paper, that sulphuric acid produces changes of E.M.F. more than enough to cover the working range generally allowed. Not only is this the case, the form of the curve as the plates go from strong acids to very weak ones* is not unlike the discharge curve observed by Professor Ayrton and his colleagues. You will observe in Fig. 3 that there is a shoulder and an abrupt descent when the pores of the plates are filled with about 6 per cent. acid. That is to say, a small weakening in the strength of the acid by further discharge will at this point produce a disproportionately large fall of E.M.F. This explains why there is a critical point in the discharge curve of an accumulator at about 1·8 volts, and throws light on other changes in P.D. described in the paper by Messrs. Ayrton, Lamb, and Smith.

Mr. Crompton, in speaking of our experiments having been made with small cells, suggested that therefore they might be liable to suspicion. This would be true if our method allowed any current to flow. But we were careful to avoid that. No discharge (except in one experiment) was possible; the paper deals exclusively with E.M.F., and in no case can E.M.F. pure and simple depend on the size of the cell. The results we obtained may therefore be regarded as applying to cells of all sizes.

* No current is taken from the cells during these changes.

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Mr. Hibbert. Mr. Swinburne points out that Lord Kelvin's law is incomplete, and that Helmholtz showed how to amend it by adding a second term. That is true. But Helmholtz's correction depends on the magnitude of the temperature coefficient of a cell, and Preece showed in 1883 that the change of E.M.F. with temperature in the case of an accumulator was very small. Although I cannot remember the figures, the value was so small that the correction may, I believe, be safely neglected.

The PRESIDENT: In Fig. 1, are we to understand that there is no current taken from those cells, but simply that there is a falling off of the E.M.F. by time, the cell remaining insulated?

Mr. HIBBERT: No discharge took place, except in one of our experiments, the observations being made by potentiometer or condenser.

The President. The PRESIDENT: In connection with what Mr. Swinburne was saying, it is exceedingly important to distinguish between the E.M.F. of the cell and potential difference at the terminals. When Mr. Crompton said the E.M.F. in his experiments rose soon after the commencement of discharge, did he not really mean that the potential difference at the terminals rose? [Mr. CROMPTON: Yes.] Because that very likely would be explained, as Mr. Swinburne suggested, by a variation in the resistance of the cell. This is borne out by a reference to the paper which my students and I published two years ago, and from which it will be seen that both in charging and discharging the resistance falls at first, although afterwards it rises very much. Mr. Crompton tells us to-night he has observed a rise in the potential difference soon after the beginning of the discharge. I may mention that in some recent tests I have been making, not yet published, on cells having non-pasted plates, I find the same sort of thing in the early part of the charging. This hump that we have observed in charging, and the similar hump which I understand Mr. Crompton has observed in discharging, arising probably from this fall of resistance in the early part of the charge and discharge, would be more noticeable when you are charging or discharging with large current. The reason, then,

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why in the curves which accompanied our papers of 1890 on the tests made on the E.P.S. cells no such hump was seen, was probably because with the E.P.S. type of cell you are only allowed to discharge with rather small current-density. But in the non-pasted plates I have been speaking of you may discharge with such a large current-density that a very small change of resistance makes a very considerable alteration of shape in the P.D. curve. The President.

Dr. GLADSTONE: I think my first duty is to say how grateful we are to the Society for the way in which it has received this communication both last week and this week. I myself have looked at the subject mainly from the chemical side, and have drawn conclusions as to what must be the electrical results, considering the chemical changes which were taking place. I am very glad to hear that this theory of ours has been already distinctly recognised by several experimenters, within certain limits. Of course everyone knows that the E.M.F. of a battery does vary along with the strength of the sulphuric acid to a certain extent; but I think, from many things I have heard quite recently, that the wide correlation between the one and the other which we have pointed out is not recognised. It was partially known to Mr. Swinburne and Mr. Crompton; but the fact is, a true explanation is never brought forward all at once and accepted immediately, but it finds it way gradually into general acceptance. With regard to the importance of promoting diffusion, I was glad to hear the remarks of Mr. Crompton, and that his attention had been directed to that matter so practically before ever it occurred to us. As to the chemical changes that take place, I do not think there can be much doubt whatever as to the correctness of the formula I put down on the board as expressing the ultimate results. There may be a great deal of difference of opinion as to whether there are any intermediate reactions taking place. Into that matter we have not gone. It is quite possible that what Mr. Robertson put on the board may exist. These are matters of speculation, and it is one of the most difficult things in the world to tell what any intermediate stage in a chemical reaction is. We know where we begin, and we know where we end, but of the Dr. Gladstone.

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Dr. Gladstone.

intermediate stages we generally know nothing. It is especially difficult to work out in this case, because we have the ultimate sulphate of lead mixed up with peroxide of lead and various other substances. It is also not for us to say exactly at present whether it is the absolute sulphuric acid, H_2SO_4, that is acting in these batteries, or whether it is the hydrates of sulphuric acid.

In regard to the thermo-chemistry of the matter, I have nothing to add to the remarks already made. We propose to work it out more fully in the future. As to the presence of the dissolved peroxides, and the difference of opinion and difference of results of experiment between Professor Armstrong and Mr. Robertson and ourselves, I am rather at a disadvantage, because Mr. Robertson knows all about our experiments, and I do not know the experiments which he has performed. At present we only know some of their results; and we cannot repeat their experiments, because we do not know the precise methods adopted. In these kinds of investigations we generally find that there is some truth on both sides. I have no doubt whatever that these peroxides do make some change of the E.M.F., but what we say is, we cannot find any definite evidence of it. The theory of Mr. Robertson may explain certain phenomena, but, in view of our observations, the concordant results arrived at from different directions, and the various arguments leading up to the same conclusion, we must still attach a preponderating amount of importance to the strength of the sulphuric acid against the working surfaces of the two plates. The various remarks made will receive our very best attention,* and I trust in one way or another a good deal more light may be thrown on this very obscure subject.

Upon the motion of the PRESIDENT, a vote of thanks to Dr. Gladstone and Mr. Hibbert for their valuable paper was unanimously passed.

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* This will include what we shall have to say when we see Messrs. Armstrong and Robertson's criticisms in print.

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The PRESIDENT announced that the following candidates had been duly elected:—

Associates:

Walter H. Brownlee.	A. B. Mountain.
T. B. A. Clarke.	George Newman, jun.

Henry Wall Wilkinson, A.M.I.C.E.

Students:

Frederick Bennett Pell.	John Clarence Lyell.

The meeting then adjourned.

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ABSTRACTS.

CARL FROMME—MAGNETIC INVESTIGATIONS.

(*Wiedemann's Annalen, Vol.* 45, *No.* 4, *p.* 798.)

The author believes, from his previous investigations (cf. former abstracts), that several identically similar pieces of iron or steel may, by various treatment, be so magnetised as to have precisely the same moment and position of poles, and yet be very different in their internal magnetic arrangements. In his last paper the author showed that the rapidity with which the magnetising force, H, was reduced had a distinct effect on permanent magnetism.

A slow reduction of H gives the maximum P.M. (say P.M. max.); (A) a sudden reduction gives a less P M. (say $P.M._1$), but a bar magnetised to P.M. max. can also have its P.M. reduced to $P.M._1$—(B) by a properly chosen reverse current, or (C) by a suitable jarring and knocking. Iron and steel bars and a bundle of iron wires were experimented on in both A, B, and C states of equal $P.M._1$ by noting the temporary magnetism and the variation of P.M. when a magnetising force, small compared to that giving P.M. max., was applied to them in different ways and various numbers of times. The B and C state appear to be the same in the case of a bundle of iron wires, and to differ from the A state; in the latter the P.M. being much more increased by a strengthening current than in the two former. Except in the case of the bundle, all three states differ from each other, a strengthening current having a different effect in each, both on permanent and temporary magnetism, as also have a large number of other influences, such as rate of diminution of the magnetising current. Some effects of combining the various conditions are also given. The author's general view is that when the magnetising current is more or less suddenly broken the grouping of the molecular magnets undergoes a thorough change, whereas a reverse magnetising force principally shifts the molecular magnets in each group; jarring has apparently much the same tendency as sudden break of the magnetising current; but the stability of the grouping with respect to jarring is less in bars than in the "bundle." A large number of tables are given showing results obtained by very various actions and combinations.

LUCAS and GARRETT—A DETECTOR FOR SPARK IN HERTZ'S RESONATOR.

(*Phil. Mag., Vol.* 33, *No.* 202, *p.* 299.)

A test tube of small diameter has two platinum wires sealed into the lower part, and is partly filled with hydrochloric acid; a few cells are connected to the wires, and by the electrolysis of the acid a continuous supply of explosive mixture of hydrogen and chlorine is kept up in the tube. In the upper part of the tube are

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two small holes opposite one another, through which two platinum wires pass which are connected to the two parallel resonator wires. A simple adjustment is provided so that the ends of the two upper platinum wires can be brought very close to one another. When the tube is full of the explosive mixture, the very smallest spark will explode it. An explosive mixture of hydrogen and oxygen was first tried, but was not nearly so sensitive. When the points are very close together they frequently get short-circuited by a particle of spray; to get over this, a battery is joined to the two resonator wires (at a node, so as not to interfere with the phenomena), which at once dissipates any such short-circuiting drop.

G. PELLISSIER.—THE WINDMILL AS A SOURCE OF POWER (I.).

(*Lumière Electrique*, *Vol.* 44, *No.* 15, *p.* 15.)

Extracts are given from A. R. Wolf's "The Windmill as a Prime Motor" (New York, 1885), giving the detailed cost per H.P.-hour for various-sized wheels. The totals vary from 8d. for a 0·12-H.P. 10-ft. wheel to 1½d. for a 1·34-H.P. 25-ft. wheel. These are based on the assumption that the wheel works eight hours per day with a 15-miles-an-hour wind. These figures are said to be well within the mark for America, but for France it would be necessary to take much lower ones. The real reason that windmills are not used is the defects of secondary batteries. Professor Blythe has a wind installation at Marykirk; a 26-ft. wheel charging 12 E.P.S. accumulators. One good breezy day stores enough to light 10 8-C.P. lamps three or four hours a night for four nights. With a strong breeze the wheel gives 4 H.P. The Duc de Feltre in 1886 erected an installation at La Hève for supplying current to the lighthouse, but it failed owing to the large number of calm days. Mr. Brush has an installation at Cleveland, U.S.A., with a 55-ft. wheel driving a 12-kilowatt Brush dynamo, and charging 12 sets of cells, 34 in series. It works excellently, but the cost per H.P.-hour is probably much higher than it would be for steam power.

M. LINDECK.—THE E.M.F. OF THE CLARK CELL.

(*Zeits. Instrk.*, *Jan.*, 1892; *Lumière Electrique*, *Vol.* 44, *No.* 14, *p.* 36.)

Various values are given for the E.M.F. of the Clark cell, but the author points out that some are in B.A. volts, some in old Legal, some in new Legal, and others in true volts. When they are all reduced to true volts, they practically agree with that of Lord Rayleigh, viz., 1·435 true volts at 15° C. With regard to the temperature coefficient, various values have been given, some only one-half, or less, of that given by Lord Rayleigh; these differences, however, are in general due to the fact that they have been taken too quickly: a cell does not change its E.M.F. with temperature at once. If the cell is rather large, and the zinc a vertical rod, it is in contact with layers of liquid which differ in concentration when the temperature rises, and the whole liquid does not become even till it has been kept at the altered temperature for some time. In the cells used in the German laboratories, the zinc is placed horizontally, so as to overcome this evil.

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Dr. FEUSSNER—ALLOYS FOR STANDARD RESISTANCES.

(*Elektrotechnische Zeitschrift, Feb.* 19, 1892; *Lumière Electrique, Vol.* 43, *No.* 10, *p.* 485.)

Metals of the German silver class—*i.e.*, alloys of copper, nickel, and zinc—are liable to go rotten in course of time, owing to the last metal crystallising. Dr. Feussner has been experimenting on various alloys of copper and nickel, with very small amounts of manganese and iron (generally well under one per cent.). He gives two curves showing the variation of specific resistance and temperature coefficient with percentage of nickel. The R_s rises in a straight line from 1·6 for pure copper to about 52 for 60 copper with 40 nickel; it then remains practically flat till 63 per cent. Ni is reached, when it falls again in a practically straight line to the R_s of pure nickel at 100 per cent. Ni. The temperature coefficient curve drops sharply from 0·38 for pure copper to 0·05 for 10 per cent. Ni; it then decreases fairly steadily to 40 per cent. Ni, where it crosses the zero line, and becomes about — 0·01 per cent.; is zero again at about 48 per cent. Ni, remaining about + 0·01 till 58 per cent., and then rising sharply again. Curves are also given showing increase of resistance with temperature of various alloys of the "manganin" class—*i.e.*, copper and manganese up to 6 per cent. of manganese: these have a small and practically constant temperature coefficient—*i.e.*, the resistance rises in a straight line with temperature; but for alloys of high percentage of manganese up to 30 per cent., the temperature coefficient varies with the temperature, the resistance increasing from 0° up to about 40° or 50°, and then diminishing again till at 100° it is less than at 0°. This class of alloy is, moreover, liable to change in consequence of oxidation of the manganese, and is also difficult to draw.

K. STRECKER—THE PROTECTION OF TELEPHONE LINES AGAINST INDUCTION.

(*Elektrotechnische Zeitschrift, Vol.* 13, *No.* 10, *p.* 128; *Lumière Electrique, Vol.* 44, *No.* 16, *p.* 122, *No.* 17, *p.* 181.)

The author begins by determining the disturbing effects on a telephone circuit of a neighbouring wire carrying alternating and direct lighting currents and telephone currents in terms of strength of current and rate of change; these he shows to be in the ratio of 0·2 for a telephone current, 110 for a current of 100 amperes from a direct machine running at 600 revolutions and having 40 commutator segments, and 30,000 for an alternate current 70 amperes mean current and 80 ∿. A lead cover to a cable is an almost perfect shield against induction from without, and a buried cable is also practically protected, by the surrounding earth being a conductor. The lead cover in a multiple cable is, however, no protection against induction of the various wires on each other, but each must have its own separate metallic sheath.

With regard to bare overhead wires, there is no method of running (twist wiring, &c.) by which the circuit can perfectly protect itself against induction. Against a single strong current line wire there is no perfect protection; against a double

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one (lead and return) a wire always equi-distant from the two is unaffected. Multiphase wires, if run on the twist, have their effect on neighbouring wires much reduced, but there is no means of entirely eliminating their effect on neighbouring telephone wires. All lighting and power wires should, the author considers, be obliged to be so run as to minimise their effect on telephone and telegraph ones in the neighbourhood.

In the discussion, Herr Uppenborn pointed out that most direct-current systems used accumulators, and were harmless; but even in those which did not, the induction effect was very slight—*e.g.*, the pilot wires of the Berlin cables are used as telephone wires when required. Alternate and multiphase currents are quite harmless in towns, as concentric cables are always used. The real disturbances in telephone lines are due to leakage—conduction, not induction—and can be overcome by using metallic telephone circuits.

J. P. ANNEY—THE EMBRUN WATER-POWER INSTALLATION (I.).

(*Lumière Electrique*, *Vol.* 44, *No.* 17, *p.* 151.)

The power is obtained from a rapid stream named the Crévoux, the station being at about a mile and a half from the town. A dam has been built across the stream, and the water is brought about half a mile—partly by open canal, and partly by tunnel—to a reservoir some 50 feet above the level of the turbines. The latter are two in number, 70 H.P. each, of the Girard type, and have a guaranteed efficiency of 75 per cent. They run at 140 revolutions per minute, and each drives, by belt, a Ferranti dynamo and its exciter at 900 revolutions, giving 100 volts and 250 amperes, which is transformed up to 2,400 volts and 10 amperes. All circuits, both high and low pressure, are brought on to the switch-board, on which also are handles controlling the flow of water to the turbines. The circuits are entirely overhead, and carried on oil insulators. There is a high- and low-tension circuit right round the town, with branches in various directions. The former feeds the latter at 10 points through transformers reducing from 2,400 to 50 volts, the houses, &c., being supplied from the low circuit only. Between the town and the station bare copper is used, but inside the town heavily insulated cable. About 300 private lamps are at present installed, which are charged per lamp-month. There are also 48 street lamps. Power is also supplied to a factory near the town by a separate circuit.

J. TROWBRIDGE—WAVE PROPAGATION OF MAGNETISM.

(*Phil. Mag.*, *Vol.* 33, *No.* 203, *p.* 375.)

The experiments were made with two telephones, to the diaphragms of which were attached small mirrors, by which the image of a small bright spot of light could be thrown on a screen. The axes of the telephones were so turned that the excursions of a spot of light reflected successively from both the mirrors of the telephones represent the figure produced by the combination of two motions at right angles to each other (the method of obtaining figures is similar to that of

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Lissajou with two tuning-forks and mirrors). A circular ring of iron 3 ft. in diameter had four coils wound on it—two of large wire, A, fixed opposite one another, say at the top and bottom, and through which the current from an alternator was passed; and two fine-wire secondaries, B, each connected to one of the telephones, and which could be moved about round the ring and over the large coils if desired. The mirror on one telephone gave a vertical line when vibrated alone, the other a horizontal one. The A coils were joined up in opposition (*i.e.*, so that with a continuous current they would give consequent poles midway between them); a B coil was unaffected at the point midway between the A ones. One B coil was then fixed, not near the middle point, and the other one opposite it; the moving spot of light reflected from the two mirrors giving an ellipse on the screen; on moving the second one over the middle point on its side of the ring, the major axis of the ellipse shifted from right to left, indicating a change of phase of 180°, the lines threading through the coil in opposite directions or opposite sides of the middle point. There was no change of phase if the A coils were not opposed. The whole makes an interesting lecture experiment, but the author was unable to detect any true nodal points indicative of wave motion of magnetism, and he concludes that the propagation of magnetism in iron, &c., is expressed by the equation of molecular magnets in a resisting medium rather than by the equation of a wave motion.

M. ASCOLI—ON THE RELATION IN BATTERIES BETWEEN E.M.F. AND SPECIFIC HEAT.

(*Rend. Lincei*, 1891, *p.* 397; *Lumière Electrique*, *Vol.* 43, *No.* 11, *p.* 545.)

Czapski has shown that when E.M.F. diminishes with rise of temperature the chemical heat is greater than the voltaic heat, and *vice versâ*; the two being equal only when the temperature coefficient is zero. To demonstrate this, M. Ascoli discusses what occurs when two exactly similar cells, A and B, of E.M.F. E_o are put in opposition and A is heated. If the cells are such that the E.M.F. rises with the temperature, there will be a current, C, through the two, direct in A, and reversed in B. If E_a be the E.M.F. of the heated cell, an amount of heat, $E_a \times C$, will be absorbed in it; and, similarly, a quantity of heat, $E_o \times C$, will be produced in B. Simultaneously a certain quantity of heat, $Q_a C$, is produced in A, and a quantity, $Q_o C$, absorbed in B. The total heat absorbed in A is, therefore, $c(E_a - Q_a)$; that produced in B being $c(E_o - Q_o)$. If $E_a - Q_a$ and $E_o - Q_o$ had negative values, heat would be produced in A and be absorbed in B, consequently after a temporary heating of A there would be a continuous transport of heat from the cold cell, B, to the hot cell, A, until A was exhausted; which would be contrary to the second law of thermo-dynamics: *ergo*, if the E.M.F. rises with temperature, E must be greater than Q, and *vice versâ*. The quantity $(E - Q)$ has the same meaning in a battery as the Peltier effect in the theory of the thermopile (neglecting the Thomson effect). The author shows that Helmholtz's equation may be derived from the above.

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L. ARONS—A LECTURE EXPERIMENT WITH HERTZ OSCILLATIONS.

(*Wiedemann's Annalen, Vol. 45, No. 3, p. 553.*)

A glass tube 8 ft. long and 2½ in. diameter is closed at the ends, and at each end two small openings like bottle-necks are blown; these are then closed with corks, and through a fine hole in each of the latter at one end an aluminium wire passes into the tube and out at the cork at the opposite end, so that there are two wires stretched parallel to each other the whole length of the tube. A glass tap is blown into the side of the tube at one end, and connected to a water exhaust pump which will reduce the pressure to from 10 to 20 mm. of mercury. When the wires are stretched the corks are covered over with sealing-wax, and will then keep a vacuum of 1 mm. for a whole day. The glass tube is then attached by parallel wires 12 ft. long to zinc plates 4 in. square close to the exciting plates of a Hertz oscillator; the two wires in the tube are bridged together at the far end from the oscillator by a short piece of wire introduced through the tap. The tube then lights up evenly throughout its length. If a second bridge, B, is applied to the parallel wires between the tube and the resonator, it generally extinguishes the light; but by shifting B along the wires positions can be found for it which are such that it divides the whole length of the wires into two parts in resonance with each other, which is shown by the tube becoming unevenly lighted—dark at the nodes, and bright at the loops. From one up to five bright loops can be obtained in the tube as above arranged, and the positions of the second bridge which cause them are shown to agree very fairly with calculation.

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LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Journals during the Months of APRIL and MAY, 1892.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

G. RICHARD—Electric Welding (S. I.). (Howard, Burton, Angell, Gendron, Siemens & Williamson, Riess).—*Lum. El.*, vol. 44, No. 21, p. 353.

F. Ross—Central Stations.—*E. T. Z.*, vol. 13, No. 19, p. 255.

C. P. STEINMETZ—The Proportion between High- and Low-Pressure Work in the U.S.A.—*E. T. Z.*, vol. 13, No. 21, p. 271.

G. RICHARD—Details of Deptford Ferranti Station (Switch-Board, Switches, &c.) (I.).—*Lum. El.*, vol. 44, No. 18, p. 215.

H. GEORGE—The Comparative Output of the Direct, Alternating, and Drehstrom Motor.—*E. T. Z.*, vol. 13, No. 18, p. 236.

Dr. MEISSNER—The Transmission of Power at Lauffen.—*Lum. El.*, vol. 44, No. 22, p. 435.

ANON.—Transmission of Power at Genoa.—*E. T. Z.*, vol. 13, No. 17, p. 216.

W. LAHMEYER—Transmission of Power (Paper before El. Tech. Ver., Berlin, and Discussion—*E. T. Z.*, vol. 13, No. 14, p. 183.

Dr. MEISSNER—The Lauffen-Heilbronn Transmission of Power Works.—*E. T. Z.*, vol. 13, No. 15, p. 193.

H. MÜLLER—Cut-Out (Break at No Current).—*E. T. Z.*, vol. 13, No. 22, p. 288.

SIEMENS and HALSKE—Coulomb Meter.—*E. T. Z.*, vol. 13, No. 22, p. 289.

H. ARON—Drehstrom Meter.—*E. T. Z.*, vol. 13, No. 15, p. 193.

F. GERALDY—The Commercial Use of Condensers.—*Lum. El.*, vol. 44, No. 19, p. 251.

J. TRUMPY—Arrangement of Cells for Central Station Work.—*E. T. Z.*, vol. 13, No. 20, p. 262; *Lum. El.*, vol. 44, No. 22, p. 425.

F. DESQUIENS—Swing Bridge with Electric Control.—*Jour. Tel.*, vol. 18, No. 2, p. 176.

C. HEIM—Remarks on Mr. Preece's Paper on Conductors.—*E. T. Z.*, vol. 13, No. 15, p. 194.

MAGNETISM.

C. STEINMETZ—Remarks on the Magnetic Circuit.—*E. T. Z.*, vol. 13, No. 16, p. 203.

— MAJES—Magnetometer.—*Lum. El.*, vol. 44, No. 19, p. 282.

— MARCHER—Exploring Needle for Magnetic Field.—*Lum. El.*, vol. 44, No. 18, p. 236.

W. KUNZ—The Effect of Temperature on Hysteresis.—*E. T. Z.*, vol. 13, No. 19, p. 245; *Lum. El.*, vol. 44, No. 22, p. 442.

GEROSA and FINZI—The Magnetisation of Iron in an Alternating Field.—*Rend. Lincei*, 7 (8), p. 253, 1891; *Beibl.*, vol. 16, No. 4, p. 229.

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TELEGRAPHS AND TELEPHONES.

W. Lahmeyer and C. Grawinkel—Further Discussion on the latter's Paper on Telephone Disturbances.—*E. T. Z.*, vol. 13, No. 18, p. 283.

Dolivo Dobrowolski—Overcoming Induction on Telephone Lines (Paper and Discussion, El. Tech. Ver.)—*E. T. Z.*, vol. 13, No. 16, p. 211.

W. Christiani—On the Theory of Inductionless Overhead Wiring without Twist.—*E. T. Z.*, vol. 13, No. 22, p. 283.

Dr. V. Wietlisbach—Influence of Lighting, &c., Wires on Telegraph and Telephone Lines.—*E. T. Z.*, vol. 13, No. 22, p. 285.

Siemens and Halske—Telephone (I.).—*Lum. El.*, vol. 44 No. 21, p. 371.

R. Bauer—The Telephone for Railway Work.—*E. T. Z.*, vol. 13, No. 30, p. 264.

E. Kolben—American Paper Insulated Telephone Cables.—*E. T. Z.*, vol. 13, No. 14, p. 175.

E. Dismoz—The Adjustment of the Break in the "Hughes" Instrument.—*Jour. Tel.*, vol. 16, No. 5, p. 133.

E. Müller—Improvement in "Hughes" Working on Underground Lines.—*E. T. Z.*, vol. 13, No. 21, p. 277.

— Jenkin—Rapid Perforator.—*Jour. Tel.*, vol. 18, No. 2, p. 131.

— Wright—Printing Telegraph (I.).—*Lum. El.*, vol. 44, No. 18, p. 223.

Strecker and Billig—Measurement of Inductance of Telephonic Apparatus.—*E. T. Z.*, vol. 13, No. 21, p. 278.

E. Brylinski—On the Capacity of Telegraph Lines.—*Jour. Tel.*, vol. 18, No. 2, p. 97.

— Lagarde—Note on the Capacity of Telegraph Insulators.—*Jour. Tel.*, vol. 18, No. 2, p. 123.

ELECTRIC CHEMISTRY AND METALLURGY.

A. Chassy—On the Laws of Electrolysis.—*C. R.*, vol. 114, No. 18, p. 998; *Lum. El.*, vol. 44, No. 22, p. 443.

G. Kummel—On the Deposition of Precipitates at the Junction of Two Electrolytes.—*Wied. Ann.*, vol. 46, No. 5, p. 105.

C. L. Speyers—The E.M.F. of Metallic Salts.—*American Chem. Jour.*—*Beibl.*, vol. 16, No. 4, p. 220.

A. d'Arsonval—Physiological Effects of Alternating Currents.—*Bull. Soc. Int.*, vol. 9, No. 87, p. 146.

C. Faure—Cost Price of Aluminium.—*Lum. El.*, vol. 44, No. 19, p. 274.

INSTRUMENTS AND MEASUREMENTS.

G. Grassi—Compensated Galvanometer of Constant Sensitiveness.—*N. Cim.* (3) 30, p. 120, 1891; *Beibl.*, vol 16, No. 4, p. 92.

Anon.—Automatic Electrical Fire-Alarm without Battery.—*E. T. Z.*, vol. 13, No. 19, p. 250.

C. Boccali—Comparison of Vacua of various Glow Lamps.—*E. T. Z.*, vol. 13, No. 19, p. 249.

M. v. Frey—The Resistance of the Human Body.—*Beibl.*, vol. 16, No. 4, p. 217.

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STATIC ELECTRICITY.

WURTZ WESTINGHOUSE—Lightning Arresters (I.).—*Lum. El.*, vol. 44, No. 19, pp. 266–281.

L. WEBER—Atmospheric Electricity (5th Report).—*E. T. Z.*, vol. 13, No. 19, p. 239.

C. P. STEINMETZ—Dielectric Hysteresis.—*E. T. Z.*, vol. 13, No. 18, p. 227.

THEORY.

WIEDEMANN and EBERT—The Apparent Repulsion of Parallel "Cathode Radiation" Beams.—*Wied. Ann.*, vol. 46, No. 5, p. 158.

— HERTZ—Researches (French Abstract).—*Lum. El.*, vol. 44, No. 19, p. 285, No. 20, p. 335, No. 21, p. 387.

ELIHU THOMSON—High Frequency and Tension Apparatus (I.).—*Elect. World*, April 2nd, 1892; *Lum. El.*, vol. 44, No. 18, p. 236.

G. SCHMETZ—The Production of Light by Direct Transformation of Electrical into Luminous Vibrations.—*E. T. Z.*, vol. 13, No. 19, p. 245, No. 20, p. 259.

E. COHN—On the Gordon-Winkelmann Method of Determining S.I.C.—*Wied. Ann.*, vol. 46, No. 5, p. 135.

Prof. PERRY—Choking Coils.—*Phil. Mag.*, vol. 33, No 204, p. 433.

Dr. J. KOLLERT—Contributions to the Theory of the Drehstrom.—*E. T. Z.*, vol. 13, No. 14, p. 171, No. 15, p. 191.

DOLIVO DOBROWOLSKI—Efficiency of Transformers (Paper and Discussion, El. Tech. Ver.).—*E. T. Z.*, vol. 13, No. 17, p. 222.

S. KALISCHER—Theory and Calculation of Branch Circuits.—*Wied. Ann.*, vol. 46, No. 5, p. 118.

RAILWAY WORK.

H. SESEMANN—Alarm for Level Crossings.—*E. T. Z.*, vol. 13, No. 17, p. 217.

G. RICHARD—Tramway and Railway Plant (S. I.). (Short, Blackwell, Phillips, Billesby, Dickenson, Love, Schuckert, Hutchinson, Thomson-Houston.)—*Lum. El.*, vol. 44, No. 20, p. 310.

— WESTINGHOUSE—Tramway Plant (I.).—*Lum. El.*, vol. 44, No. 21, p. 359.

E. DE BAILLEHACHE—Train Indicator.—*Lum. El.*, vol. 44, No. 21, p. 353.

VARIOUS.

ANON.—Oil Insulation. Claim of Invention by M. Jean, 1858.—*Jour. Tel.*, vol. 18, No. 2, p. 149.

Lieut. BUCHHOLTZ—Wind Power.—*E. T. Z.*, vol. 13, No. 21, p. 281.

A. D'ARSONVAL—The Generation of Electricity by Living Beings.—*Lum. El.*, vol. 44, No. 18, p. 207.

— SCHOMBURG—Oil Insulator (I.).—*Lum. El.*, vol. 44, No. 19, p. 271.

G. PELLISSIER.—The Tides and Waves as Sources of Power. (Historical *Resumé.*) (I.)—*Lum. El.*, vol. 44, No. 20, p. 301.

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JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXI. 1892. No. 100.

The Two Hundred and Fortieth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 26th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General held on May 19th were read and approved.

The PRESIDENT: Before proceeding to the business of the meeting, I have the melancholy duty to perform of reporting what you probably all know but too well—the lamentable death of our member Mr. P. W. Willans. Unnecessary, indeed, is it for me to add a word to make you appreciate the man who is gone. Mr. Willans was no less marked by his scientific attainment than by the distinction he had won as an engineer. His death is not only a loss to our Institution, but to the electrical engineering profession generally, and that, at the present time, must be regarded as a loss to our country. Nobody knew Mr. Willans better than Mr. Crompton, and I will therefore ask him to say a few words about our friend, whom we can so ill spare.

Mr. R. E. CROMPTON: I generally rise with pleasure to speak in this room, but this time it is with a very different feeling, as I have to-day returned from the funeral of our friend. I say nothing about my own feelings, but they can be understood when

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I say that I have lost in Mr. Willans a personal friend second to none. I had been very closely connected with Mr. Willans for many years, and throughout a long period of close personal and business relationship never had the shadow of a quarrel. When I consider the loss which our profession—in fact, I may say Englishmen as a whole—have sustained by his death, I believe that my opinions will be shared by everyone in this room when I say that in him we have probably lost the greatest steam engineer of the time. There are many present who, like myself, have profited from Mr. Willans's advice and assistance. He was one of those modest, unassuming men who could stand at your side and suggest ideas which you found to be of incalculable value, and he did this in such a manner that one felt almost as if the idea had come from oneself instead of from him. I am afraid, now that he has gone, many of us will find how difficult it will be to get along without him. Willans was in the prime of life, and was just beginning to make his opinions felt. I believe that another few years would have made the whole world acknowledge that by his inventions and investigations in thermo-dynamics he had revolutionised steam engineering. It is sad that such a career should have been cut short in this melancholy manner. I think that I can now add little further than that I believe nowhere in the world will a more hearty note of sympathy arise than from the members of the electrical profession, whose labours he has so greatly aided.

The names of new candidates for election into the Institution were announced, and, this being the last meeting before the recess, it was agreed that the candidates should be balloted for that evening.

Messrs. J. Fairfax and E. Scott were appointed scrutineers of the ballot.

Donations to the Library were announced as having been received since the last meeting from Messrs. Cassell & Co.; Messrs. E. & F. N. Spon; Prof. Robinson, Member; and Mr. A. P.

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Trotter, Member; to whom the thanks of the meeting were duly accorded.

The PRESIDENT: I have to announce that the Council have made their first award of the Salomons Scholarship, value £35, to Mr. C. H. C. Woodhouse, a matriculated second-year student in the Department of Electrical Engineering at the Central Institution.

The following paper was then read:—

ELECTRICAL DISTRIBUTION BY THE NEWCASTLE-ON-TYNE ELECTRIC SUPPLY CO., LIMITED.

By A. W. HEAVISIDE and R. C. JACKSON, Members.

PRELIMINARY.

The town of Newcastle is supplied with electrical energy by two companies, operating different districts, and distinct from one another. These are the Newcastle and District Company, using Parson's turbines, supplying the southern and western portions of the town, and the Newcastle Electric Supply Co. (the company with which we are connected), supplying the northern and eastern portion.

This paper deals with the distribution of electrical energy by the Newcastle-on-Tyne Electric Supply Co., Pandon Dene, it being thought that the members of the Institution would find interest in an account of the methods of working, and of the technical and financial results obtained during the first financial year of the company's operations.

The system employed is a high-pressure one of 2,000 volts, alternating current, with separate transformers in the consumers premises, the price charged being 4½d. per Board of Trade unit.

The company was formed on the 7th January, 1889. Some questions having been raised upon legal points by the local authorities, the whole of 1889 was employed in negotiating the license. These negotiations were more prolonged than usual, and it was not until the close of November that a provisional arrangement was entered into, enabling the company to proceed with the street works in anticipation of the Board of Trade license, which was not received until March 21st, 1890.

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Site.

Eligible sites for the establishment of a central station were found to be limited in number; however, at Pandon Dene, some vacant land was eventually selected, which appeared to have some advantages in its favour, amongst others that of being in a very central position. The business quarter is to the south and the residential quarter to the north, thus facilitating the whole of the company's operations being transacted from one point. The site is also alongside the sidings of the Blyth and Tyne branch of the North Eastern Railway, which taps the Northumberland steam coal field. There is ample room for extension, as the company's land occupies a space about 127 feet wide and 436 feet long, parallel with the railway. The absence of dwelling-houses in the immediate vicinity of the works avoids difficulties which might arise from the vibration caused by machinery in motion. Against these advantages must be set the want of cheap water for condensing purposes. The "supply company's" water is taken from the intermediate service of the local water company, the average cost per 1,000 gallons being 9·78 pence.

Buildings.

The dominant idea in laying out the station buildings was to so arrange them as to reduce any reconstruction to a minimum; the erection of each successive section being carried out without interruption or disturbance to the plant already fixed. At the present time, two sections have been built, enclosing a space 119 feet by 74 feet by 13½ feet high to the top of the girders. This space is covered by means of a light iron lantern roof, supported upon wrought-iron girders and cast-iron pillars, resting on concrete footings. The openings between the pillars are filled up with common brick, lined on the inside of the engine and dynamo room, to about 7 feet 6 inches high, with varnished pitch pine. A brickwork partition separates the boiler house, which is 74 feet long by 55 feet wide, from the engine and dynamo room, 74 feet by 63 feet 6 inches. Two chimney-stacks have been built, one at either end of the main boiler flue, which runs along

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the boiler house side of the partition wall. One chimney has a sectional area at the top of 30 square feet, and is 85 feet high; the other chimney measures 23·5 square feet at the top, and is 110 feet high. Great care had to be taken with the foundations, as the natural bed is no less than 75 feet below the present ground level, the intervening soil being made ground.

Contents of Buildings.

The station at present contains plant capable of a total output of 600 kilowatts, in the following units:—

1 250-kilowatt alternator;
3 100-kilowatt alternators;
1 50-kilowatt alternator;

each machine being rope-driven by means of a separate engine. The year's operations dealt with in this paper were, however, carried out by

3 100-kilowatt alternators, and
1 37½-kilowatt alternator.

The plan and elevation (Figs. 1 and 2, Plate 1) show the general arrangement of the machinery in the station, which consists of—

Three Double-flued Lancashire Boilers, manufactured by Messrs. Robey & Co., Lincoln.

General Particulars.

Working pressure, 120 lbs. per square inch; all plates of the best Siemens-Martin mild steel; diameter of shell, 7 feet; length of boiler, 28 feet; boiler shell plates, $\frac{9}{16}$ inch thick; fine plates, $\frac{5}{8}$ inch thick; end plates, $\frac{3}{4}$ inch thick; number of flues, two; diameter of flues, 2 feet 9 inches; 12 Galloway tubes; heating surface, 750 square feet; grate area, 38 square feet. Two of these boilers have been fitted with Messrs. Bennis's mechanical stokers, and one of them with Messrs. Jas. Proctor's stoker. The figures of a test with these stokers will be given later in the paper. N.B.—Two additional boilers have been fixed for the extensions now proceeding, fitted with Proctor's stokers.

Engines and Dynamos.

1 Vertical Compound Engine by Robey & Co.

General Particulars.

Cylinders, 11 inches and 18 inches diameter, by 12 inches stroke; speed, 200 revolutions per minute; fly-wheel, 6 feet diameter, grooved for eight 1 inch ropes; power, economical load, 75 indicated H.P., with 110 lbs. steam pressure at the engine stop-valve. The engine is fitted with automatic expansion gear, and

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with Meyer's adjustable cut-off on the L.P. cylinder. This engine drives a 37½-kilowatt Mordey-Victoria alternator with an E.M.F. of 2,000 volts effective at a speed of 650 revolutions per minute.

3 Compound Horizontal Fixed Engines by Messrs. Robey & Co.

General Particulars.

Cylinders, 16 inches and 26½ inches diameter, by 36 inches stroke, fitted with the Rowland-Richardson trip valve gear; speed, 73 revolutions per minute; fly-wheel, 15 feet diameter, grooved for 12 1¼ inches ropes; weight of fly-wheel, 5 tons. N.B.—The fly-wheels are being weighted with an additional 5 tons each. Power, economical load, 175 I.H.P.; maximum working load, 240 I.H.P., with 110 lbs. pressure of steam on the engine side of the stop-valve.

These engines each drive a 100-kilowatt Mordey-Victoria alternator at a speed of 430 revolutions per minute.

Each alternator (Figs. 3 and 4, Plate 2) is excited independently by means of an exciter connected direct to the alternator shafts. Another engine of the same class has lately been erected for the purpose of driving a 250-kilowatt Mordey-Victoria alternator.

General Particulars.

Cylinders, 19½ inches and 33 inches diameter, by 40 inches stroke; speed, 70 revolutions per minute; fly-wheel, 17 feet diameter, grooved for 18 1¼ inch ropes; weight of fly-wheel, 15 tons. Power, economical load, 400 I.H.P.; maximum load, 500 I.H.P.; with 110 lbs. of steam on the engine side of the stop-valve.

Water-Feed Apparatus.

One Glo'ster Duplex Pump, to throw 800 gallons per hour. Two Holden & Brooke's Exhaust Injectors, fitted with auxiliary live-steam nozzles, each delivering 600 gallons per hour at about 170° Fah. One additional injector, as above, for the plant extensions now going on.

Switch-Board.

Several forms of high-tension switches have been tried, but none have been found quite satisfactory. A permanent switch-board is now being designed to suit the company's requirements.

Use of Plant.

For the light day and night load, the high-speed vertical engine is employed, the other machinery being brought into use as the service requires. The circuits, four in number, are coupled together or separated, as the load varies.

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Distribution.

The electrical energy is distributed by means of four main circuits of concentric cable, insulated with vulcanised rubber, drawn into cast-iron pipes, of which nearly 10 miles have now been laid. The streets served by the mains are shown on the town map.

The point of furthest supply from the generating station is just under 2½ miles.

The system adopted is that of one pipe, one cable, and means are provided at the street boxes by which the service may be treated either as a radial one or as a network, without disturbance to the insulation.

Cables.

The inner and outer conductors of the concentric cables are separated at each junction or lead-in, and are terminated by means of metallic washers, which are screwed down and clamped together on porcelain insulators. The system of mains is thus quite flexible, and changes in connections are made as easily and expeditiously as at a Post Office test box.

Street Boxes.

The street boxes are built of brick, set in cement, and protected from surface leakage by double cast-iron covers.

Transformers.

The transformers employed are, in general, those of the Elwell-Parker design, varying in capacity from 1 to 10 E.H.P., and are usually placed in cellars, upon the premises of the individual consumers. The Mordey, Kapp, Swinburne, and Tyne transformers are being experimented with.

Meters.

The "Shallenberger" is in general use, with some Aron, Ferranti, Thomson-Houston, and Frager meters under trial.

Working Results.

As already stated, the company did not commence the supply of electrical energy until 1890, and then only experi-

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mentally. It was simply a year of construction and organisation. Nevertheless, the progress diagram (Fig. 5, Plate 3) which accompanies this paper cannot fail, it is thought, to be of interest, as showing the comparative rapidity with which the service developed within the area of the city covered by the operations of this company, so soon as the preliminary delays were overcome. Commencing from the 1st January, 1891, it will be seen that by the end of the year the lamps installed equalled 431,000 watts = 14,400 30-watt lamps, rising from 253,000 at the beginning of the year, or a mean of 342,000 watts. During the year 244,470 units were metered at the consumers' premises. The units metered at the station amounted to 327,821, showing a loss from all causes in distribution of 25·4 per cent. Dividing the total watt-hours sold during the year by the mean capacity in watts installed, we get a result of $\frac{244,470 \times 1,000}{342,000} = 714$ as the average lighting hours per annum. That is to say, our annual output was equivalent to running all the lamps installed for 714 hours—practically 2 hours per day per lamp—the net revenue per lamp working out at 7s. 6d.

Character and Class of the Installations.

With the exception of the Post Office and the Public Library, the installations are small rather than large, there being no hotels and few restaurants, but mainly shops, offices, and private houses; the net result being a low load factor of 8·16 per cent. By the term "load factor" we mean the ratio of the units sold to the units that could be sold if all the lamps were always on. Thus,

$$\frac{\text{Metered watt-hours} \times 100}{\text{Mean watts installed} \times \text{number of hours in the year}}$$

$$= \frac{244,470 \times 1,000 \times 100}{342,000 \times 8,760} = 8{\cdot}16\,\%.$$

Price.

The price charged by the company has been described as the "phenomenally low one of 4½d. per B.T.U." (less 5 per cent. dis-

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count). With gas costing only 1s. 10d. (less 10 per cent. discount) per 1,000 cubic feet, it is quite evident that whatever advantages the electric light possesses over gas, it would be difficult to obtain a higher price than 4½d. Believing that at this price a profit could be earned, we recommended the company to charge this low figure, and we may say that our anticipations have been realised. The problem was, therefore, how to produce the unit at a low cost, and to obtain custom in sufficient amount to earn a fair dividend upon capital invested.

N.B.—It may be objected that the period of work upon which this statement is made is too limited to inspire confidence; but we venture to think that, once having overtaken our establishment charges, the commercial prospect will improve rather than the reverse.

Cost of Production: Coal.

The company uses small steam coal, supplied and delivered by the Gosforth and West Moor Coal Company at the average price, over the year, of 5s. 10d. per ton.

Since 2,300 tons of coal were used, the quantity of coal burnt per unit sold amounted to $\frac{2,300 \times 2,240}{244,470} = 21{\cdot}074$ lbs., at a cost of 0·663 of a penny.

It is to be noted that this coal bill includes all consumption for trial runs of new plant, various testing work, banking and making up of fires, steam heating to offices, and for all other purposes.

We consider that it would be interesting and of value if a relation could be established between the result as given above and the results—

1. If a standard Welsh coal had been used;
2. With water-tube boilers, burning small steam coal, with mechanical stokers;
3. With water-tube boilers, burning Welsh coal, both 2 and 3 being worked under the same conditions with respect to the load factor as in our own case.

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For the purpose of facilitating such a comparison, we give analyses of Welsh coal and of that used by us—

	Welsh Coal.	Gosforth and West Moor.
Carbon	$14{\cdot}500 \times {\cdot}84$	$14{\cdot}500 \times {\cdot}649$
Hydrogen	$62{\cdot}032 \times ({\cdot}046 - \frac{\text{O}}{8})$	$62{\cdot}032 \times ({\cdot}053 - \frac{\text{O}}{8})$
Sulphur	$4{\cdot}032 \times {\cdot}015$	$4{\cdot}032 \times {\cdot}0124$
Ash, per cent.	4·9	13·44
Heat units	14,833	11,300
Theoretical evaporative efficiency in pounds of water from, and at 212° Fah., deducting ash in both cases	15·45	11·75
Or including ash ...	14·69	10·17

as the theoretical value per pound of coal, showing a difference in favour of Welsh coal of 31 per cent. It may here be remarked that the difference in price between the best Newcastle steam coal, and that used by us, for the period under review, was one-half.

It is quite evident from the foregoing analyses that, in order to compare our results as to weight of coal per unit sold with London figures, a fair-sized divisor would be required. Our want of sufficient data does not enable us, unfortunately, to state what that divisor ought to be. We are in hopes of being enlightened on that point during the discussion on the paper.

Water Account.

All the water consumed at the station for all purposes was measured by a Kennedy water meter, which registered 2,922,000 gallons for the year. To arrive at the amount of the water evaporated, a deduction of 10 per cent. is made to allow for such items as washing out and filling up of boilers, cleaning down lavatories, buildings, &c. The allowance being made, we

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have, as the total water evaporated to steam, 2,629,800 gallons, or a water consumption per unit sold of $\frac{2,629,800 \times 10}{244,470} =$ 107·5 pounds, at a cost of 0·1053 of a penny per unit generated, or at the station = 107·5 × 0·75 = 80·63 pounds.

The average evaporation of the boilers thus becomes—

$$\frac{\text{Pounds of water per unit sold}}{\text{Pounds of coal per unit sold}} = \frac{107·5}{21·074} = 5·1 \text{ lbs.}$$

We have obtained an evaporation of from 7 to 8 pounds of water per pound of coal with the boilers working under the best conditions; the difference, 7·5 − 5·1 = 2·4 lbs., is accounted for principally by banking of fires at times of low load and heating up of standing engines.

Oil.

Oil averaged 0·0014 gallons, at a cost of 0·044 of a penny per unit sold.

Waste, Sweat Cloths, and Sundry Stores.

These work out at 0·055 of a penny per unit sold.

Repairs.

Repairs to buildings and plant at the station amount to 0·2480 of a penny per unit sold. This sum includes everything necessary to maintain the machinery at its full efficiency, and also to carry out some small alterations that experience suggested for the improvement of the plant.

Labour and Supervision.

It is found that at the period of maximum load the staff necessary to run three 200-H.P. engines and three 100-unit alternators consists of 1 station assistant, 1 engine-driver, and 1 stoker.

Three sets of men are employed, arranged in shifts of eight hours each, with one hour overlap for cleaning, oiling, &c. The cost of the service, inclusive of proportions of the manager's and secretary's office charges, works out at 1·9462 pence per unit sold.

The cost of all service external to the station over nearly 10 miles of mains, leads-in, transformers, meters, &c., is 0·2817 of

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penny. To these figures have to be added the general charges, which amount to 0·6807 of a penny.

Summarising the cost, we have—

	d.	d.
Coal	0·663	
Oil	0·0440	
Petties	0·0579	
Water	0·117	: 0·8819
Repairs	0·2480	
Labour and supervision ...	0·8163	: 1·0643
		1·9462
External	0·2817	
General (rent, rates, taxes, law, sundry expenses) ...	0·6807	: 0·9624
		2·9086 per unit sold.

Capital.

When the capital account is closed for the works in progress, it is estimated that for each kilowatt installed an expenditure of £50 will have been incurred. This result will in all probability be reached by the end of the present year, when, as shown by the progress diagram, the plant erected will be capable of working at 600 kilowatts, or of dealing with 800 kilowatts wired, leaving one-fifth reserve plant.

Plant Efficiency.

We shall now consider some of the points in connection with the plant efficiency, it being always borne in mind that the results actually obtained are over a period of 12 months, with a mean load factor of 8·16 per cent.

Boilers.

With a view to the economising of labour, and to abate the smoke nuisance as far as possible, the boilers were fitted with mechanical stokers, as already stated. The following is a table

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showing the result of a test between the two classes of stokers now in use:—

Stoker Tests, July 5th and 18th, 1891.

Description of Firing.	Time in Hours.	Gallons of Water Evaporated.	Coal Consumed in Pounds.	Pounds of Water per Pound of Coal.	Per Pound of Combustible. From, and at 212° Fah.
Bennis stoker and bars	3¼	1,750	2,492	7·02	8·29
Proctor „ „	3¼	1,920	2,352	8·16	9·64
	—	9·7 %	6 %	16·2 %	In favour of Proctor's stoker.

We have found the boilers supplied to us to be each capable of generating steam for 240 I.H.P. at the engine cylinders without undue forcing.

The boilers are generally fed through exhaust steam injectors, assisted by auxiliary live-steam jets, the average temperature of the feed water being about 170° Fah.

To bank one of these boilers, keeping steam at from 80 lbs. to 90 lbs. pressure, requires 35 lbs. of (Gosforth) coal per hour, costing, say, one penny.

Water.

The water used is of a medium quality, having 15° of hardness.

Engines.

The engines are fitted with steam jackets in both the high-pressure and low-pressure cylinder. The steam, on its way to the low-pressure cylinder, passes through an intermediate receiver, where its temperature is raised, and probably some of the contained water re-evaporated to steam, by means of a coil of piping, through which a passage of live steam is maintained.

Fig. 6 (Plate 4), and Figs. 7 and 8 (Plate 2), are diagrams of work done by the engines at varying loads.

We find as the average of a large number of indicator diagrams that 202 I.H.P. are required to drive a 100-kilowatt

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Mordey-Victoria alternator at full load; the losses from friction and other causes being 68 I.H.P. which we analyse as follows:—

Engine friction	27·5
Back pressure on L.P. cylinder	11·0
Ropes	17·4
Dynamos	7·75
Excitation	4.6 = 68

Mr. Crompton, in his paper read before the Institution of Civil Engineers, gave a figure of 84 per cent. to 85 per cent. as an efficiency which had been obtained in practice with continuous-current steam dynamos at full load.

The above analysis enables us to see how far, and where, we depart from the efficiency mentioned by Mr. Crompton.

If we add the rope losses to the E.H.P. and divide by the total I.H.P., we obtain the following:—

$\frac{134 + 17·4}{202}$	= 75 %
To this percentage we add the extra efficiency of the continuous current dynomo (as given)...	= 4 %
Leaving a difference of	= 5 %
To be accounted for to come up to Mr. Crompton's figure of...	84 %

Dynamos.

The directors have been much pleased with the satisfactory reports which have come in from the consumers regarding the steadiness of the light, which is in part due to the good governing of the engines, and to the incidental advantage that the Mordey-Victoria alternators possess in the momentum acquired by the revolving field magnet.

Improvements are being introduced as suggested by experience, and the 250-unit alternator now in course of erection is fitted with an outer bearing. The mechanical construction of the armatures also improves with each succeeding make. The oiling arrangements, by means of small pumps driven from the alternator shafts, have proved very effective.

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We find the efficiency of the 100-kilowatt Mordey-Victoria alternator to work out at—

$$\frac{\text{E.H.P.}}{\text{B.H.P.}} = \frac{134}{146{\cdot}35} = 91{\cdot}5\ \%;$$

which result, we believe, agrees within ½ % with that given by the Brush Company.

Station Output and Efficiency.

The E.M.F. of the dynamos is measured by Cardew voltmeters, and the current by means of Evershed ammeters, the readings being taken every half-hour and posted in the engine room day-book. The correctness of the various ammeters is checked from time to time by comparison with a Siemens dynamometer, which is, again, checked in the laboratory of the local College of Science. The principal data are as follows:—

Units metered at the consumers'	244,470
Station units	327,821
Loss in distribution	25·4 %
Total running hours at the station ...	7,504
I.H.P. hours	939,500
Average I.H.P.	$\frac{939{,}500}{7{,}504} = 125{\cdot}2$
Units per running hour (at the station)	$\frac{327{,}821}{7{,}504} = 43{\cdot}686$
Average watts per I.H.P.	$\frac{43{,}686}{125{\cdot}2} = 348$
Weight of water per hour per I.H.P., inclusive of radiation and condensation losses	$\frac{2{,}629{,}800 \times 10}{939{,}500} = 27{\cdot}91$ lbs.
Average commercial efficiency of engines and dynamos	$\frac{\text{Total E.H.P. for the year}}{\text{Total I.H.P. for the year}} \times 100 = 46{\cdot}6\ \%$

Parallel Working.

Trials have been made to work in parallel, with only a partial success. The trials called attention to the varying angular velocity of the engine fly-wheels, due to the early and

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quick cut-off of the trip valve gear. We hope to obtain successful results by increasing the weight of the fly-wheels, which work is now in progress; also by increasing the elasticity of the drive by the use of spring pulleys arranged on the principle of the transmission dynamometer, in place of the ordinary pulley. As a uniform turning effort is the end to be aimed at, we believe that the steps we are taking will accomplish that result.

Distribution.

The installations on the consumers' premises, as shown in the progress diagram, amounted to 431,000 watts on December 31st, 1891. The nominal transformer capacity supplying the above was 391,000 watts, or an excess of installed watt capacity to transformer provision of 9 per cent. The percentage of excess has been reduced by the recent extension of small shop-lighting, &c.; it had previously been run up to as high as 25 per cent.

The object we had in doing this was to keep the average transformer efficiency as high as possible, and at the same time to avoid undue fall of pressure in the service. That this course may be safely carried out, if we discriminate between those consumers who burn all their lamps simultaneously and those who never by any chance do so, is supported by an examination of the progress diagram. It will be observed that the average proportion of lamps lighted at any one time, to lamps installed, is about 45 per cent. Taking the extreme case on the sheet, we have a proportion of 28 per cent. excess. We find the average lighting hours per annum of the various classes of consumers on the company's mains to be as follows:—

Restaurants.	Shops.	Offices.	Private Houses.
1,563	459	499	344

As previously stated, the distribution losses from all causes average 25·4 per cent. over the year. The loss is incurred from two sources—transformer and line losses. Weekly observations are taken, by means of Lord Kelvin's milliampere meter and a low-reading Siemens dynamometer, for the purpose of checking and comparing these losses.

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Summary.

Summarised, the chief results embodied in this paper are—

1. Load factor (as per definition, page)	8·16 %
2. Loss in distribution	25·4 %
3. Average lighting hours per annum	714
4. Coal bill per unit sold	21·074 lbs.
5. Water = 107·5 + 10% for water deducted for efficiency results	118·2
6. Pounds of water evaporated per pound of coal, banking, &c., inclusive	5·1 lbs.
7. Water consumption per I.H.P.-hour	27·91 lbs.
8. Engine and dynamo average efficiency	46·6 %

General Conclusions.

Much discussion has taken place on the merits of the various methods of supply. We have had to consider the matter from a purely commercial point of view, having regard only to giving an efficient service, at the least cost, and quite without prejudice in favour of any particular system or mode of working.

That the results have been obtained by the use of separate transformers in the consumers' premises, without a secondary network, by machinery constantly running, with no storage except the coal heap, gives some ground for believing that in alternating-current distribution there exists a means of supplying the public with electric energy, and of meeting Parliamentary and other obligations, at a price which is satisfactory to the public, and affords investors a reasonable interest on their capital.

It may be stated to those interested in sub-transformer station distribution that the average losses in direct distribution *over this year* have been reduced from 25·4 per cent. to 20·5 per cent., and as the load increases it is hoped that further reductions will take place. Also, the cost of the unit sold equals 2·51 pence, which includes all charges.

Mr. R. E. Crompton: The authors' paper is a welcome addition to our stock of data, as it gives us for the first time a fair idea of the actual cost of producing and distributing electrical energy by the Mr. Crompton.

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Mr. Crompton.

best and most modern forms of alternating plant. The first point that occurs to me is that the method of calculating the efficiency of distribution is open to doubt. I think Dr. Fleming, or any of our highest authorities on this subject, will agree with me that the station output in units generated cannot be obtained by multiplying together the virtual volts and virtual amperes, as has been done in the paper. I am sure that the 25 per cent. loss in distribution given in the paper is too low a figure—that is to say, if it includes all losses in the consumers' meters as well as the use of electricity by the company itself. My criticism on this point will not, however, affect my subsequent remarks, as I am glad to see that the authors have very properly taken out all the costs at per unit sold. Turning to these, I do not propose to discuss the two items of repairs and general charges, although I congratulate the authors on their being able to reduce them to the low figure given; but these figures are no criterion as to what they will be in the future, and, moreover, cannot be easily brought in comparison with corresponding charges of other companies employing similar or different systems. The costs, however, which are of great interest and can be usefully discussed, are those relating to fuel, water and petty stores, labour and superintendence. The authors have been at great pains to supply us with the means of making a comparison between the coal duty given in the paper and that of the London supply companies, who have for some time been publishing their results. I think, however, that they may have saved themselves the trouble of supplying us with the analysis of the fuel, as, after all, they themselves give a far better means of comparison, namely, in the consumption of water used per unit sold: they give this at 107·8 lbs.

I find, on turning to the accounts of several companies with which I am connected, that 60 lbs. per unit sold is fairly representative of the consumption of water by the best London companies using the direct current, and that with the boilers used in London 8·2 lbs. of Welsh coal, costing 24s. a ton, was used to evaporate this quantity of water. The cost for coal in London, therefore, may be taken at 1·06d. per unit sold; and if the Newcastle figure be increased in the proportion of 60 to 107·8,

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it makes the Newcastle figure 1·88d. I can now fill in the remaining figures, so that you may have them before you for purposes of comparison. Mr Crompton.

	London Companies. (Direct Current.)		Newcastle Company (Alternating.)
Lbs. of water per unit sold ...	60	...	107·8
„ coal „ „ ...	8·2	...	14·6
	Costs.		Costs.
Coal, at 24s.	1·06d.	...	1·88
Oil, petty stores, and water ...	0·17	...	0·219
Labour and superintendence	1·10	...	1·10
	2·33	...	3·20

These comparative figures show that the system used at Newcastle costs 40 per cent. more than the direct system used in London. If the comparison errs at all, it errs in being rather too favourable to the Newcastle system, especially under the heading of "Labour and superintendence." I am by no means satisfied that the figure 1·1d. per unit for all labour and supervision is a sufficient one. The sum expended inside the station apparently is £830, equal to about £16 15s. a week. The authors state that this sum is divided between superintendence and the wages of three shifts of men. In whatever way it is divided, it will be seen that it leaves a very trifling sum as the cost of superintendence; whereas the London figures, being for only two shifts of men instead of three, allow of good wages to be paid, and a fair margin for well-paid supervision. I do not even hint that these figures are incorrect, but simply that, as during the earlier stages of a supply company it is so easy for the engineering staff employed to charge a large proportion of their time on to construction of works, this cost of supervision is apt to be a misleading one. I think there is little doubt that future and more correct comparisons will show that the Newcastle figures for the above items will be from 50 to 60 per cent. higher than the corresponding London ones.

Whether this increased cost of production can be wholly or partially compensated for by the reduced first cost of the plant on the alternating system, I cannot say. The figures the authors

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Mr. Crompton.

give as the capital cost per lamp cannot fairly be compared with the corresponding figures of the London companies, who have, owing to special circumstances, been compelled to lay nearly the whole of their distribution system over very large compulsory areas. The London companies have had to pay in the first instance the full cost of sites and expensive buildings. I have had considerable experience in examining projects and estimates for the supply of plant on both alternating and direct systems, and even in cases where the area has been a very extended one I have found that the first cost of the alternating system shows little or no saving over that of the direct system. I think that the paper shows as beyond doubt that up to the present the *working* cost of the alternating system, under the most favourable circumstances, cannot compare in economy with that of the direct system; and the reasons are not far to seek.

First, the efficiency of the alternating plant as generating plant is inferior to that of the direct-current plant. The authors quoted a statement from my paper read before the Institution of Civil Engineers, in which I gave the net generating efficiency of direct-current plant; that is to say, the E.H.P. generated reaches 84 per cent. of the I.H.P. of the engines. This efficiency is now frequently surpassed, as I have under my own charge plant made by Siemens Brothers that when tested gave, and now in regular working continues to give, an efficiency of 87 per cent., and the actual working cost of this plant is far below the figures that I have given for last year's working.

Turning now to the load factor question, the fraction,

$$\frac{244{,}470 \times 1{,}000 \times 100}{342{,}000 \times 8{,}760} = 8{\cdot}16 \text{ per cent.},$$

is wrong; the denominator should be the greatest load in watts observed, multiplied by 8,760. This greatest load is not actually given in the paper, but it can be inferred from the authors' remark that it was 45 per cent. of 431,000 watts: this is equal to 195,000 watts. The fraction should, therefore, be,

$$\frac{244{,}470 \times 1{,}000 \times 100}{195{,}000 \times 8{,}760} = 14{\cdot}2 \text{ per cent.},$$

which is rather a better load factor than we find in most parts of

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London, and which explains the fairly high income earned per lamp in spite of the low price charged per unit. Mr. Crompton.

Mr. J. F. ALBRIGHT: I have made one or two notes on the subject of this paper. Of course Mr. Crompton has completely taken the wind out of my sails on all technical points, which are not my *forte* at any time. But one or two points occur to me on general grounds. It appears to me we have learnt one thing very prominently from this paper: not only is coal dirt cheap in Newcastle, but superintendence also is to be got for nothing; because, as far as I can see, taking the labour and supervision charges, which are put down at 0·8163 of a penny per unit, and allowing less for wages of assistants than Mr. Crompton has allowed—in fact, allowing what I believe is most likely to be actually paid—there would be left £100 per year for superintendence. Well, if the supply company can get the services of a gentleman who can deal with figures in the masterly way they have been dealt with in this paper for £100 per annum, they are extremely fortunate. Then there are some details in regard to this paper more matters of interest than criticism. I notice that at Newcastle the unit is sold for 4½d., and the lamps are taken as 30-watt lamps; if my figures are right, each 30-watt lamp in Newcastle yields a gross revenue to the company of 8s. 3d. Well, now, in London, the hardly used consumer who bemoans his lot because he has to pay 8d. per unit, pays practically 8s. 6d. or thereabouts for a 33-watt lamp; in other words, the average price in London at present is the same as in Newcastle, and this in spite of higher rents, and of the fact that coal is costing us in London about four times as much as in Newcastle. I believe this rather curious fact points to an explanation of the unusually favourable position of this particular company—namely, that they have been able to pick out the plums of their district, and get hold of the "long-hour cus-"tomers." Whether this favourable condition will continue when the short-hour customers come on more extensively, remains to be seen. I think we ought to thank the authors for having put such a lucid paper before us. Mr. Albright.

Mr. W. H. PREECE: I think it would only be fair, as represen- Mr. Preece

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Mr. Preece. tative of one of the largest clients of this company in Newcastle, that I should say this—that during the whole time the current has been supplied to the Post Office at Newcastle, it has given the very greatest satisfaction. It is quite impossible for the light to behave steadier and better, and it is my experience everywhere, where you are dependent upon the alternating-current system, that your E.M.F. is more constant than it is with any direct-current system. I think that the criticism that Mr. Crompton has passed on this paper is both just and generous; but at the same time it reminds me very much of the attempt of a critic to criticise the play of "Hamlet" when the character of Hamlet is left entirely out. Now I do not know whether it has struck the audience as the paper was being read, as it struck me, that one of the most startling and one of the strangest parts of this paper is, not the cost of working, but the capital expenditure. Now it is said here that the capital expenditure amounts to about £50 per kilowatt: that runs out at a capital expenditure of something under £2—in fact, about 30s.—per lamp. That is a capital expenditure which is less than the capital expenditure in London on the mains alone of some of these low-pressure systems. Now, gentlemen, my experience is this—that if you examine the reports of these companies that have extended electric lighting over the metropolis and over other parts of the country, you will find that in some cases the capital expenditure averages from £5 to £6 per 8-C.P. lamp; whereas, if you examine this report, you will find that it comes out at 30s. per lamp. That means this: if you establish a system over a place like Newcastle at one-third the capital expenditure, you can afford to work it at twice the cost, and even then you find you have an ample margin to pay a very handsome dividend to your shareholders. Again, another feature connected with this system in Newcastle—a feature often ignored—is the fact that you are able to extend your system to great distances: the paper says to a distance of 2½ miles. Now, is that done on any low-pressure system in London? And, again, it has also been pointed out that by this system you are able to extend your pioneer system over a very large area at a very small cost, and you are able to accommodate

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scattered clients in different parts of a great district at an expenditure averaging less than £2 per lamp. I quite agree with Mr. Crompton that we are deeply indebted to these gentlemen for having the courage of their convictions, and for their honesty of purpose in bringing before us authentic facts and figures; and I think we must look at these figures from all sides, and not alone from the mere question of cost of working expenditure. The information in this paper on capital expenditure is meagre: we are simply told it amounts to about £50 per kilowatt. If either of the authors would favour us with some details of that expenditure, and show us how much is devoted to building, how much to steam machinery and dynamos, and how much to distribution and to mains, they will confer an additional favour upon us, and enable us to criticise them in the same just spirit which I hope we shall do during the whole evening. Mr. Preece.

Sir DAVID SALOMONS: As you know, I am interested now in one of the largest undertakings in London—that of lighting the City—and the cost of working I hope at a future date to bring before you. I think, if a report of this paper were to get into the journals of to-morrow, Londoners would at once cry out, "In "Newcastle people get the current for 4½d., while here we have to "pay 8d." I would point this circumstance out—not in the way of criticising the paper, but merely to state the fact—that in reality the people of London are paying less for their current than those in Newcastle. To obtain a fair comparison, you must compare your electric light bill with what gas, candles, and oil were before the new light was adopted. You must not lose sight of the fact that gas is 1s. 10d. per thousand feet in Newcastle, but in London it is something like 3s. 4d.; and if you work it out to get the equivalent values, Londoners will find that the Newcastle station in London would have to charge 9d. per B.T.U., whereas the actual price is 8d. This fact must not be lost sight of, independently of the questions which might arise as to whether the continuous or alternating current is most efficient. I am very much with Mr. Crompton in favour of the continuous current, and I disagree with Mr. Preece that the alternating current is more steady than the direct, although in Newcastle it may be extremely Sir D Salomons.

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Sir D. Salomons.

good. I can answer for this fact—that the London Electric used to supply me in London, and it was most irregular; and now that the Westminster Electric supply me, the current could not be better, and only varies 1 per cent. over the whole day, and it has not been cut off, excepting once for half an hour by an accident, for a year and two months. It will be most interesting eventually to have the figures of alternating and direct systems compared, and I think the figures of the City of London will be of enormous interest. There are 8,000 or 9,000 lamps on already, and before October there will be over 30,000, and after that time the figures will be sufficiently accurate and the engines sufficiently loaded to obtain results which will be of value. Besides these incandescent lamps, there will be 400 or 500 arc lamps running. The statistics to be obtained at a later period will have an especial interest on account of this company having started with the large experience gained by its predecessors.

Mr Mordey.

Mr. W. M. MORDEY: I did not wish to occupy any time this evening; I wanted the low-pressure people to have plenty of rope. Really, it is a paper on which I have nothing to say except in commendation. I agree with nearly everything done at Newcastle; but as you have been good enough to call upon me, there are one or two things which have arisen in the discussion to which I should like to refer, if I may be allowed to do so. As to the method of measuring criticised by Mr. Crompton: The volts have been multiplied by the amperes. That, I know, is a dreadful sin. There ought to be a new commandment, "Thou "shalt not multiply the volts by the amperes." But I think the authors can do that at Newcastle without being far wrong. I do not think Mr. Crompton can do it at Chelmsford without being very far wrong indeed. The transformers are not the same in the two places. Mr. Crompton has a considerable proportion, I believe, of open-circuit transformers, in which there is a large magnetising current, and in which there is a great disparity between the real watts supplied and the apparent watts supplied. Any slight difference there may be between the real and apparent watts at Newcastle is more than covered by the fact that the meter losses have been treated in the way they have

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Mr. Mordey.

been in the paper. But the value of the paper is not affected by this matter. The really important points of the number of units sold (measured in ampere-hours, be it observed), and the coal consumption, are not touched by it. It will be granted that the units metered at the consumers' premises are at least 5 per cent. less than the true units supplied. And as with this system the secondary volts are sensibly constant, and as the secondary circuits are non-inductive, the secondary watts recorded are for practical calculations correct. And if we confine our attention to the recorded secondary output, and to the cost of generation, we get all that commercial requirements demand—namely, the number of units sold, and the cost of those units.

Mr. Crompton appears to think that alternators are necessarily not as efficient as direct-current dynamos. I do not think there need be any difference, and, at any rate, the question need not remain a matter of opinion. I will undertake that the Brush Company shall put one of their alternators at the Crystal Palace at the disposal of the Expert Committee or the jury, and run it direct, not on transformers, but on a lot of incandescent lamps. We will run a 100-unit alternator direct on to those lamps, arranged in series for that purpose, and Mr. Crompton can run a 100-unit direct-current dynamo on the same lamps, and we will see who can run the most lamps with a given power. That test would put the matter at rest perfectly. It does not involve anything of a doubtful or disputable kind. We have merely to take the C.P. of the lamps and the number of them, arranging them in series or parallel to suit the machine.

Although I am pretty confident that such a test would show that an alternator is as good, or better than a direct-current dynamo, it would not settle any large question as to the merits of the two, for it seems to me that the conditions of this test are not the conditions of practice. If Mr. Crompton had the lamps scattered over two or three square miles of country, as at Newcastle, he would have very much greater difficulty in supplying them than the authors have had.

A point in the paper which is deserving of special notice is the extremely small labour staff necessary to run the machinery. The

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Mr. Mordey authors say they have, for three 200-H.P. sets, only three working men in the station—stoker, engine-driver, and assistant. That speaks volumes for the character of the plant, and I think it will be difficult to get a direct-current plant to give better results than that. Reference is made to parallel working in the station. Perhaps I might supplement what has been said by the authors by stating that I think we are agreed that it would be an advantage to run in parallel, and I should like to remove any misunderstanding that may be felt as to why that method is not at present in use. The alternators cannot be got out of step; the failure is not due to the fact that they would not keep in step. I would undertake to pull the coils out of the machine before they would get out of step. But it is not found advisable to run with the driving conditions they have there. The engines are satisfactory in most respects, but are not even throughout the stroke. Attempts have been made, by increasing the weight of the fly-wheels, and by the use of spring pulleys, to improve them in this respect, so that the present difference of speed between the periods when the engines are taking steam and the periods when they are going over the dead centres shall be reduced. At present there is a difference of speed which necessitates transference of power from one engine to the other through the dynamos, and a synchronising current of a considerable amount is necessary. Of course there cannot be two opinions as to the desirability of working a station on the parallel plan, whether it is alternate or direct current. The complication and trouble of switching circuits from one machine to another is very objectionable, but still more objectionable (from a dynamo designer's point of view, at least) is the sudden changing of load in this way. This sudden switching over imposes a strain that should be avoided if possible.

And while on this question of parallel working, I should like to say that although there has been a good deal of discussion about it during the last two or three years, I would make no claim whatever to have invented parallel working. I am very often reproached with having claimed to have invented parallel working. I never did anything of the kind, and I put formally and fully before the Institution when I read a paper in 1889 what had been

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done previously. All alternators, as I then pointed out, have a tendency to run in parallel, but the difference is this: To exert a given synchronising effort one machine may require a greater current than another. The whole thing may be set at rest on that point by a simple test. Let any alternator be run up to speed, thrown into synchronism, and run as a motor, idle. The machine that will run best in parallel is the machine that will run with the lowest current to so drive it as a motor. This does not cover the whole ground, but it is enough; I need not now enlarge on it. Mr. Mordey.

Earl RUSSELL: If I may be allowed to say one word on a subject not exactly connected with the subject of the paper, I do not think Mr. Mordey must take advantage of the absence of Mr. Swinburne to suggest that there is no magnetising current in a closed-circuit transformer. I would rather suggest that the magnetising current of an open-circuit, compared with a closed-circuit transformer, is as 3 to 9 or 10. Earl Russell.

Mr. MORDEY: I do not want to occupy time by dwelling on the design of transformers now, as we have much more important questions before us. In reply to Lord Russell I would only say I have found—what is, I believe, generally admitted—open-circuit transformers take from 25 to 35 per cent. magnetising current. I am in the habit of designing transformers to work with from 1 to 2 per cent. Mr Mordey.

Mr. W. B. ESSON: When discussing this point with Mr. Swinburne last night, I understood him to say that the exciting current required for the "hedgehog" open-circuit transformer was approximately one-third of the full-load primary current, and that in the ordinary commercial pattern of this transformer the primary current at no load was about three times that of the ordinary closed-circuit type. The point is important, although perhaps not bearing directly on the present discussion. Mr. Esson.

Mr. E. T. CARTER: It appears to me that the discrepancy mentioned by the authors of the paper, in connection with the horse-power wasted in the station, may partly arise from the manner in which the I.H.P. was measured. They have pointed out a discrepancy between the efficiency obtained at their station Mr. Carter.

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Mr. Carter. and the efficiency which Mr. Crompton has shown should be expected at such a station. I think I am correct in saying that the authors measured the I.H.P. by taking the area of the indicator card down to the vacuum line. The losses due to back pressure on the low-pressure piston were not deducted in estimating the I.H.P., but were credited to the station losses. Of course this would give too high a value both for the I.H.P. and the station losses, and would make the efficiency of the station appear too low. The authors were really unfair to themselves in thus measuring the H.P. of their station and the losses occurring in it; for had they adopted the usual practice in determining the I.H.P., the efficiency of the station would have been a higher figure. I think I am correct in saying that this method was adopted, and that this deduction due to back pressure was charged to the station losses?

Mr. HEAVISIDE: That is correct.

Mr. CARTER: That would give too high a figure for the horse-power, and you obtained a fallacious and, unfortunately, inferior efficiency. This will partly account for the serious discrepancy already mentioned.

Mr. Richardson. Mr. JOHN RICHARDSON: With your kind permission, perhaps I may be allowed to say two or three words, principally with regard to the engine performance alluded to. The total loss between the I.H.P. at the engine and the E.H.P. given out to the customers has to be accounted for in some way. The authors have quite honestly put down 27·5 per cent. to friction. Of course that is an assumption. I should scarcely allow that so much friction is expended in the engines, excepting that sometimes, and for a considerable time, they are working with very low loads indeed. When the engines are working at their fair power, the percentage of friction would not be more than 15 at the outside. The low loads would also account for the 27 lbs. of water. That quantity is the result of a very long test, taken over 12 months. If that test had been taken over a few hours when the engines again were working at their maximum efficiency, it would be nearer 17 lbs. per H.P. per hour than 27 lbs. One word about the difference of speed. The engines are fitted with very

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sharp cut-off gear, and it was thought that there might be a difference between the speed of the various parts of one revolution of the engine. With a very elaborate electrical apparatus which Mr. Heaviside placed at my disposal, I spent some hours trying to find out what differences there were between one part and another part of the revolution. I do not say that there is absolutely no difference, but there was no difference that we could detect with the electrical measurements then used. It might be possible at some time to detect differences that were not detected then. One word about the cheap superintendence. I think perhaps the authors would say—I merely throw it out as a suggestion—that there is even some margin for that skilled superintendence which Mr. Crompton found is got for nothing. But three men are in the station—two workmen only—and the whole of the machinery, it is fair to say, works with so little trouble that highly skilled and highly paid men are not needed. Just ordinary mechanics, at comparatively low wages, very soon attain the necessary knowledge to manage such very simple machinery, and thus there is a considerable margin left for that skilled attendance that lies so near the heart of Mr. Crompton. Mr. Richardson.

Mr. A. W. Heaviside: In reply to Mr. Crompton's remarks, we do not think that for commercial purposes the method of measuring the output of the station is objectionable where closed-circuit transformers are used. What error there may be is not material, as the coal consumption and the units sold are the main points to observe; but whatever it is, it will be balanced by the tendency the staff have to over-read rather than under-read the ammeters. For a long time during a 24 hours' run, the four ammeters each on a separate circuit read very low down upon the scale, and there is room for guess-work. But it would be interesting to learn from Mr. Crompton what he supposes the percentage of error to be. Mr. Crompton, in stating that 10 per cent. of the total power generated was absorbed in meter losses, gave the alternating-current advocates a powerful argument against himself, as this loss was included in the 25 per cent. difference between the station units and the units sold. Mr. Heaviside.

As regards the magnetising current and line losses, roughly

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Mr. Heaviside.

about 10 amperes are indicated at the ammeters upon Sunday mornings, when it is known that there is only a small load on the Post Office installation, and few, if any, of the transformers are switched off. This indicates where some of the 25 per cent. loss goes, the balance being due to greater losses with heavy loads and meter losses, that is, unrecorded current.

This branch of the subject is being watched very closely, and it is hoped that at some future meeting of the Institution we may be able to afford trustworthy data of where the energy is absorbed. At present the coal consumption, indicator diagrams, and the units sold, give a very solid foundation for such figures as we have been enabled to put forward. In running a station such as the one under discussion, where such simple machinery is employed, a stoker at 25s. per week, an engine-driver at 27s. per week, and an assistant at a variable cost—sometimes he is an apprentice or an improver, sometimes he is an assistant engineer at a higher rate—a sufficient margin is left for supervision. Mr. Crompton manages to get through the 24 hours with two shifts of men. We are of opinion that two shifts of men will not be found sufficient for the permanent working of central station plant in London or anywhere else, whether the plant be of the alternating- or of the direct-current type. We think that Mr. Crompton should have included the item of repairs in his analysis of the respective costs, as the maintenance of the class of plant employed at Newcastle will eventually probably compare most favourably with the class of plant generally used by the London companies. In all the items of the fuel account the losses are being gradually stopped. There is no reason to doubt but that the economies which are being effected by the direct-current companies can also be effected by those companies using alternating-current plant. The question of the relative efficiency of the alternating- *versus* direct-current dynamos may be safely left to the result of the challenge made by Mr. Mordey, and accepted by Mr. Crompton. The load factors adopted by Mr. Crompton, and appearing in various guises in his paper, are apparently of little use in comparing the relation of everyday output to possible output, or of comparing one year's work with

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another year's work, or the operations of one supply company with those of another supply company; whereas the load factor we have adopted enables us to compare year by year, or at any moment, what we are doing. A study of the progress diagram shows, amongst other things, what the greatest load is at any time, the maximum working output of the plant, the greatest weekly output, and the growth of the external installation. It is simple to keep, and affords at a glance a bird's-eye view of the supply company's operations. Mr. Crompton will there find the greatest load at any time of the year. Mr. Heaviside.

Mr. Albright's suggestion that the company has picked out the plums is quite erroneous. It is expressly stated in the paper that small rather than large installations are the rule, and 7s. 6½d. is the average revenue per 30-watt lamp per annum; thus:

$$\frac{\text{Average lighting hours} \times \text{watts per lamp (30)}}{1{,}000} \times \text{net cash per B.T.U.} = \frac{714 \times 30}{100} \times 4{\cdot}222\text{d.} = 7\text{s. } 6\tfrac{1}{2}\text{d.}$$

Sir D. Salomons's point is as to whether the ratio between the London price for gas and for electrical energy is the same as that which obtains in Newcastle. Assuming Sir D. Salomons's figures to be correct, there is a remarkable agreement; for if 3s. 4d. per 1,000 cubic feet is the price for London gas, and 8d. per unit for electric supply, then 1s. 10d. for Newcastle gas equals 4·4d. for electric supply. The Newcastle company actually received 4·222d., making the comparison in favour of cheaper electricity *pro rata* at Newcastle. If the company received 4½d., then the equivalent in London would be 8·8d.

Mr. Preece has touched upon the kernel of the case—*i.e.*, for a small capital expenditure a large area can be served efficiently; and the figures in the paper show that the cost of generation and maintenance is very moderate. If, owing to some unforeseen reason, in future development the cost should go up, then with so small a capital expenditure, details of which are given below, the supply company will be in a better position to meet the increased cost than any direct-service system involving three to four times the capital expenditure to serve a similar area.

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Mr. Heaviside.

Statement showing Capital Expenditure upon an Installation of 26,000 30-Watt Lamps wired.

	£	s.	d.	
Buildings	0	2	8	per 30-watt lamp.
Station plant—				
Boilers				
Water service ...				
Stokers				
Steam pipes	0	13	1	
Engines				
Dynamos				
Switch-board	0	0	7	
Street pipes and boxes	0	4	7	
Mains and leads-in ...	0	4	7	
Loose plant and tools	0	0	4	
Transformers, meters, and switches ...	0	4	2	
	£1	10	0	

Say £50 per kilowatt wired.

We venture to think that the point in favour of this alternating system is its simplicity in distribution; and, as Professor Perry has asked for more details, we propose to write another paper for a future meeting of the Institution, in which to lay bare every point.

In calculating the cost of production, is it not much safer to work upon the basis of the coal consumption, rather than that of the water used, there being so many leaks by which water may get away? For instance, the progress diagram shows that when the weekly output reaches 10,000 units per week the energy can be produced for one penny half-penny per unit. A time must come when the summer load will equal that, and though we cannot be certain that the ratios will remain the same, we think that it is fair to assume that the cost of production will diminish rather than increase.

By the end of the summer it is expected that the question of

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parallel running will be settled one way or the other, as the arrangements now in progress to secure that end will then be completed; but it does not appear to us that parallel running is the only true solution of the problem of distribution from a central point. There are many risks about it, and having too many eggs in one basket is one of them. Mr. Heaviside

Mr. Carter, in criticising the steam diagrams, drew attention to the fact that back pressure was included. So long as the amount of back pressure was stated, there is not much objection to have it included, as it represents real work; but, deducting back pressure as the more usual form of expression, the commercial efficiency would be:

$$\frac{\text{E.H.P.}}{\text{I.H.P.}} = \frac{134}{191} = 70 \text{ per cent.};$$

or, allowing for the ropes,

$$= \frac{134}{191 - 17{\cdot}4} = 77 \text{ per cent.}$$

Mr. Carter, in a letter of his to the *Electrician*, dated June 3rd, seems to have quite misunderstood the purport of the I.H.P. diagrams exhibited, which were for the purpose of illustrating the steam distribution and action of the valves for the full range of the engine.

Mayer valves are not fitted, as stated by Mr. Carter, to the horizontal engines, and these are in practice not run at less than half load. At full load the cylinders are practically balanced; and, in addition, such matters as variations of steam pressure, &c., have to be considered, all of which have had their influence in determining the valve setting. It is not a question of class-room diagrams, but of trying to so arrange matters as to get the best all-round results. As to Mr. Carter's criticism of the spring pulleys, it is sufficient to say that they are neither clumsy nor dangerous, and we intend recommending their use in succeeding sets of plant.

It remains for us to express our thanks to the directors of the Newcastle-upon-Tyne Electric Supply Co., Limited, for permitting us to publish the results of the company's working over the past year. It is only by publishing such results for free and inde-

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Mr. Heaviside.

pendent criticism that they can learn to correct their errors and endeavour to make some little progress for the common good, and help to push forward the electrical industry. In the case in point the distinctive feature of the company's work has been that they have had a perfectly free hand to go to the open market and buy that which appeared best suited to their needs, having done nothing to bind themselves to any particular system in the future. Should the advance of science show that other machinery could be employed to better advantage to that which they now possess, the elastic system of street distribution adopted will enable them to apply existing machinery to a section of the city, using more modern and advanced methods for other sections. There can be no standing still, and all this paper pretends to be is a statement, up to the 31st December, 1891, of what it has cost to put down an alternating plant and serve nine miles of route—where pipes are on both sides of a thoroughfare, the distance being only counted once—with electric energy; no promotion or other costs of a like nature having been incurred—in fact, it is a case of electricity upon its merits.

The PRESIDENT: I beg to propose a vote of thanks to both authors for the interesting and useful paper which they have given us.

The vote of thanks was accorded by acclamation.

The PRESIDENT announced that the following candidates had been duly elected:—

Associates:

Juan Abella.	A. Lee Pogson.
J. Devonshire.	George Rolf.
Haydn T. Harrison.	Hugh Treherne.

Henry E. Walter.

Students:

Arthur Frederick Harris.	George Thomas-Davies.
Frank Holmes.	Leopold L. du Verge.

The meeting then adjourned.

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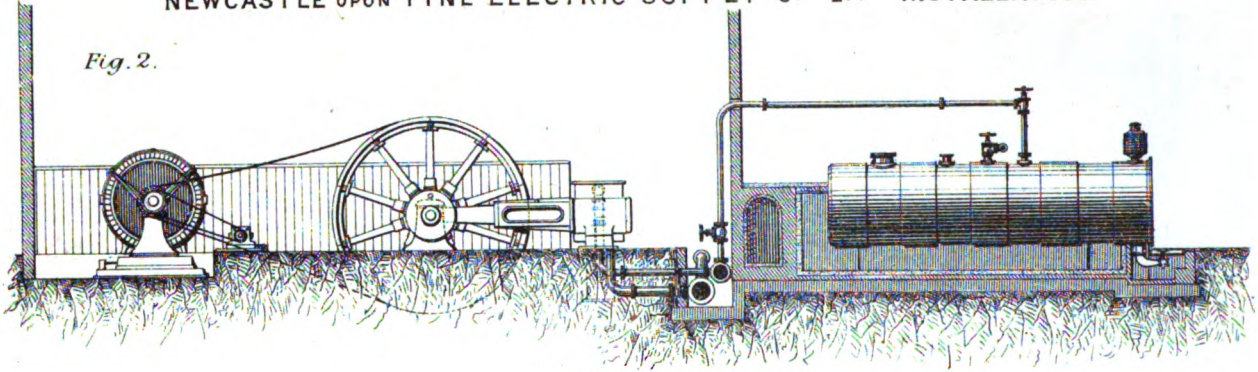
NEWCASTLE upon TYNE ELECTRIC SUPPLY Co Ltd INSTALLATION

Fig. 2.



ELEVATION.

Fig. 1.

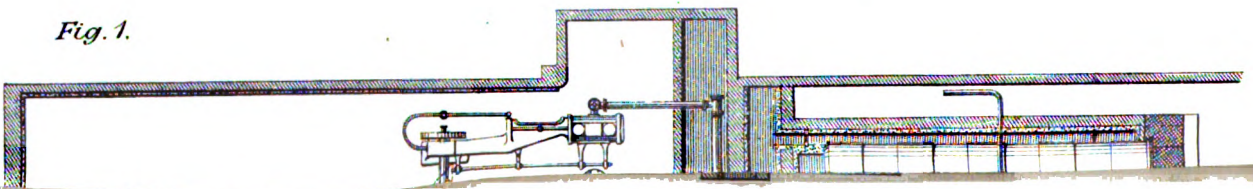


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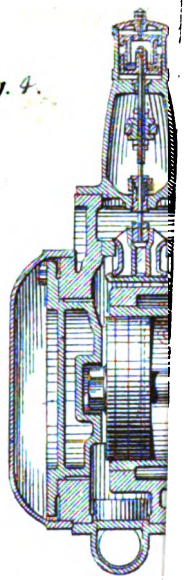
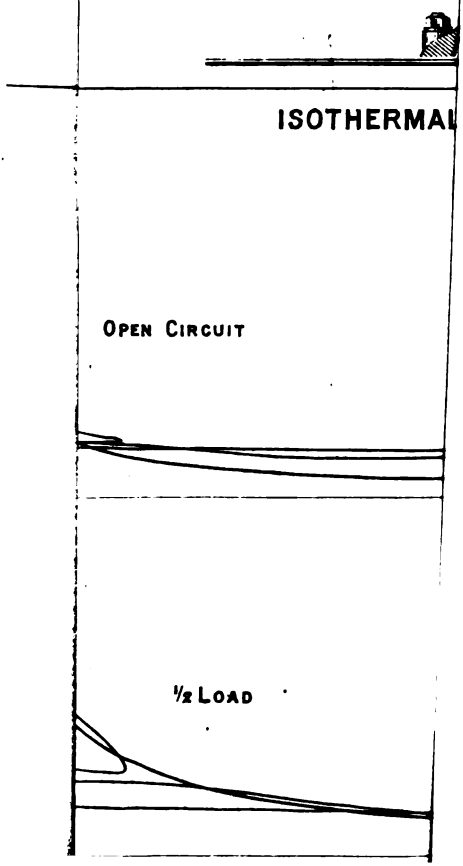
Fig. 4.





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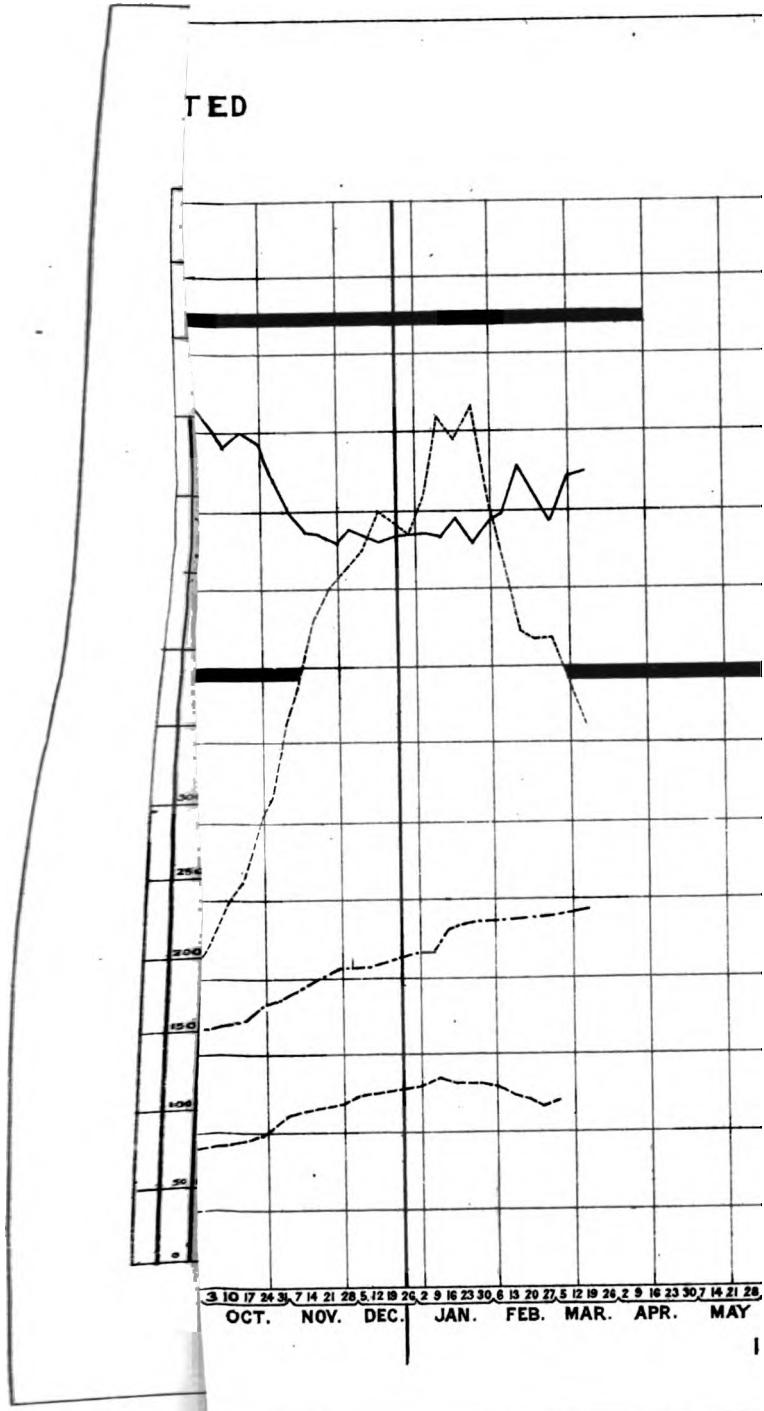

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THE PROBLEMS OF COMMERCIAL ELECTROLYSIS.

By J. SWINBURNE, Member.*

It would be idle to attempt to go over the whole subject of the technical applications of electrolysis, as, although this development of industry is of very recent growth, it is already so extensive that many of its branches demand evenings devoted exclusively to their discussion. My object is, however, to take a sort of bird's-eye view of the subject, and, if possible, to try to foresee the directions in which others must seek for future developments.

There is a very general feeling that there is a mine of untold wealth in the application of electricity to metallurgy; but chemists are not generally electricians, and electricians are seldom chemists. It seemed, therefore, advisable to bring the paper before this Institution, in the hope that it will result in a discussion; and if it leads to the expression of the views, both of electrical engineers and manufacturing chemists, and to a further approach to common ground, our time will be well spent.

However puzzling the theory of electrolysis may be, for commercial purposes it may be regarded as involving no mystery. In fact, broadly speaking, we may disregard the electrical side of the question and look upon electrolysis as a method of oxidising and reducing, and nothing more. It must be remembered, however, that there are often many ways of oxidising the same substance. For instance, a sulphide may be oxidised into a sulphite or sulphate, or sulphur may be liberated. Electrolysis does not always afford the means of oxidising in the particular manner desired. A very large number of the processes employed in chemical industry consist merely in oxidising and reducing, special conditions being arranged to suit the particular substance being operated upon. Once we take this broad view, all we have

* This paper will be discussed at one of the first meetings after the recess.

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to consider is, "Which of the oxidising and reducing processes in "use in chemical works can we advantageously replace by electrical "methods?" In many cases electrical methods are impossible; in others they are more expensive than those at present in use; but it is hardly too much to say that in an enormous number electrolysis is destined to play a leading part in the near future.

Take, for instance, the case of reduction. This is almost universally effected by means of hot carbon, and at first sight it seems unlikely that it could pay to burn the carbon under a boiler, and get a small percentage of the energy back in the electrical form, and to utilise that portion alone. It must be remembered, however, that the methods of reduction by burning carbon are themselves exceedingly inefficient, and, moreover, they very often give impure products. By means of electrolysis, however, it is frequently possible to secure metals of the highest purity by direct processes. Before going further, it will be wise to form some idea of the cost of electrical energy in, say, a large chemical works. Of course "cost" is a loose term: we might calculate it from the cost of coal and labour and sundries; or we might add interest and depreciation, rent and superintendence. In all cases in estimating the cost, further additions have to be made for standing charges, advertising, profits, maintaining patents, &c. It is unnecessary to trouble ourselves about definitions of cost, for if it is stated how the figures are obtained, the selling price can be worked out and comparisons can be made.

Suppose a plant is put up to give a million watts: it will be found that if the plant is taken as running day and night, including Sundays, and if the best engines, with modern dynamos, are employed, the cost comes out about 0·25d. per kilowatt-hour. This figure includes interest, and depreciation of engines, boilers, and buildings, and coal, labour, and petty stores; but does not include rent or salaried superintendence. This figure will appear ridiculous to those accustomed to London central stations; but that is because central stations work under the most disadvantageous conditions possible, while a chemical works has everything in its favour.

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Oxidation and Reduction.

Oxygen.

The first obvious application of electrolysis is the decomposition of water. In an acid solution platinum or lead anodes are necessary; platinum is expensive, and lead takes a high electro-motive force, so it would be best to use an alkaline solution with iron electrodes. Allowing 2 volts as the pressure required, the cost comes out at 5s. 7d. per 1,000 feet for the oxygen. The hydrogen is taken as valueless. As the output in oxygen must be small, we must take a much higher figure than the farthing per kilowatt-hour—in fact, it would be safer to double the price and say 11s. 2d. To find the selling price we must allow for packing in cylinders, and add all the trade charges, which will probably bring the price up to something like 15s. per 1,000 cubic feet. The retail price of Brinn's oxygen is 2d. per cubic foot, so there would appear to be a very large margin.

Alkali.

We come next to a subject which has already absorbed an enormous amount of labour and thought, and that is, the production of caustic soda and chlorine from common salt. The Leblanc and Weldon process is exceedingly complicated and very costly; and as by simply electrolysing salt, costing about 15s. a ton, chlorine and caustic soda can be produced, it seems, at first sight at least, most strange that electrolytic processes have not replaced all others within the last few years. Assuming that the process works, let us first form a rough idea of its probable economy. With suitable vats, and a reasonable current-density, we may take it that 3 volts is a liberal allowance. For the ton of 70 per cent. caustic soda we have—

	£	s.	d.
Electrical energy...	2	15	0
Salt	1	2	6
Lime	1	3	0
	£5	0	6

For £5 we have thus obtained a ton of caustic and 1½ tons

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of bleaching powder. We have allowed nothing for the cost of vats, for labour at the vats and in handling, or for evaporation of causticised liquor. The ordinary caustic liquor after causticising is quite weak, but electrolytic caustic can be made into a strong lye. On a small scale I have produced lye with 30 per cent. caustic. To do this the brine is electrolysed and then stirred up with more salt, and so on; the process being repeated several times. Strong caustic is a good conductor, so this method is good from that point of view as well as from that of saving in evaporation. Evaporation with a Yaryan should not come to more than a shilling or two a ton extra. It would doubtless be safe to say that, taking this way of estimating the cost, a ton of caustic and a ton and a half of bleach should be made for £6. This allows an enormous margin for expenses and for profits. This method of estimating is very rough; and unless a new process worked out in this sort of way comes out with an enormous margin, it is not likely to be worthy of serious consideration. On looking into it further, it will generally be found that when all the details of expenditure which will be necessary in practice are added, the margin, if it still exists, is too small. In fact, a new process must look marvellously good on paper to be really worth anything when tried commercially. Electrolytic soda and bleach seems to have an excessive margin, however, and we have to see whether there is any other weak point.

I believe the difficulty, and the only difficulty, lies in the anodes. Process after process is brought out, and each claims to have solved the problem of electrolytic soda by the use of some new arrangement of the porous partition, or by some other detail which can hardly make all the difference between success and failure. The various processes make a little excitement for a time, and then disappear, I believe because the anodes are destroyed.

Of course none of the ordinary metals can be used as anodes, as they are attacked at once. Platinum is generally supposed not to be attacked by chlorine; but I find that it is slowly eaten away. I would like to hear this confirmed or denied by others, for it is a very important point. On the one hand I find experimental anodes eaten away; on the other we hear of platinum

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anodes being used extensively in the electrolysis of chlorides. Of course, in the manufacture of such a cheap commodity as soda, the capital invested in platinum anodes would be prohibitive; and the least waste of metal would lead to ruinous expenditure. The next material to consider is carbon. In 1882 Bartoli and Papasogli pointed out that carbon was attacked when used in any solution which evolves oxygen. Without knowing of their work, I found the same thing by a series of experiments carried out in 1883. In those days it was tacitly assumed that carbon with peroxide of lead could be used in the ordinary secondary batteries, without any fear of local action. Though carbon does not seem to combine with chlorine during electrolysis, I find that it is always attacked, even in a solution of salt. I have tried every kind of carbon I can think of, and the result has always been that the anode was corroded. I do not mean to say that carbon is eaten away at a rate at all corresponding to that with which the soda is formed; in comparison the corrosion is slow. It must be remembered, however, that a very slow corrosion makes carbon anodes useless when the products are cheap.

Carbon is also troublesome mechanically. It is difficult to make good contacts with it, especially when there is chlorine about. Large carbon plates are also expensive. Mr. Greenwood overcomes this last difficulty by electro-plating one side of a number of comparatively small plates, and then soldering them to a plate of type metal. The type metal is not exposed to the solution anywhere, else it would be attacked by the chlorine. By this means he produces a large carbon anode with a good metallic connection.

Peroxide of lead seems to present a solution of the problem. Unfortunately, however, peroxide of lead is reduced by hydrochloric acid; and it seems to be impossible to electrolyse salt without producing some free acid at the anode. Some specimens of lithanode I tried were attacked; I do not know, though, whether the inexhaustible ingenuity of Mr. Fitzgerald has produced any special make which will stand traces of free acid. I am inclined to think, therefore, that progress is barred in this direction solely by the want of a good anode. If this is the case, there is a fortune

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awaiting the inventor who can find something which will act as an anode in a solution of common salt.

I have tried electrolysing sodium sulphate. There is not the same margin here. You get hydrochloric acid as a by-product, instead of chlorine, so that the Weldon process would still be necessary; and the sulphuric acid would have to be concentrated every cycle, in order to act on the next batch of salt. This process would, in fact, save the black ash part of the soda process; and, like all these methods, would demand carbonic acid to make soda crystals. Of course this last is not a serious difficulty, especially if quicklime is needed to absorb the chlorine. In this case I tried lead anodes, when, to my surprise, the small traces of chloride left in the sulphate corroded the lead anodes.

Paper-makers use large quantities both of caustic and bleach, and they recover soda and causticise it on their works. It would therefore be an enormous advantage to be able to produce caustic and bleach direct on their own premises. I do not know if the presence of salt would interfere in any way with the action of caustic in the grass or straw boilers, but should think not. By making the caustic on the spot, a saving is effected in the processes otherwise necessary. There would be no need to concentrate the lye, or to fish out the salt that may be present. Caustic drums and carriage would be saved; but, of course, against this there may be carriage of salt. The importance to paper works is so great that it is almost a question whether it would not pay to face the corrosion of the carbons, and to arrange vats so that the anodes can be easily replaced.

In the Hermite process the chlorine is not given off, but a hypochlorite is formed. Of course in this case caustic soda cannot be made. For paper works especially it is obviously cheaper to supply lime and get bleaching powder and caustic, than to make sodium hypochlorite only. This may, however, depend on the anodes. Platinum anodes are used in this process, and it seems to be the case that platinum is not attacked in an alkaline chloride. If this is so, it is possible to produce hypochlorite of soda, but not caustic and bleaching powder. The Hermite process is apparently commercially successful. Magnesium

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chloride is generally employed in the Hermite process, but I have been told that salt is now used. It has been said that electrolytic hypochlorites, especially of magnesium, have more bleaching power than the ordinary bleaching powder. Of course a sodium, or any other hypochlorite, may be formed which has twice as much of its chlorine available, when compared with bleaching powder; but this is merely an apparent gain, as it takes twice as much electricity to produce the hypochlorite. Messrs. Cross & Bevan, however, hold that the bleaching power of these compounds is not as determined by arsenious acid. They maintain that the fibres to be bleached contain some ketones or other, and these ketones absorb some of the available chlorine, especially when provided by bleaching powder; that the hypochlorite of sodium and of calcium waste available chlorine on the useless work of chlorinating these ketones, while magnesium hypochlorite devotes its whole attention to honest bleaching. Of course, if it is slightly easier to oxidise the colouring matters than to attack the ketones, it may be the case that a hypochlorite of a weak base, like magnesium, may be able to bleach, but not to attack ketones. It would then be better than the hypochlorite of calcium, or especially sodium. These matters are better left in the hands of such authorities as Messrs. Cross & Bevan. They make out, also, that electrolytically prepared magnesium hypochlorite is best. This seems very strange. Of course it would mean that some other salt is really formed. The properties of a salt cannot depend on the method of its preparation, and there is no magic in electrolysis.

Potassium Chlorate.

The manufacture of potassium chlorate is carried out by electrolysing a hot solution of the chloride. The chlorate is then formed instead of the hypochlorite. This process is carried out on a large scale at Vallorbes, in France, under the direction of M. Gall-de-Montlaur. It is stated that they make a ton a day, 24 H.P.-hours yielding 2·2 lbs. The power is supplied by a waterfall, so the chlorate should be very cheaply made, if the anodes are absolutely free from corrosion. Platinum anodes are employed.

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The Electrolysis of Fused Salt.

Many inventors have been at work on electrolysis of fused salt. At first sight this also seems most simple. Salt is cheap, and is fusible, and therefore all that has to be done is to melt it and electrolyse it, getting sodium at one pole, and chlorine at the other. The sodium could be sold at a very low price for making aluminium, and for amalgamation in gold extraction, and can, of course, be converted into caustic soda or crystals as desired. Consumers of caustic would buy the metallic sodium, and would add water, thus obtaining pure caustic. The margin of profit in this case is apparently enormous.

The difficulties are, however, very serious. In the first place, sodium chloride boils at a very little above its melting point. The result is that the sodium is obtained mixed with large quantities of salt. Melted salt is by no means a convenient substance to handle. It acts on many kinds of clay, so that ordinary fire-clay crucibles cannot be employed. Chlorine at a temperature corresponding to a red heat is also troublesome to deal with. It is said that chlorine will not attack a dry metal, so that iron can be used for the containing vessel. Most patentees describe vessels of iron with porcelain where insulation is necessary.

We now come to the anodes again. As there is no oxygen present, it would seem as if carbon might be used. Patentees always mention carbon anodes. Retort carbon is eaten away, and so are electric light or artificial carbon rods.

It seems strange that carbon should be attacked, and it would be well for these experiments to be repeated, to make sure that there is no error. Of course carbon is the only hope. Whether the chlorine is dry or not, metals are eaten away with great rapidity. The anodes are corroded chiefly at the surface. In addition to these troubles, the theoretical quantity of sodium is not obtained. Some people have supposed that there is a sub-chloride formed, and that sodium is really not a monovalent metal. This is rather an extravagant notion. I do not know why both sodium and chlorine should uot be soluble in the fused chloride without forming any definite compound. So many workers have been unsuccessful with fused salt, that it looks as if

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there were some insuperable obstacle in the way; and it is questionable whether the ground is worth any further labour.

Electrolysis of Fused Soda.

Castner has recently proposed to produce sodium cheaply by the electrolysis of fused caustic. The temperature is to be high enough to fuse the caustic, but not high enough for the sodium to decompose the fused hydrate forming oxide. Whether this process has had any success commercially, I do not know.

Aluminium.

The extraction of aluminium has long been an important problem. The old method was to replace aluminium in its chloride by means of sodium. This involves the manufacture of metallic sodium, and of the anhydrous chloride of aluminium. The anhydrous chloride cannot be obtained by evaporation of its solution, as the solution gives off hydrochloric acid, and leaves alumina; otherwise the anhydrous chloride would be quite cheap. The price of sodium has been reduced recently by the Castner process, but the extraction of aluminium by ordinary means is still so expensive that electrical methods can compete.

It seems to be impossible to deposit aluminium from any of its solutions, so fused salts have to be employed. The electrolytic processes differ in detail only. The electrolyte is cryolite, or a solution of alumina in cryolite. If cryolite is electrolysed, aluminium is deposited, and fluorine is presumably evolved at the anode, which is made of carbon. I have not seen any account of how the fluorine comes off. Yet in some processes it is stated that cryolite is electrolysed by itself. In the Minet process the solution of oxide is employed, and aluminium is deposited, and the anodes are burned away by the oxygen, giving off, according to some accounts, carbon dioxide, but probably the monoxide in fact. The chief difficulties in the electrolysis of cryolite solutions are due to the cryolite itself, as it attacks all the substances commonly employed for making crucibles. It is therefore usual to use cast iron to make the containing vessels or vats, and to heat the electrolyte by electrical power from the inside.

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Minet uses iron vats, and arranges a shunt current, which makes the vats to some extent cathodes, so that they are not attacked by the electrolyte. Presumably, fluorine is soluble in fused cryolite, and attacks iron. By making it a cathode it is always coated with aluminium, so that any fluorine is taken up in dissolving off some of this aluminium. The importance of preventing the corrosion does not lie so much in the slight saving of iron as in the avoidance of any deposit of this metal in the aluminium. The great difficulty is to get the aluminium pure, and any trace of iron reduces its value for most purposes very greatly. Minet reduces the melting point of the electrolyte by the addition of common salt. In the Hall process, carried on at Pittsburg, the electrolyte is practically the same. Calcium fluoride is used to mix with the cryolite, or calcium chloride. The Kleiner, and, I believe, the Hall, processes are carried on in England. The processes have so often been fully described elsewhere, that I need not discuss them at any length here. There is still a great demand for cheaper aluminium; and there is much to be done in making it pure. Aluminium has a strong affinity for silicon, and small percentages of silicon and iron are generally present, and are very deleterious.

Magnesium.

This metal is closely allied to aluminium, and is similar with regard to its anhydrous chloride. There is not the same demand for it, as either it does not give remarkable alloys like those of aluminium, or they have not been investigated. It is said to be made on the Continent by electrolysing the double chloride of magnesium and sodium or potassium obtained from the mother liquor after evaporating sea water. The double salt can apparently be rendered anhydrous without the formation of much magnesia.

Zinc.

Proposals have been made to deal with zinc ores electrically. The blend is to be roasted into sulphate, and the solution dissolved and electrolysed. A great difficulty arises in the deposition of the metal. It comes down in trees. Kiliani proposed to use high current-densities, holding that zinc then comes down in the

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reguline state. Watt prefers to use acetate of zinc as the electrolyte; and alkaline solutions, such as zincate of soda, have also been proposed. I am not aware that zinc has been deposited commercially. Similar processes have been proposed for dealing with the silver-zinc alloy produced in the Parkes or Karsten desilverisation process.

Lead.

So far, little has been done in the way of dealing with lead ores electrically. It has been proposed to make anodes of galena, and to work with lead nitrate as electrolyte, but I have not heard that any such process has been carried out successfully. It is hardly likely that it could, as lead forms troublesome trees in acid solutions. Anyone who has worked with solutions of such salts as lead nitrate would be inclined to give up all hope of obtaining anything like a reasonably compact deposit of lead.

The extraction of silver from lead by the Pattinson process is very cumbrous and roundabout, and is, of course, expensive. Keith started a works in America for desilverising electrolytically. He used a solution of sulphate of lead in sodium acetate as electrolyte. Though the Keith process was brought out some ten years ago, it does not seem to have come into general use. The deposit of lead even from such a solution as sodium plumbite is very bulky and spongy, and is difficult to deal with, as it is apt to decompose water and oxidise spontaneously during handling.

Gold and Silver.

Greenwood and others have proposed to make chlorine electrolytically for catching gold by the chlorine process. As this is exactly the same problem as making bleaching powder, the same difficulties have to be overcome; so it need not be discussed further.

Mr. Crookes's sodium amalgam for making the mercury ready to seize any particles of gold, and to keep it from getting "sick," can be replaced by making the mercury itself the cathode. It is questionable whether this would be worth while, as the sodium amalgam is very convenient.

Mr. Crookes has recently brought out other electric gold

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processes. He finds that the particles of gold become amalgamated, and collect and get caught in the mercury, if the stamped quartz is washed in a weak solution of a mercury salt, under the influence of an alternating current of small frequency. It is difficult to see how the alternating current works. Unless the current-density or the resistance of the electrolyte is something fabulous, there cannot be any current in the small particles of gold, as the minutest polarisation would make all the electricity flow by the electrolyte, and not by the little pieces of gold, even though they are very much better conductors.

Electro-Metallurgy of Copper.

Electrolysis may be employed either in connection with the extraction of the copper from its ores, or in refining copper, especially when there is silver alone, or with gold in addition, to be extracted.

In the first case—that of extraction of copper from pyrites—the ore may be converted into matt, and the matt can be cast into anodes, or the ore may be acted on by oxidising solutions produced by electrolysis, or the two processes may be combined.

In the first method the matt is cast into anodes, which are treated in baths of copper sulphate with copper cathodes. The sulphides are attacked, sulphate of copper is formed, sulphur and impurities being left on the anode. The copper is deposited, and iron remains in solution. The chief difficulties in connection with this process are that the anodes are brittle and troublesome, and are often eaten away unevenly, and the solution becomes richer and richer in iron salts. In addition to this, it is stated that the circulation of the iron salts must be avoided, otherwise ferric sulphate will be produced at the anode and reduced at the cathode; and this process will go on wasting power over resistance. Some sort of diaphragm is therefore necessary.

It must be pointed out that sulphuric acid can be made from the sulphur dioxide given off in the first roasting of the ores, or from the surplus ferrous sulphate. The ore can be roasted so as to have a good deal of soluble oxide of copper, which can be dissolved in the acid and treated electrically. Merely working

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with anodes is an incomplete process; it is better to combine it with extraction by lixiviation. The solution, after being in contact with the anode, so as to contain ferric sulphate, is led to the ore. The ferric sulphate is reduced, and the copper and iron are said to be dissolved, and if there is oxide of copper it is taken up by sulphuric acid. This solution is led to the cathodes, where the copper is thrown down, and is then passed to the anodes, where the iron salt is oxidised. The various impurities and the precious metals are left with the sulphur of the anode. The sulphur is burned into sulphur dioxide, which goes to the chamber, and the silver, with the gold, if there is any, is extracted. The sulphate of iron can be burned into sulphur dioxide, and the iron oxide sold as Venetian red. This is, in rough outline, the Marchese process. It may seem strange to those unacquainted with industrial chemistry that if you want to extract copper you should also become involved in making sulphuric acid and red paint; but in chemical works by-products must be utilised, so that often making one thing involves the production of a number of other substances which have at first sight nothing to do with it. Messrs. Siemens & Halske avoid the necessity of making anodes of matt, by employing carbon. The ferrous sulphate is thus oxidised into the ferric salt, and this is used to lixiviate the crushed and roasted ore. The solution is then led to the cathodes first to take out the copper, then to the anode to oxidise it, and so on.

In the Höpfner process chloride of copper is used instead of sulphate. The action is stated to be as follows:—Cupric chloride is formed at the anodes; this is led to the matt, where it attacks the copper sulphide, forming cuprous chloride and sulphur; and it is also said to dissolve the silver as well as the copper, which, if possible, would be a drawback. I do not know why the chloride should be preferred to the sulphate; and no information is given as to what is done with the sulphur in the ore, and with the excess of ferrous chloride that must be produced.

The Marchese and Siemens & Halske processes are in commercial operation, and the electric extraction of copper is already a growing industry. The electro-motive force necessary for this process is less than a volt; so that, assuming coal to be

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used as in the estimate at the beginning of this paper, the electrical cost is 14s. 5d. per ton for the extraction of copper. I cannot give the whole cost, as that involves so many considerations. This figure will give copper smelters some idea of the saving of replacing the ordinary refining process by electrolysis.

In dealing with such a valuable substance as copper the time element must not be neglected. The interest on the money locked up in the copper under treatment may be a very serious item. This is an objection of some weight in the case of matt anodes, which take some time to dissolve. The whole of the copper turned out is not obtained from the anodes; but, taken all round, it may not be far wrong to assume that the copper takes about three months to pass through the works where matt anodes are employed. If the output is, say, 1,000 tons a month, the copper represents a capital of some £150,000, and the interest increases the cost of refining some 12s. a ton.

Electrolysis may also be employed for refining crude copper. In this case the process is simplicity itself. The crude copper is made into anodes, and thin plates of fine copper are used as cathodes, the electrolyte being sulphate of copper. Pure copper is deposited on the cathodes, and the impurities, and the silver and gold, are left in the mud, and the precious metals are extracted in the ordinary way. It is usual to employ cast anodes, and arrange the plates in each vat in parallel, the vats themselves being coupled in series. This arrangement is clumsy, and should be modified. I have had an opportunity of examining the large copper refinery at the Bridgeport Copper Works, and it may be well to describe the arrangement employed there. As the books were examined by Mr. J. H. Hays at the same time, I am also able to give you the actual cost of the process, without having too roseate a hue imparted by an enthusiastic inventor. The refinery is arranged and worked in accordance with the patents of Mr. E. S. Hayden. The departures from ordinary practice are made with the view of reducing the amount of copper under treatment, and of keeping down the cost of labour.

The anodes, instead of being thick cast copper, are sheets of thin rolled metal. The cost of rolling is small, and the plates used

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can thus be made thin, and dissolved very uniformly. Each vat contains a number of plates arranged vertically and across the vat. At one end a thin plate is supplied as a cathode. The first plate is made the anode, and copper is eaten off one side of each plate, and deposited on the plate next to it. The result is that each vat acts as a number of small vats arranged in series, and requires only two electrical connections. The whole question of large leads and connections is thus solved. There is almost no waste through partly consumed anodes, as the whole plate is eaten away evenly. The process is carried on until the whole of the anodes are eaten, and their place taken by the pure deposited copper. The plates are hosed and removed, and the slime is treated to recover the silver and gold. The product is the highest grade of copper in the market, and fetches £6 10s. a ton above "tough "cake," or £4 10s. above "best selected." Silver and gold are completely separated without loss, and as 35 oz. of silver and ½ oz. gold per ton are allowed as tare, any copper containing more than that yields £8 per ton in bullion. Argentiferous copper thus yields at least £14 10s., leaving the gross profit of £12 10s. at present rates. I have here taken the cost of refining at £2 per ton. The actual cost of refining copper depends on the output of the works and the cost of labour. In Bridgeport it came out just over £2 a ton. With modern dynamos, and reasonably good engines and labour at English prices, the cost would be a good deal less, say 20s. to 25s. per ton. The use of thin plates as anodes also reduces the capital investment in copper, which, as already pointed out, may be enormous, and thus lessens the margin to be allowed for fixed charges.

The Elmore process is too well known to require a full description. The copper is deposited on a revolving mandrel and burnished by a travelling agate burnisher. By this means copper tubes of great strength are made, and by cutting the tubes sheets are produced. Narrow strips can also be made, and these can be drawn down into high-grade copper wire. A number of thin sheets can be made by depositing several separate coatings one over the other on the mandrel. The sheets separate when the tube is slit lengthways.

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Writers of books on political economy are fond of saying that the chaffering of the market makes people manufacture in the cheapest way, and that the cheapest and best goods displace dearer and inferior qualities. They forget the interference of custom, ignorance, and prejudice. If a new process is brought out, it takes ages before it is taken up abroad, and still longer before Englishmen will touch it. Here we have a process of refining copper at a gross cost of £1 or £2 a ton, with a gross profit of £13 or so; and yet copper refiners continue their barbarous methods. The first manufacturers to take up copper refining by electrolysis on really modern principles have a chance of making fortunes before the market is disturbed.

Antimony.

Little has been done in the way of treating antimony ores electrolytically. Dr. W. Borchers proposes to use the solution of sulphide in sodium sulphide as an electrolyte, and to deal with the ores. The specimens of antimony ore that I have examined have been perfect insulators, so that regulus of antimony cannot be used as an anode. There will be an accumulation of sulphur in the bath, and it will probably insulate the anode. It is also difficult to say what material could be employed as anode in such a solution. Carbon would be attacked, and platinum would be too expensive, and other metals would be converted into sulphides. As the reduction of antimony, by iron, for example, is comparatively easy, there does not seem to be much opening for the electrolytic treatment in dealing with its ores.

Recovery of Tin from Scrap Tin.

This, of course, can be readily effected by electrolysis. The scraps can be made the anode in an alkaline solution, in which iron is not attacked. Though such a method removes all the tin from the surface, it may leave some alloyed with the iron, as no doubt some tin sinks into the iron during dipping.

Electrolysis has also been employed to assist in cleaning plates before dipping. I do not see how it can be of much service here, however. I have examined one of the processes, and it was said to

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be working very successfully, but I found the brushes had been accidentally left off the commutator.

Electrical Tanning.

Electrical tanning, though it depends on electrolysis, differs materially from all the processes already described in not depending on the isolation of radicles at the electrodes. Ordinary tanning is a very slow process, because it takes a long time for the tannin to diffuse into leather properly. If this substance is an electrolyte, it has a very complex molecule, and therefore diffuses very slowly. The outside of a thick piece of leather also becomes tanned, and to some extent protects the rest from the diffusion of more tannin. We may get an idea of what I suggest is the action in electrical tanning by regarding a hide as being made up of cells in groups, with interstices among the groups. If the skin is merely left in tan liquor, it takes months for the liquor to penetrate even into the interstices. It has then to diffuse through the walls of the cells. This is necessarily a very slow process, when it is remembered that the gelatine uses up the tannin as it comes in. Mechanical agitation has been tried, but it does not accelerate the process much. It is probable that agitation helps the liquor into the interstices, but of course it cannot help it into the insides of the cells, or, in fact, into those parts of the leather which are accessible by diffusion alone. The agitation can thus only perform the first part of the process—that is, getting the liquor into the pores that are open to it.

Passing electricity through the vats without agitation has also been tried, but it, again, is of little or no use by itself.

If, however, the skins are agitated and electricity is passed through the bath at the same time, the agitation circulates the liquid into the interstices, and the electric current sends the tannin through the walls of the cells, so that the leather is made infinitely faster than by the ordinary methods. Dr. Rideal and Mr. Trotter have made numerous and elaborate experiments on electrical tanning. The results of their work are given in the exhaustive paper read before the Society of Chemical Industry. I brought forward this as being probably the explanation during

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the discussion on their paper, and it agrees entirely with the results they obtained. The best results, according to this theory, would be secured by agitating the hides first to get the liquor into all the interstices uniformly, and then electrolysing to get the tannin into the skin itself; then agitating to refresh the solution in the pores, and then electrolysing, and so on. Messrs. Rideal and Trotter showed that this method does, in practice, give the best results. Leather made in this way also has a better chance of being uniform, because the outside is not acted on perceptibly quicker than the inside.

Tanning by agitation and electrolysis combined seems to have been invented by Messrs. Worms & Balé, and their process has been worked successfully for some time past in France, and in England by the British Tanning Co. Mr. Groth has developed a modification, and is also making leather by this process. Groth's appears rather better than ordinary leather, being more uniform in texture, and giving better results when tested for tensile strength. It is shown at the Crystal Palace Exhibition this year.

Ozone.

Now that induction coils in the shape of transformers have become commercial, there is every reason to expect that ozone will be made industrially. Mr. Fahrig has already been making ozone commercially in this country. He employs an alternate current which works a transformer which gives a high electromotive force. Air is first drawn through an apparatus which is supposed to separate the nitrogen by diffusion. As nitrogen and oxygen have nearly the same densities, it is hardly likely that much nitrogen is separated. The air is then led through the ozonisers. These consist of piles of corrugated metal plates with sheets of celluloid between them. They are alternately positive and negative, being connected to the high-pressure side of the transformer. There is thus an alternating discharge from the corrugated electrodes to the celluloid sheets, so that the air is ozonised on passing through.

Messrs. Siemens & Halske employ ozonisers of the more usual form. Two concentric glass tubes are used. One electrode

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is inside the inner, and the other is outside the outer tube. High-pressure transformers are of course employed, and the air, or oxygen, is partly converted into ozone on its way there. Very little is known about the electrolysis of gases, if such an expression is admissible. The so-called silent discharge seems to work best, but the whole subject now demands quantitive investigation. The use of glass must involve low efficiency, as glass possesses the property of dielectric hysteresis in a high degree. We also want to know the current-density and frequency which give the best results. The arc, and, in fact, many forms of electric discharge, produce traces of nitrogen compounds. It is worth pointing out that if under any commercial conditions nitrogen compounds can be produced from air, there would be an unlimited demand. A works situated on a waterfall would have its power and material provided, and the only cost of production would be the interest on plant, and the wages of a man or two to look after the dynamos.

It has been said that chlorine acted on by the electric discharge is affected in the same way as oxygen. In the first place, chlorine is already quite a strong enough oxidant; and in the second, it is difficult to see how chlorine can form an allotropic modification.

Miscellaneous Processes.

Under this head I have classed a few electrolytic processes whose theory is obscure, and about whose practical success there may be some doubt.

Mr. Webster has brought out an electrolytic sewage process. The effluent has electricity passed through it, and this is said to separate the solid material completely, and to kill all germs. Of course, when any sewage containing salt is electrolysed, it produces chlorine or hypochlorites, and these would act on organic matter. Hermite, for instance, has applied his process to the disinfection of ships. This does not seem to be the basis of Webster's process, however, and there seems uncertainty as to what the action is supposed to be, and why the solid particles should be influenced by the current.

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The next process is ageing wine and spirits by means of the electric current. Whether there is really anything in this I do not know. Various inventors have brought out methods of ageing liquors by electrolysis. There is uncertainty as to whether the result is produced, and as to how it is produced. Various deleterious alcohols might be oxidised into ethers, but it is strange that electricity should select just those that give a taste which is unpleasant to the human palate. Mr. Turell uses a direct, while M. de Meritens employs an alternating current.

The sterilisation of milk by electrolysis is another process which is somewhat obscure.

The Electric Furnace.

The idea of using the heat of the arc for producing high temperatures for such purposes as melting iridium is about half a century old. Recently, however, we have had the Cowles furnace, used especially for the reduction of alumina. Properly speaking, this does not come within the scope of this paper, as there is no reason to suppose that electrolysis has anything to do with the reduction of the oxide. By pouring the power from a large dynamo into a carbon resistance, closed in so that the heat cannot readily escape, a very high temperature is produced. Alumina is then reduced by the hot carbon. It may be taken that at lower temperatures, such as those produced by ordinary furnaces, the affinity of aluminium for oxygen is greater than that of carbon, and that at the higher temperature the reverse is the case. It is not impossible, however, that the carbon in an ordinary furnace is inactive merely because it cannot get into good contact with solid alumina, and that in the Cowles furnace the alumina melts. In any case, there is no reason to suppose there is any electrolytic action going on.

The Cowles furnace is applicable to all processes which demand very high temperatures, such as the reduction of oxides of aluminium, magnesium, calcium, barium, strontium, silicon, chromium, boron, tungsten, and manganese. Of course many of these metals are not at present in demand; but then, it must be remembered, that may be because there has, until now, been

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no way of making them reasonably cheap. Many, if not all, of these metals have been already obtained by the Cowles process.

It is a question of economy whether the Cowles furnace should not be employed in many other reduction processes which do not require such high temperatures. The furnace is efficient, as the heat is applied where it is wanted, and little is wasted by radiation or convection. We thus have to compare an ordinary furnace in which the heat is applied badly, and a great deal is wasted because the gases go off at a very high temperature, with a system which begins with an efficient furnace working a very inefficient heat engine and an efficient electrical machine. The electrical furnace will doubtless soon be applied in many cases, especially those which ordinarily involve inefficient furnaces, and retorts which break down quickly or leak.

The manufacture of phosphorus is a good example. On paper the process is simple. Bone ash, or tricalcic phosphate, is not reduced by carbon at ordinary furnace temperatures, so it is treated with sulphuric acid, and converted into monocalcic, or superphosphate. Making this involves making sulphuric acid, or buying it. Acid phosphate is then heated in retorts with carbon, and two-thirds of the phosphorus is obtainable theoretically. If sand is used it acts as a strong acid—or, rather, anhydride—at high temperatures, and all the phosphorus should be obtainable. The retorts, of course, give rise to trouble and expense. Messrs. Readman, Parker, & Robinson, by employing the electric furnace, can deal with mineral phosphates, and require no sulphuric acid. I do not know whether carbon at the temperature attained in the electric furnace will reduce calcium phosphate to calcium and phosphorus, or whether calcium phosphide is formed. With sand, of course, calcium silicate is formed. One might expect that this would be reduced to calcium and silicon, in which case it would be reasonable to suppose that the operation could be carried on without silica. It has been said that some oxides which can be reduced by very hot carbon when dealt with separately, can form compounds that cannot be reduced if one oxide is strongly acid in comparison with the other. If that is the case, though the lime and silica might be reducible separately, the combination of the

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two, calcium silicate, might be irreducible. Whether calcium silicate is reducible or not, silica must, of course, be employed if the phosphate would otherwise be converted into phosphide.

It would be well to see what other reduction processes could be dealt with economically by the Cowles furnace. Doubtless there are many, and Messrs. Parker & Robinson are sure to explore the field pretty thoroughly.

Conclusion.

A very few of the possible applications of electrolysis have been described in the foregoing paper, and the labours of many workers have been passed without mention. This was unavoidable. There is also another difficulty in writing such a paper as this: I have not worked at all the processes myself, and have had to rely on published accounts in many cases. Out of every hundred new inventions in electro-metallurgy, ninety are impossible chemically or electrically. If I have ruthlessly cut out a possible or admitted an impossible process, I must ask pardon on account of the impossibility of trying all the processes myself, even experimentally, and must plead that I have been as sceptical as possible.

There are other directions in which electrolysis may play an important part. The improvement of the manufacture of white lead deserves the most careful attention. The Dutch process is unhealthy, costly, and barbarous. The electrolytic manufacture of aniline dyes has received considerable attention. Dr. Goppelsroeder has done a good deal of work in this direction, and samples of silks dyed by electrolytically prepared colours were shown both in the Paris and the Frankfort electrical exhibitions. The extraction of gold from sea water, which is said to contain minute quantities, need hardly be treated seriously. Even if it were possible, such a process would be of no use. All improved processes for obtaining precious metals merely enrich some at the expense of others, and reduce the value of the metals. Obtaining gold more easily would be no good to the community unless it became cheap enough to be used commercially.

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THE EDITOR OF THE "JOURNAL OF THE INSTITUTION OF ELECTRICAL ENGINEERS."

SIR,

On page 461 of No. 99 of the *Journal* there occurs the following statement, which I made in the course of my remarks on the paper by Dr. Gladstone and Mr. Hibbert:—"The reason, then, why in the curves which accompanied our "papers of 1890 on the tests made on the E.P.S. cells no such "hump was seen, was probably because with the E.P.S. type "of cell you are only allowed to discharge with a rather small "current-density."

It would have been more accurate had I said "you were" instead of "you are," since my remark applied, of course, to the 1888 type of cell, which was the type we tested, and not to the K type of E.P.S. cell, which has since been brought out.

I may take this opportunity of mentioning that I have recently observed, not merely the hump which I stated on May 19th had been noticed in my laboratory as occurring in the early part of the charging curve of non-pasted accumulator plates, but also the hump which Mr. Crompton stated that he had observed in the early part of the discharging curve.

I am, &c.,

W. E. AYRTON.

July 13th, 1892.

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ABSTRACTS.

Dr. MEISSNER—THE LAUFFEN-HEILBRONN WORKS.

(Elektrotechnische Zeitschrift, Vol. 13, No. 15, p. 193.)

The generators are driven by the surplus water power of some cement works at Lauffen. There are two 300-H.P. vertical Geislingen turbines running at 35 revs., and each driving an Œrlikon Drehstrom machine giving, at 150 revs., 50 volts and 4,000 amperes. A third similar unit is to follow; only one unit and one set of leads are at present working. The armatures are fixed, and the field magnet revolves; the latter has one circuit and 32 pole-pieces. The current is transformed up to 5,000 volts by Œrlikon transformers; these are not run in oil, in order to allow of easier cooling. The line is of 6 mm. copper (No. 4 S.W.G.) on oil insulators, and about 6 miles long. Half-way to Heilbronn the line passes Sontheim, where there is a transformer station for 20,000 watts for local use. Here, as also at Heilbronn, there is a switch by which, in case of serious emergency, the line can be short-circuited, thus blowing the fuses at Lauffen, which immediately shuts down automatically. Pilot wires are run on the same poles as the leads, and by disconnecting these at the sub-stations they can be used for telephoning to Lauffen; the voltmeters falling to zero acting as the call. After passing Sontheim, the line continues to Heilbronn, where the current is transformed down to 100 volts, at which pressure the distributing network is fed. 1,100 16-C.P. glow lamps, 8 arcs, and 15 H.P. of motors are now running. The loss in the mains is 10 per cent. at full load.

E. BRYLINSKI—NOTE ON THE CAPACITY OF OVERHEAD LINES.

(Annales Telegraphiques, March, 1892, p. 97.)

M. Vaschy has published some figures on this subject (*Ann. Tel.*, November, 1892), in which he always found that K was larger than the value calculated from the length, &c., of the wire. He suggested that the discrepancy was due to the effect of neighbouring wires; but M. Brylinski now shows that this will not nearly account for it, and that the real cause is the K of the insulators. From measurements given this would appear to be of the order of 0·0001 microfarad each when dry, and 0·0004 when wet. The insulation resistance of the insulators being rather low, especially when wet, made the capacity tests somewhat difficult; the throw due to the capacity being complicated by the conduction current. The author investigates the question at some length, and gives the following results:—If D be the total throw obtained when the circuit is first made, and d the permanent deflection due to leakage, then, if d is small compared to D—say not greater than $\frac{1}{10}$ D—the throw due to the capacity alone is $\Delta = 1{\cdot}0272\,D - 0{\cdot}84\,d$; whereas, if d be large compared to D—*i.e.*, getting on for D/2—then $\Delta = \frac{1}{8}D - 1{\cdot}73\,d$. In a subsequent paper by

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M. Lagarde several further measurements of K of insulators are given, with dimensions of the insulators themselves; K varies from 0·00009 to 0·0002 microfarad each.

SIEMENS and HALSKE—COULOMBMETER.

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 22, *p.* 289.)

This is an intermittent integrating meter with clockwork motion. The principle is the same that has been used for some time by the makers in previous instruments; the novelty of the present one being in using a permanent magnet "A and P" type of ammeter. The clock actuates the recording mechanism every two and a half minutes, and requires winding once in five weeks.

— **MARCHER**—EXPLORING NEEDLE FOR MAGNETIC FIELDS.

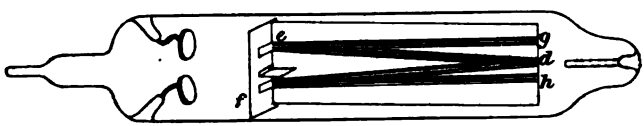
(*Lumière Electrique*, *Vol.* 44, *No.* 18, *p.* 236.)

A very thin steel tube is closed at each end with a thin iron disc, and magnetised; a glass tube has a bulb blown at the end rather larger in diameter than the length of the magnet, the bore of the tube being large enough to allow the magnet to be passed down it into the bulb. The bulb is then filled with liquid of such specific gravity that the steel tube will be in equilibrio, and the tube closed with plaster close down to the bulb in it. The magnet should be made as light as possible, and works very well when it is in equilibrio in liquid of 0·7 to 0·8 specific gravity.

WIEDEMANN and EBERT—THE APPARENT REPULSION BETWEEN TWO CATHODE RADIATION BEAMS.

(*Wiedemann's Annalen*, *Vol.* 46, *No.* 5, *p.* 158.)

Mr. Crookes has shown a tube with two cathodes at one end and the anode at the other. At the cathode end he had a mica screen with a small slit in it opposite each cathode. He also had a plate of mica covered with some phosphorescent substance lengthwise in the tube. If only one of the cathodes was used, the ray



passed to about the centre of the tube *d*; but if both were on, the two rays did not meet, but passed parallel to each other to *g* and *h*. This, the authors say, is not really due to repulsion. Over one of the openings, *f*, they hinged a small door, and found that, when both cathodes were joined up, the beam *e* passed to *g* just the same when the other ray was stopped by the door being closed. The authors consider, therefore, that the direction of radiation from a cathode is influenced by the presence of a second cathode in the neighbourhood; that the change is due to an actual directive effect at the cathode itself, not to repulsion between the two beams

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STRECKER and BILLIG—MEASUREMENTS OF COEFFICIENT OF SELF-INDUCTION OF VARIOUS TELEPHONES.

(*Elektrotechnische Zeitschrift, Vol.* 13, *No.* 21, *p.* 273.)

Measurements are given of 86 different receivers. The values vary considerably: the lowest is 0·007 secohms for one form of Siemens & Halske; others of the same makers averaging 0·15, Bell-Blake 0·064 and 0·12, Berliner 0·02 and 0·2, while some instruments run up to nearly 0·5 secohms.

W. KUNZ—THE EFFECT OF TEMPERATURE ON HYSTERESIS.

(*Elektrotechnische Zeitschrift, Vol.* 13, *No.* 19, *p.* 245.)

This is a preliminary publication of the results of some experiments. With equal maximum induction the hysteresis loss distinctly diminishes with increase of temperature. In a piece of soft iron the loss per cubic centimetre was 23,490 ergs when cold, 19,180 ergs at 530° C. (dull red), and 21,640 ergs when cold again. Steel showed a diminution of hysteresis of about 20 per cent. when raised to 100° C. The methods of measurement are not given.

G. GRASSI—COMPENSATED GALVANOMETER OF CONSTANT SENSITIVENESS.

(*N. Cim.* (3), 30, *p.* 120, 1891; *Beiblätter, Vol.* 16, *No.* 4, *p.* 227.)

In a tangent form of galvanometer, besides the ordinary vertical coil in the meridian, a second vertical coil in the plane at right angles to the first is traversed by the current in a direction such that it opposes the direction of the earth's force. If a_1 and a_2 are the distances of the centre of the needle from the peripheries of the two coils respectively, n_1 and n_2, r_1 and r_2 the number of turns and the radii of the coils, α the deflection, and H the horizontal component, then C is approximately $= (H\, r_1 \tan \alpha) \div 2\pi (1 \times A \tan \alpha)$, where $A = n_2\, r_1 \div n_1\, r_2$.

C. BOCCALI—COMPARISON OF VACUA OF GLOW LAMPS.

(*Elektrotechnische Zeitschrift, Vol.* 13, *No.* 19, *p.* 249.)

With constant supply of heat the temperature to which the filament of a lamp will rise depends, *cæteris paribus*, on the vacuum. The author uses a bridge method, with the lamp under test as one arm. The resistance of the lamp is first taken in an oil bath at 50°, then at 20°; care being taken in both these tests to use as small a battery current as possible—in the author's case 0·3 milliamperc. The current used in the bridge is then raised by an adjustable resistance in the battery circuit till, keeping the oil bath constant at 20°, the filament is shown, by its resistance, to be 50°. The current being measured, the watts required to keep the filament at 50° under these circumstances are known, and applying the same tests to other lamps of the same make, size, &c., a comparative idea of their

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respective vacua is got. Tests on seven similar lamps, 65 volts, 16 C.P., show the milliwatts required vary from 7·8 to as high as 14·8. The lower the watts, the better presumably the vacuum.

H. GEORGE—THE COMPARATIVE OUTPUT OF THE DIRECT, ALTERNATING, AND DREHSTROM ARMATURE.

(Paper read before the Elektrotechnische Verein, Berlin.)

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 18, *p.* 236.)

Dubois Reymond showed in 1891 that, in a ring excited by several multiphase coils, the intensity of magnetisation increased with the number of coils; the total winding being evenly distributed over the ring, and the number of turns and mean current-strength being kept constant. The author takes the reciprocal case, and shows that, given a rotary field in an iron ring, the E.M.F. set up in coils surrounding it, and therefore the work, increases with the number of coils. Taking the work, when the number of coils is infinity, as 100 per cent., that with two coils is 63·6 per cent., with four coils 90 per cent., and 12 coils 98·8 per cent. The same armature with the same winding and direct current would be 90 per cent. An ordinary Siemens & Halske "H" machine was experimented with; the pole-pieces bored somewhat oval, so as to get a strong field in the middle, weakening towards the side. The winding on the armature was arranged so that it could be coupled up to collector rings to give either direct current, or as a three- or six-coil Drehstrom. The ratio of the E.M.F.'s obtained with the three arrangements were within 2 per cent. of calculation.

Herr von Dobrowolski, in the discussion, rather objected to comparisons between the output of the same machine for direct and other currents; as a rule, a machine was built and was suitable for one purpose only. He might mention, however, a multipolar direct machine of the "Allgemeine" Company which gave 11,000 watts; the same field magnet with a Drehstrom armature gave an output of 30,000 watts.

GEROSA and FINZI—THE MAGNETISATION OF IRON IN AN ALTERNATING FIELD.

(*Rend. Lincei*, 7 (8), *p.* 253, 1891; *Beiblätter*, *Vol.* 16, *No.* 4, *p.* 229.)

The authors have previously shown that there is no turning point in the magnetisation curve of iron traversed by an alternating current, and at the same time placed in a magnetising spiral conveying alternate current. The maximum of magnetisation is also less. The present tests were made with a Bellati dynamometer, at one end of the needle of which was fixed a magnetising spiral (1,000 $\curvearrowright\curvearrowleft$ per min.) containing a bundle of hard-drawn iron wires; a bundle of the same wires annealed was also tested. At the other end of the needle of the dynamometer was a compensating spiral similar to the first, but without core; this was movable by a micrometer screw. The results showed no hysteresis for either hard or soft wires; the temporary moment of the annealed was the greater; and

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both approached the maximum of magnetisation much sooner, though the actual moments were less than with direct current.

E. COHN—ON THE GORDON-WINKELMANN METHOD OF MEASURING S.I.C.

(*Wiedemann's Annalen, Vol.* 46, *No.* 5, *p.* 135.)

In 1889 Wiedemann published a very simple method, and the results he obtained by it were in close agreement with those by other methods. Recently, however, Tschegläjew has published results obtained by the method which are in direct opposition to all previous ones: *e.g.*, for water he gives, instead of about 80, the figure 1·75, which is exactly the square of the index of refraction; and similarly with other liquids. These figures the author unhesitatingly condemns as untrue; and, in searching for the probable cause of error, he found that there was one great fault in the method. It is practically the Gordon method with two condenser plates removed, and a telephone replacing the electrometer, and the author finds that the results may be materially affected by the movements of the observer himself or other masses in the neighbourhood. The fault is due to the removal of the two outer plates in the Gordon method; if these are replaced, they act as screens, and prevent any disturbance.

ELECTRICAL TRANSMISSION OF POWER TO GENOA.

(*Elektrotechnische Zeitschrift, Vol.* 13, *No.* 17, *p.* 216.)

The principal water supply of Genoa is now obtained from a tributary of the Po on the frontier between Piedmont and Liguria. The stream has been dammed at a cost of £140,000, and a constant supply of 5,000 to 6,000 gallons per minute obtained. The reservoir is about 2,000 feet above sea level. 1,600 H.P., or about one-half the total power, is taken up for transmission by three stations on the mountain side. The first (450 H.P.) has been running since 1890. A second has been running night and day since November 3, 1891. In this there are six Thury machines 70 H.P. each, coupled in pairs to 140-H.P. Faesch-Picard turbines. There is a fourth similar unit, to take the place of any other while cleaning, &c. The machines work at 1,000 volts 47 amperes each, at 450 revs., and run all in series. It is intended later on to add another pair and be able to run up to 8,000 volts. The machines are all insulated on porcelain. To cut one out, the water is nearly turned off, and just before the dynamo stops running it is short-circuited; to cut it in again, the water is turned on till it gives the normal 47 amperes on short-circuit, the short-circuit opened again, and the water turned full on. An automatic regulator worked by the main current acts on the water supply of the exciters. Another regulator on each pair breaks their exciting current when their volts exceed 2,400, and short-circuits them. It is considered desirable to separately excite the exciters themselves, which is done from a small machine used also to light the works and driven by its own turbine. The line, about 16 miles long, is mostly of 0·34 in. bare copper on oil insulators. The motors

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at the Genoa end vary from 15 to 60 H.P., and are all in series. They are used both for power in mills and in a central lighting station.

Dr. v. WIETLISBACH—THE ACTION OF STRONG ON WEAK CURRENT CIRCUITS.

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 22, *p.* 285.)

The author considers that it should be made compulsory that strong current circuits should be so run as to have the minimum disturbing influence on neighbouring telephone and similar wires; although, on the other hand, it is almost as necessary that telephone wires should be run double and, when overhead, on the twist. A simple direct-current circuit can easily be made innocuous to a double telephone circuit by running the wires fairly close together; a three-wire system is a little more difficult, but the only really troublesome direct circuit is a tramway with rail return: this even is not so very bad if the rails are all joined with copper straps, or, preferably, all joined to a common return lead. The Sissach-Gelterkinden line (2½ miles), on which this precaution is taken, is found to have no effect on the telephones in the neighbourhood. Alternating circuits cause more trouble, but can generally be made fairly harmless. A simple double line will not affect a neighbouring telephone circuit unless the two circuits run alongside one another for several miles; if, however, a transformer is introduced in the middle, it increases the disturbance enormously. The most troublesome of all is a Drehstrom circuit. When the motor is running light, in unison with the dynamo, there is practically no disturbance. As the load is slowly put on beats are heard in the telephone, which get more and more frequent as the load increases, till they make conversation impossible. Some experiments were made on the Kessel-Zurich power line, 5,000 volts, working at 4 amperes. This was found to disturb a double-wire telephone trunk line which crossed it to a very great extent; and probably, when the current is raised to 40 amperes, as is intended, the only way will be to run the telephone wires underground in an iron tube for some distance each side of the cross. Calling "6" the very greatest disturbance under which conversation could be kept up, the author gives the following comparative effects:—One, two, and three crossings with a single telephone circuit, 3, 4½, and 5½ respectively; with a two-wire telephone circuit. 1, 3, and 4½. Running parallel to the Drehstrom circuit for 120, 380, and 1,000 metres gave disturbances of 1, 2½, and 5½ respectively. The distance between the circuits being 25 metres. These are with the 4-ampere current as mentioned; but the circuit is a particularly bad one, as, owing to the way that the power is taken off close to the crossing point, the currents in the three wires are very unequal.

DOLIVO-DOBROWOLSKY—THE EFFICIENCY OF TRANSFORMERS.

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 17, *p.* 222.)

After treating the subject generally, somewhat on the lines of Mr. Swinburne, the author gives figures and diagrams of the Allgemeine Elektricitäts Gesellschaft

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3,500 watts, 40 ∿ transformer, 1,000 volts to 100 volts; cross section of iron, 42 cm.²; primary, 3,600 turns of 2·3 mm. diameter (13 S.W.G.), R = 9 ω; secondary, 360 turns, 5 by 8 mm. (= say 0·6 square inch), R = 0·064 ω. Iron, 50 kilos.; copper, 120 kilos.; copper loss at full load, 5·3 per cent., exciting on open circuit 50 watts. The curve of total watt losses with increase of load is given, as compared with that of a transformer built on the old lines of 2 per cent. copper loss and 4 per cent. magnetising current. The all-day efficiencies of the two in winter are given as 91 per cent. and 84 per cent. for the new and old types respectively, and in summer as 83·8 per cent. and 65·8 per cent. Similar particulars are also given of a 20-kilowatt transformer, and reference made to a Drehstrom transformer built on the same lines. In the discussion, Herr Görges said that the cost of the power must be taken into account: if this is high, the magnetism must be kept low; if water power of nominal value is employed, then the magnetisation should be run up as high as the heating of the transformers will allow. Herr Imhof mentioned that Messrs. Ganz had built high-copper-loss transformers for some years.

F. ROSS—CENTRAL STATIONS.

(Paper before the El. Tech. Verein, Berlin.)

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 19, *p.* 254.)

The author deals with the cost only, which he divides into "Plant," including engines and dynamos, batteries if used, and all other station plant, but not including land; "Network," including mains, junction boxes, and transformers if used; and, thirdly, "Running Cost." He gives the following figures:—

50-Watt Lamps.		Cost per Lamp.	
		Plant.	Network.
		£ s. d.	£ s. d.
Dusseldorf ...	13,000	3 2 0	3 3 6
Cologne	20,000	2 17 0	1 11 0
Berlin	100,000	2 0 0	(?) 5 0 0

The first column is the maximum output of the station.

The cost of network per lamp in the first two towns is taken from capacity of the mains, not from that of the central station.

The running cost is estimated at 1·5d. per 50-watt lamp-hour, including management charges 0·5d., and upkeep 0·31d. The costs of alternating- and direct-current working the author considers much alike. As to density of supply, it is probably optimistic to reckon on one lamp for every 4 yards of street front, taking the average of a whole town. In the densely populated parts the demand is, of course, much greater. The total yearly output may be taken as 1,000 hours at maximum output. The cost of electric power he takes at 4d. per H.P.H. In the

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discussion, Herr Dihlman objected to this last figure,—he would put it at half. The power would not be on simultaneously with the light, and would therefore be supplied without increase of plant.

DOLIVO-DOBROWOLSKI—OVERCOMING INDUCTION OF TELEPHONE WIRES.

(Paper before the El. Tech. Verein, Berlin.)

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 15, *p.* 211.)

Two methods of overcoming the effect of a neighbouring strong current circuit on a simple telephone wire were mentioned. The first was by introducing a transformer into the telephone wire somewhere about the middle of the part disturbed. The second was to have a coil in circuit somewhere in the telephone line, and at the same point a coil in the disturbing line; the winding of the two coils being such that the mutual action between the coils was opposite in direction to that between the rest of the circuits. The two coils could then be adjusted at such a distance from each other that the effect between them just neutralised the effect between the wires. A working model was shown. In the discussion, many Post Office telephone officials took part. The first plan was shown to have been tried between Berlin and Potsdam in 1884, but without much benefit; and with regard to the second there was a general consensus of opinion that it would not be practical.

W. CHRISTIANI—THE THEORY OF INDUCTIONLESS OVERHEAD WIRING WITHOUT TWIST.

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 22, *p.* 283.)

Each line consists of two wires near together. The top line is on the right hand of the post, and one of its wires higher than the other, so that the plane passing through the two is at 45° to the post. The next line is on the left of the post, and lower down; its two wires are in the plane at right angles to that of the first pair, and passing between them—*i.e.*, draw a perpendicular from the middle of a line joining the first two wires and it will cut both the wires of the second line. The third line is similarly run with respect to the second, and so on alternately on either side of the post. The whole is treated mathematically at considerable length on the supposition of perfect wiring.

C. P. STEINMETZ—REMARKS ON THE MAGNETIC CIRCUIT.

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 16, *p.* 203.)

Fröhlich's hyperbolic formula connecting B and H is now obsolete. Kapp's tangent formula implies a final state of absolute saturation: of this there is no trace in the curves of Prof. Ewing, who has worked up to as high as 45,000 lines per cm.^{2}; nor does it express the fact that the reluctance with very small values of H is very high, diminishing as H increases, and then increasing again. Dr.

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Corsepius* gives a table of observed values of B, the total induction in a piece of iron with various values of H, the magnetising force; deducting H from B, he obtains E, which he calls the "iron induction." E increases with H for a time, but then becomes constant; a change of H from 491 to 4,910 only increasing E from 19,626 to 19,644. The circuit, in fact, behaves as if it were made up of two parts joined in parallel; the one of which is of constant permeability for all values of H, while the other becomes absolutely saturated at comparatively low values of H. For all values of H greater than 7, the curve of variation of reluctance $\left(\frac{H}{E}\right)$ with H is a straight line. The whole curve is expressed by the formula, $\frac{H}{E} = a\,e^{-bH} + 0{\cdot}6 + 0{\cdot}0508\,H$. $b = 0{\cdot}82$; a is given by Herr Steinmetz as 6·85, but by Dr. Corsepius himself as 3·75 (in a subsequent communication, E.T.Z., No. 19, p. 243); $a\,e^{-bH}$ becomes negligible when H is greater than 7.

* Dr. Max Corsepius, "Theoretische und Practische Untersuchungen zur Konstruktion "Magnetischer Maschinen." Berlin, 1891.

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LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Journals during the Months of MAY and JUNE, 1892.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

G. RICHARD—Recent Arc Lamps (S. I.). (Boardman & Fairfax, Crompton, Essinger, Gwynne, Sautter-Harlé, Dobbie, Weeks, Patin, Spoke, Bishop, Hayes, Waterhouse; Soc. Indust. Elect., Geneva; Mosher, Sawyer, Rushmore, and Pfluger.)—*Lum. El.*, vol. 44, No. 25, p. 550.

ANON.—Bremen Tramway (Overhead) (I.).—*E. T. Z.*, vol. 13, No. 24, p. 322.

MAGNETISM.

G. C. GEROSA—Intensity of Magnetisation of Iron Magnetised Simultaneously by the Primary and Secondary Current of a Transformer.—*Rend. Lomb.* (2), 24, p. 1, 1891; *Beibl.*, vol. 16, No. 5, p. 305.

TELEGRAPHS AND TELEPHONES.

— VAN HOEVENBERG—Synchronising Telegraph (I.).—*Lum. El.*, vol. 44, No. 23 p. 482.

F. VESPER—On Artificial Cables. (Paper, E. T. Verein.)—*E. T. Z.*, vol. 13, No. 23, p. 313.

P. GEISSLER—Multiplex Switch-Board with Automatic Break. (Paper, E. T. Verein.)—*E. T. Z.*, vol. 13, No. 26, p. 349.

— GRISSINGER—Telephone Receiver (I.).—*Lum. El.*, vol. 44, No. 23, p. 479.

A. FRANKE—Electrical Phenomena in Telephone Wires and Apparatus.—*E. T. Z.*, vol. 13, No. 23, p. 295.

H. CORDS—Ship's Telegraph.—*Lum. El.*, vol. 44, No. 24, p. 524.

ELECTRO-CHEMISTRY AND METALLURGY.

E. F. HERROUN—The E.M.F. of Gold and Platinum Cells.—*Phil. Mag.*, vol. 33 No. 205, p. 516.

G. RICHARD—Aluminium (S.).—*Lum. El.*, vol. 44, No. 24, p. 507.

INSTRUMENTS AND MEASUREMENTS.

LUMMER and KURLBAUM—Investigations with the Bolometer.—*Wied. Ann* vol. 46, No. 2, p. 204.

A. WEINHOLD—Drehstrom Lecture Apparatus.—*E. T. Z.*, vol. 13, No. 23, p. 300

— MILTHALER—The Use of Manganin for Resistance Coils.—*Wied. Ann.* vol. 46 No. 2, p. 297.

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H. Abraham—New Determination of "v" ($2{\cdot}992 \times 10^{10}$).—*C. R.*, vol. 114, No. 23, p. 1355.

H. Passavant—On a Reproduction of the Siemens Hg Unit.—*Wied. Ann.*, vol. 46, No. 2, p. 336.

Trouton and Lilly—Measurement of S.I.C.—*Phil. Mag.*, vol. 33, No. 205, p. 529.

J. F. Weyde—Specific Resistance of Lead Peroxide ($0{\cdot}14 \omega$ to $0{\cdot}45 \omega$ per cm.^{3}). —*E. T. Z.*, vol. 13, No. 24, p. 315.

I. Klemencic—Measurement of Inductance.—*Wied. Ann.*, vol. 46, No. 2, p. 315.

STATIC ELECTRICITY.

E. Trutat—Photography of Lightning (I.).—*Lum. El.*, vol. 44, No. 24, p. 527.

D. Korda—Ozone Apparatus (I.)—*Lum. El.*, vol. 44, No. 25, p. 583.

F. v. Lepel—Oxidation of Nitrogen by Sparks.—*Wied. Ann.*, vol. 46, No. 2, p. 319.

C. Ludekin—The Action of the Electric Discharge on Gases and Vapours.—*Phil. Mag.*, vol. 33, No. 205, p. 521.

A. v. Obermayer—Discharge from Points in various Gases at various Pressures.—*Beibl.*, vol. 16, No. 5, p. 306; *Wien. Ber.*, p. 127, 1891.

THEORY.

H. Poincarré—The Propagation of Electrical Oscillations.—*C. R.*, vol. 114, No. 22, p. 1229; *Lum. El.*, vol. 44, No. 25, p. 590.

A. Toepler—Contribution to the Knowledge of Oscillations of very Short Period.—*Wied. Ann.*, vol. 46, No. 2, p. 306.

A. Roiti—Displacement of Nodes by Addition of Capacity.—*Atti Lincei*, April, 1892; *Lum. El.*, vol. 44, No. 25, p. 626.

— Vaschy—As to the Possibility of Mutual Reaction between an Electrified Body and a Magnet.—*C. R.*, vol. 114, No. 25, p. 1474.

E. Bouty—The Co-existence of S.I.C. and Conductivity.—*C. R.*, vol. 114, No. 24, p. 1421; *Lum. El.*, vol. 44, No. 24, p. 535.

Elster and Geitel—Photo-electric Investigations.—*Wied. Ann.*, vol. 46, No. 2, p. 281.

A. Oberbeck—The Behaviour of Allotropic Silver with respect to Electric Current.—*Wied. Ann.*, vol. 46, No. 2, p. 265.

G. Darrieus—Theory of the Accumulator—*Bull. Soc. Int.*, vol. 9, No. 88, p. 205; *Lum. El.*, vol. 44, No. 24, p. 513.

RAILWAY WORK.

E. de Baillehache—Automatic (Train Contact) Signalling, &c., Apparatus (S. I.).—*Lum. El.*, vol. 44, No. 23, p. 465, No. 24, p. 516.

— Fricke—Train Alarm for Level Crossings (I.)—*Lum. El.*, vol. 44, No 25, p. 571.

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JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXI. 1892. No. 101.

The Two Hundred and Forty-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 10th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 26th, 1892, were read and approved.

The PRESIDENT: I am sorry to say I have again to open the meeting by announcing a death—in this case of our old friend Mr. Graves, who was, as you know, our Honorary Treasurer for many years, and also a few years ago was President. I will call upon General Webber, who has known Mr. Graves much longer than I have, to propose a motion, and afterwards upon Mr. Spagnoletti, also an old friend, to second the motion.

Major-General WEBBER: Sir and gentlemen,—It is my painful duty to move—"That this meeting desires to record its deep sense "of the loss sustained by the Institution in the death of Edward "Graves, Esq., Past-President and Hon. Treasurer; and further

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"desires to express its deep sympathy with Mrs. Graves and other "members of the family in their bereavement."

Sir,—As a brother officer of Mr. Graves in the Postal Telegraph Department for upwards of nine years, and in the absence of any other representative of that department who also has had the honour of filling the same chair as you, Sir, now do, as President of this Institution, I have been called upon to say a few words in memory of one whom we all esteemed, and who amongst his own friends was regarded in a way in which only our present sorrow can sufficiently express. Mr. Spagnoletti probably knew him better than I did—that is to say, previous to the year 1870, when the transfer of the telegraphs was made to the State; but during the nine years when I served as Mr. Graves's brother officer as a divisional engineer, and at the end under him when he was chief engineer, I can remember the frequent times upon which we had to work together, occasionally night and day, and I can truly say I never met anyone whom, for his modest simplicity, whom, for his genuine kindness and loyalty as a friend, I have more reason to regret. If Mr. Preece had been here to-night, it would have been his sad duty to speak as probably his oldest friend and his oldest colleague. In his absence, I will only remark that Mr. Graves was, as many know, Engineer-in-Chief to the Postal Telegraph Department, having succeeded Mr. Culley in the year 1878. From the day on which he first filled that position at the Post Office I believe everyone who served under him regarded him not only as a superior, but also as a friend. I feel that there has hardly been an occasion upon which anyone could have been called upon to address this Institution on the death of a Past-President when the regrets I now attempt to express would be more cordially joined in, especially by those old servants of the Post Office, whether telegraph inspectors, linesmen, or those in other capacities. I feel sure there is not one who does not this day look back with pleasure to their period of service under Mr. Graves. Mr. Spagnoletti, who knew him before 1870, and was aware that he was interested in telegraphy as early as in 1851, when he commenced his career at York, will probably speak as to the earlier period of Mr. Graves's career in seconding this motion;

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and I am sure the result will be that the motion will meet with unanimous support.*

Mr. C. E. SPAGNOLETTI: I rise with a very great deal of sorrow indeed to second the proposition of Major-General Webber. I endorse every word he has said in praise of Mr. Graves. The Post Office authorities, I am sure, will lose a most valuable officer; his colleagues, like myself, will lose an esteemed and valued friend; and this Institution will lose a devoted member. I have had the pleasure of serving with him on the Finance Committee

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* Major-General Webber has requested the insertion of the following note to the above:—

"My first acquaintance with Mr Edward Graves was in May, 1870, when he "was stationed at Birmingham.

"In command of the 22nd Company of Royal Engineers, with Lieutenants "Ramsay and Jekyll as subalterns, and Mr. Ross and Mr. Morrison as staff "sergeants, I had already joined the service of the newly created Postal "Telegraph Department, and our first work was to renew the old (United "Kingdom Company's) road line between Uxbridge and Oxford, which was at the "time in Mr. Graves's charge. The line, in view of the purchase by the "Government, had been allowed to fall into a condition of considerable dilapida- "tion, and in consequence the work had to be carried out with the greatest care, "all the eight or nine wires being in full use; and, as engineer responsible for its "efficiency, Mr. Graves frequently visited me; and then commenced a friendship "which lasted till his death.

"It was on the occasion of one of these visits that the idea of forming a Society "of Telegraph Engineers was conceived. It was on a broiling afternoon of June, "and the men had knocked off work for an hour, and Mr. Graves and I were "sitting in the shade of a tree; and having given him an account of a meeting of "the Association of Gas Managers and Engineers, which I had been attending, I "suggested to him that a somewhat similar society was wanted to bring telegraph "engineers together.

"Having in the meantime talked the idea over with the late Sir Francis (then "Major) Bolton, in September of the same year I brought the proposal before "Mr. Culley and a meeting of the divisional engineers of the Postal Telegraphs. "Nothing, however, came of this, and, indeed, Mr. Graves himself did not join the "Society before 1873; but the work of founding the Society was gradually carried "on by Major Bolton and myself, first assisted by the late Mr. R. Sabine, and, I "think, Mr. Whitehouse, and later on with the encouragement and cordial assistance "of Mr. Latimer Clark and the late Sir William Siemens, until in the early spring of "1871, after frequent meetings, the scheme assumed a concrete form, and I under- "took the duties of Honorary Treasurer, which I carried on until 1879, when "Mr. Graves, ever ready to come to the assistance of a friend, took them over "without hesitation (when I was ordered at a moment's notice to Zululand). The "efficiency of the conduct of this work of Treasurer, which Mr. Graves carried on "up to his death, we are all fully conscious of."

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for many years, and I can say that no lack of care or want of attention was at any time visible in the exercise by him of his duties in looking after the interests of this Institution. I have, as Major-General Webber says, known him and worked with him for very many years; and no one could do so without feeling the highest esteem and respect for him. He was an exceedingly good man, and most kind-hearted; he took a very high standard in duty and integrity, and I am sure he was a pattern for many of us to follow. I quite agree with all that Major-General Webber has said, and am very pleased that the Institution will record its deepest sympathy with Mrs. Graves, who has lost a kind, considerate, and affectionate husband.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Donations to the Library were announced as having been received since the last meeting from the Astronomer Royal; Messrs. Blackburn & Stevens; Messrs. Crosby, Lockwood, & Co.; Mr. A. Harleben; Mr. O. Heaviside; the India-Rubber, Gutta-Percha, and Telegraph Works Co.; the Minister of Telegraphs, Rome; Mr. H. Wilde; Mr. J. W. Wilson, jun.; Dr. Werner von Siemens, Professor A. Stoletow, and Professor K. Zetzsche, Foreign Members; Mr. John Aylmer, Dr. J. A. Fleming, Mr. S. H. C. Hutchinson, Mr. Gisbert Kapp, Mr. A. F. Matveieff, and Mr. A. Reckenzaun, Members; Mr. J. T. Niblett and Mr. J. Poole, Associates. The thanks of the meeting were unanimously accorded to the several donors.

The Secretary announced that, in addition to the donations to the Library, the Institution had received from Lord Armstrong a set of 23 photographs of dust figures obtained from his multiple induction machine; from Mr. Alexander Siemens, a set of six photographs of the electric discharge through air and over glass produced with the high-tension alternating current as shown by his firm at the Crystal Palace Exhibition; from Mr. W. O. Smith, a large framed photographic portrait of the late Mr. Willoughby Smith, a former President of the Institution; and from the

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publishers, Messrs. Barrauds, a large framed photograph of the members of the Institution.

The Secretary read a letter from a committee representing the American Society of Civil Engineers, the American Society of Mechanical Engineers, the American Institution of Mining Engineers, and the American Institute of Electrical Engineers, offering a hearty welcome to members of the Institution who might attend the Columbian Exposition to be held in Chicago next year, and stating that the home of the several societies named would be open to such visitors as were duly accredited through the secretaries of their respective associations.

The letter was received with acclamation.

The PRESIDENT: We will now commence the discussion on Mr. Swinburne's paper on "The Problems of Commercial "Electrolysis."

Mr. C. F. COOPER: Having read Mr. Swinburne's paper with much interest, I think that one ought to first suggest a vote of thanks to Mr. Swinburne for bringing up the subject, and I think it is a paper that is very likely to afford a very interesting discussion. Having been for some years connected with the Hermite process, and as I believe the Hermite process is one of the few electro-chemical processes which has been a success, I thought it possibly might interest you to have some particulars from a practical point of view. I do not propose, there being other gentlemen present more competent to speak on the subject, to go into the chemical question at all. I propose, as briefly as I can, to give you a short description of the way the Hermite process is worked, and to state that it is a going concern, and a great success on the Continent. Possibly it would be interesting to follow the development, I will not say from the beginning, but from the Antwerp Exhibition. Possibly many gentlemen present will remember a little châlet in the grounds of the Antwerp Exhibition in which Mr. Hermite's electrolytic process of bleaching was shown at work. Mr. Hermite at that time was bleaching linen yarns. The apparatus then in use was a source of considerable trouble—I think that was about the year 1885—and Mr. Cooper

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Mr. Cooper. it was found, principally, I think, from the experience gained at the Exhibition, that the apparatus was not a practical working apparatus. After that, Mr. Hermite and myself designed various arrangements—various electrolysers—making various types, with the view of getting at the most satisfactory. The diagram on the screen, I think, gives a fairly good representation of one we selected, and which is now generally in use. That electrolyser consists of a galvanised cast-iron box and a number of electrodes. The diagram shows one of the platinums, or one of the positive plates. We have experimented with pretty well everything, I think, that one can imagine, in the endeavour to get an unattackable anode, and we fell back at last on platinum, with which Mr. Hermite had started. Of course it was necessary to reduce the weight of platinum as much as possible for commercial reasons, and we originally started with very thin foil. We found that foil had many disadvantages: it had a knack of expanding and short-circuiting, and generally behaving like the secondary battery of old days; and we found also that it cracked, and we got an unequal distribution of current. In course of time we arrived at making the anode of platinum gauze. We found that, practically, our results were the same with the same weight of platinum as if we had a solid sheet. We mounted these gauzes in ebonite frames. I think that possibly No. 4 will show fairly well what it was. The blue is intended to represent the platinum gauze, and the black is intended to represent ebonite. The top piece, which is bent down, is lead. We cast the platinum into a lead frame, and then fixed them in the ebonite, and then ran in bitumen on the top; and this construction has proved very satisfactory. Mr. Swinburne mentioned, I think, in his paper, the question of attacking the anode. We have found in practice that there is practically no attack on the platinum anode. We have had the process in use for, I think I am right in saying, six years at Messrs. Montgolfier's mill at La Haye Decartes, and I think that out of 10 electrolysers we have certainly not put in one set of anodes in the whole six years' working. I think that fairly disposes of the question as to whether plati-

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num is eaten away. I am not speaking of the theoretical point, but merely of what we found in practice. I was contemplating writing a paper on this subject in conjunction with Mr. Esson; in fact, I think Mr. Esson did write a considerable portion of it; but I regret to say it has never been delivered. There is really such a lot to say that I don't know how to say it. I think possibly I may be taking up too much time if I go into it very thoroughly, but I have had these few diagrams got out to give you a rough idea of the thing. The gentleman who takes a glass pot and puts two electrodes into a solution of sodium chloride and decomposes that sodium chloride, generally talks about it as if it were a simple affair. So it is; but it is not at all simple when you get into a paper mill, where you have varying conditions every five minutes: the pulp is of all sorts—let us say esparto, wood pulp, soda sulphate, possibly all at once; you can never tell from one minute to another what state your bleaching solution is in. I cannot conceive that it is possible to observe the chemical conditions of that solution for more than half an hour together. The Hermite process is, as probably everybody knows, based on the decomposition of magnesium chloride. The process is essentially a continuous one. The solution of magnesium chloride in the top tank runs down to the bottom tank. We use a thousand amperes of 5 volts for each electrolyser; we occasionally run them as many as eight in series, but we prefer five in practice. The decomposition takes place in the electrolyser, and runs down into the poacher; it is washed through the ordinary washing drum one sees in a paper mill: the circulation goes on continuously in that way. When the pulp is bleached, of course there is a certain amount of solution left in the pulp that is to be treated and got back again, because one of the principal points in the Hermite process is that we use the same solution over and over again. It is run down from the poacher either into a drainer or over a press pate. A press pate will take out, let us say, about 40 per cent. of moisture; and it is necessary to pass the pulp which is to be dealt with over a press pate in the first instance, and then after bleaching to pass it over another press pate, so as to keep the same amount of solution in

Mr. Cooper.

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Mr. Cooper. circulation. I don't think I must trouble you with any more, as I hope some day to be able to give Mr. Webb a paper on the subject. I need hardly say that this arrangement of electrolyser was arrived at after a very large number of others had been tried, and we found that the present form, owing to the ease with which it could be taken to pieces and cleaned, and so on, was practically the best thing we have got out. I should be very glad to hear anybody else's experience of dealing with anodes other than platinum. Mr. Hermite has never been able, although he has conducted an enormous number of experiments with that object, to get anything that is satisfactory. In dealing with carbon, we have used various sorts years ago: we have always found that it disintegrated on the water line; that, irrespective of the carbon coming out, it is apt to disintegrate, and you get small specks of black in your pulp, which renders it entirely useless. We have tried a great many experiments with it, but always came to that difficulty, that instead of a bleaching solution we have practically a dyeing solution. We have had the same difficulty again in dealing with the pipes and conduits which convey our solution from one part of the mill to the other. I used to think it rather hard lines, some years ago, that in using the process we dyed many tons of our pulp a beautiful green. That was due to the copper in the washing drum. We had to take all the copper away and put on zinc gauze. And then, again, we used occasionally to dye a very nice rich red, from the fact of peroxide being present; and the difficulties were considerable, and they were accentuated by the fact that this is a continuous process, and in starting a mill one has to sit up day and night until the men get used to it.

Mr. Falkenstein. Mr. CONRAD K. FALKENSTEIN: Mr. Swinburne stated in the reference which he made to electric tanning that "if the skin is "merely left in tan liquor, it takes months for the liquor to "penetrate even into the interstices." From this it would appear that the interstices, or pores, between the fibres of the hide were very close and narrow indeed. But, in the preparatory process of liming, the cement substance which joins the fibres together is dissolved away by means of the lime, which also "plumps," or

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swells up, the hide and separates the fibres, thus leaving it fairly porous for the diffusion and absorption of the tan liquor. When the hide goes into the tan, the tannage at first progresses very rapidly, for this reason, viz.—that the pores are filled with water and organic salts, which diffuse out very quickly, thus creating a partial vacuum and tending to draw the tan liquor in. The rate of absorption grows slower afterwards, because these bodies are no longer present in such large quantities, and tannin, being a colloid body, has a very slow rate of diffusion by itself. Mr. Falkenstein.

If, now, a current of electricity be passed through the hides, it impels the tan liquor in the same direction as the lines of current-flow, a greater quantity of tannic acid solution being thus brought into contact with the gelatinous fibres of the hide. In tannic acid solution this endosmose effect is pretty considerable, being due, no doubt, to the molecular structure of tannic acid; and from the fact that it has a high resistance, comparatively high differences of potential can be employed to produce this endosmotic flow without necessitating the use of a heavy current, and therefore a large expenditure of energy, in the process. The fact observed by Messrs. Rideal and Trotter last year—viz., that after the current has been stopped the rate of absorption continues at an increased value—is due, no doubt, to the fact that the pores have been cleansed and opened by the endosmotic flow of liquor through them.

The above accounts for increased diffusion; but there is another point which may be noticed, and that is, that electricity is of the nature of a vibration, and therefore by vibrating the molecules of the liquid it tends to accelerate, or help, the combination of the tannic acid with the gelatinous fibres.

Mechanical agitation, too, has been commented upon in Mr. Swinburne's paper. This, I think, has two functions—first, to mix the liquor and render it of uniform strength; and, second, to bend the hide and open the pores on the outer side of the bend, thereby also assisting diffusion.

Mr. W. Stepney Rawson: Without being in the position either of Mr. Cooper or Mr. Falkenstein, of being able to draw attention to a particular process, as I am not now personally Mr. Rawson.

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Mr. Rawson. engaged on one, like those two gentlemen, I may perhaps say a word with regard to one or two points in Mr. Swinburne's paper, with some of which there is not much difficulty in agreeing, whilst with others perhaps it is not quite so easy to agree. Mr. Swinburne puts the cost of the kilowatt-hour at about a farthing, and he says it will appear ridiculous in connection with central stations. I think that, unless he has some figures arising out of his visit to Bridgeport to confirm the farthing per hour, it will appear, not ridiculous, but very small, to those who are engaged in commercial electrolysis; but at the same time he does stipulate for the use of the best engines and modern dynamos. The modern dynamo—we know pretty nearly what that is capable of; but we do not quite know what the modern engine is capable of. An exceedingly good authority told me once that in a succession of tests he made on various engines working commercially, the efficiency ran anywhere between 28 lbs. and about 4 lbs. of coal per H.P., so that there is a certain margin! I know that people are very fond of putting down on paper 2½, and in France a kilo corresponds so nearly with that, that they always call it a kilo, but it is almost impossible to get it in practice. Then Mr. Swinburne very wisely makes his estimate at the dynamo terminals. I say wisely, because in actual working one of the greatest enemies is leakage. There is no doubt that, theoretically, it is easy to get rid of it. There are a thousand ways in which leakage takes place which will surprise even an old hand. Many cases within my experience have occurred where it has puzzled even old hands to find out what is going on. Moreover, another great difficulty is the large number of parasitical currents set up, and you may hunt round for weeks before you find out where they arise. Therefore, although at the dynamo terminals you may arrive at a farthing per kilowatt-hour, I think by the time the current has, so to speak, got back again, you will find it averages a great deal more. With regard to the use of carbon, that, no doubt, is a source of difficulty, and many people have had troubles with it. I remember having got hold of a very good sort of carbon that I thought would do well for the purpose, but instead of that, I was surprised at

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the result. With a very heavy current-density it behaved tolerably well—that was, a current-density of 100 amperes per square foot. When I got down to 10 it disintegrated very much faster. The fact which Mr. Swinburne has been wise to put before us very clearly is that a new process must be marvellously good on paper before it is worth anything commercially, and that it is of no use to exploit it as a commercial process unless you realise this fact to start with. Regarding zinc and lead, Mr. Swinburne mentioned the difficulty of treeing in the deposition of both these metals. The arrangement of burnisher which the Elmores use has proved very successful in removing this difficulty. I have seen a solution of zinc deposited from a sulphate solution with a porous partition come out very definitely indeed—not, of course, as well as copper—and you can see the attempt to tree, but you can also see the success of the burnisher in keeping it down; and I rather think that the deposition of zinc from the sulphate solution with the burnisher working will turn out a fairly good process, because there is a very considerable commercial margin in that work. In the same way the treeing of lead can be fairly easily kept down. I was very pleased to see the figures quoted with regard to the method of Mr. Hayden, because his process is a remarkably beautiful one, and there is no doubt that it must leave a very considerable margin, with the extra simplicity brought about by the clever arrangement which he has introduced; and it gives Mr. Swinburne the opportunity of giving us one of his characteristic remarks, commencing with the words, "Writers of books on political economy," in which he speaks of the prejudice of Englishmen to touch anything new. With regard to the electric furnace, I thought that possibly it might be interesting to mention, although it does not come under the heading of electrolysis exactly, the electric furnace of Maxwell, in which he produces his heat simply by the resistance of carbon in a closed vessel exhausted to a tolerably high vacuum, through which he passes a current of something which will not act upon the matter to be melted, and he attains very comfortably the melting point of platinum, and has got some remarkable results with regard to products

Mr. Rawson.

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Mr. Rawson. from the fact that he is working nearly in vacuo, or, at any rate, in an attenuated gas which will not combine. I would rather not say anything with regard to the Elmore process, about which perhaps I know more than any other, because I believe a paper is being prepared by those much more capable of dealing with the thing than myself; but I hope that anyone who is interested in seeing the process in full work will pay a visit to the French works at Dives, near Trouville, where the work is going on on a large scale, and with beautiful results—results of commercial working, and on such a scale as to enable the manufacturers to say, "That is an average product coming "from the factory, and as such may be taken as a sample."

Mr. Cross. Mr. C. F. Cross: I have the pleasure of addressing the meeting upon the subject first brought before you this evening by Mr. Cooper, viz., the "Hermite" electrolytic bleaching process. I was interested in this process when it was first brought to this country in 1884-85, and of course viewed it with the amount of scepticism which, having special experience in matters of bleaching, and knowing nothing of electrolysis, I was entitled to feel; and it was not until after discussion with Mr. Hermite that my *à priori* difficulties vanished one by one, and I saw prospects of technical and commercial success. At that time there was a good deal of "fog" hanging round—not so much, perhaps, about Mr. Hermite, as about the actual mechanism of the electrolysis and process. The existence of fog generally leads to controversy, and a somewhat acute controversy took place in 1887-88, and is recorded in the *Journal of the Society of Chemical Industry*. In the year 1887 our original paper on the subject was published, confining itself chiefly to the practical results of comparative experiments with this solution, and with bleaching powder used under ordinary conditions. There is a difficulty before an audience such as the present in going into the special chemistry of the matter, and I will confine myself to dealing with its practical aspects. At that time Dr. Hurter, the well-known chemical specialist, gave us what is commonly known as a very severe "dressing," and he, having carried out a number of experiments, and having been been engaged for a long time in electrolytic

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investigation, criticised this paper of ours very severely, and arrived at a very different final and practical conclusion from that at which we arrived. However, we were then in the midst of our own laboratory investigation of the subject, and in 1888 we published our rejoinder to Dr. Hurter. Those who are at all interested in the evolution of the subject from a state of fog to something approaching lucidity will find it interesting to read that controversy. Avoiding theoretical questions as much as possible, I would point out that the essential matter in dispute was the ratio of a certain mixed chemical effect, namely, the bleaching of vegetable material—not a simple matter—to the watts expended in producing it. Now, so long as electrolysis had been confined to the actual separation of bodies in a condition to be weighed, there could be no doubt, of course, as to the mechanism of the electrolysis, and no doubt either as to the measurement of the current in the terms of the product obtained. But when you have to get that product to do work in a transitional state, and, having done work, to revert to something like the *status in quo*, there came, of course, an element of doubt and difficulty. We had to catch a particular element—namely, oxygen—flying, and that led to some very remarkable results, which we have been latterly enabled to establish, but which at the time naturally led to difficulty and dispute. From the point of view of the bleaching action of the products, the practical question was this: We had found that in bleaching powder units the work of this product (and we will take a ton of bleaching powder) could be effected on the electrolytic plan at a cost of from £2 10s. to £4. That is to say, given a certain amount of bleaching work to do, and requiring a certain amount of bleaching powder to do it, to the weight of one ton, and costing, of course, from £7 to £8 at the then rates, it could be done at from £2 10s. to £4 on Hermite's plan. Now, Dr. Hurter, adopting the method of cumulative criticism, and annihilating us in successive stages, arrived finally at the figures £22 to £42—multiplication of our figures by a considerable whole number. If Dr. Hurter had differed from us by any ordinary percentage, say 30 per cent., we should have thought it a serious matter; but, when he multiplied

Mr. Cross.

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Mr. Cross. our figures by a big number, we knew there was something totally wrong. His figures were based (1) on current-efficiency in units of chlorine per ampere, and (2) the bleaching effect per unit of available chlorine, measured as oxidising effect by the ordinary chemical reagents. Dr. Hurter's position with regard to these factors was this: He found that the eloctrolysis was conspicuously wasteful, and the efficiency—that is, the yield of chlorine per ampere—was only 14 per cent. of the theoretical; and then that the bleaching product was magnesium hypochlorite pure and simple, and that it could not exceed in bleaching effect the corresponding calcium compound or bleaching powder solution. These are the points upon which he differed from us. We found always from 70 to 90 per cent. of the current-efficiency, and the compound produced we clearly showed was not the ordinary hypochlorite. We also showed that there was formed at the cathode an active oxidising compound, and this, stirred into the solution, was dissolved, and no doubt contributed largely to the particular efficiency of the solution. Now since the publication of these papers, by which the mechanism of the electrolysis was fairly established, we have carried out experiments on the mechanism of the bleaching process itself. If you take bleaching powder as simply oxidising the compound to be bleached, it reverts to the form of calcium chloride ($Ca\ Cl_2\ O_2 = Ca\ Cl_2 + O_2$). Now we found, in bleaching with bleaching powder (and the matter required rather careful finding), that, instead of being confined to oxidation, it took also the form of an actual combination of chlorine with the fibre constituents. Now in these experiments we carefully compared the hypochlorites of calcium, sodium, and magnesium, the latter prepared in the ordinary way from the calcium compound by double decomposition, and the products of electrolysis by Hermite's method. We found that 26 per cent. of the calcium and sodium compounds was used up in the *chlorination* effect; in the case of magnesium hypochlorite, 14 per cent. of the total used went into this by-path or in some other direction; but with the electrolysed solution prepared by the Hermite process there was no chlorination at all. It is interesting to note that in bleaching with the electrolysed solution there

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is complete reversion to chloride, and therefore the bleaching process is limited to oxidation pure and simple. It is easy to see, therefore, why bleaching under the one set of conditions may be totally different from and much more economical than under another set of conditions. I am not going to detain you much more. I could not possibly exhaust this aspect of the subject without becoming very "special;" and it is much more interesting to this meeting to know that this process, having fought its way through not a few failures and certainly a great many difficulties, and having worked under free exposure to the fierce light which beats on the throne of things scientific in France (in the paper mills of France, where the scientific conduct of processes is certainly on a par with that in the mills in this country), has established itself, and continued to increase in favour through five or six years. During this time further orders have been from time to time placed with the makers for electrolysers—the only efficient way for the users to evince their satisfaction with the thing. The process must therefore be pronounced to-day, not a chimera, as it was judged to be by many high authorities, and altogether outside the range of practical science, but an undoubted and proved success. Mr. Cross.

Professor J. PERRY: Might I suggest that in a discussion before a Society of this kind by practical men who must have kept accounts, it ought to be quite easy to give us actual figures of the cost as compared with the cost of bleaching powder, and not merely the speculative figures which were contradicted in the papers five or six years ago? I have no faith in these speculative figures. Might I also suggest that Mr. Swinburne should give as an addition to his paper the materials from which he has produced the extraordinary estimate of one farthing per unit as the cost of electric power? Professor Perry.

Mr. DESMOND G. FITZ-GERALD: I think that the thanks of the members, and more especially of the younger members, of this Institution are due to Mr. Swinburne for having brought forward a subject which has been somewhat neglected, but which is likely to become of great practical importance. I cannot speak with the authority of anyone having conducted bleaching operations on a Mr. Fitz-Gerald.

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Mr. Fitz-Gerald.

large scale. In conjunction with others, I have taken some interest in experiments upon a semi-practical scale; otherwise my experiments have been confined to the laboratory. I think I shall begin by noticing a point in Mr. Swinburne's paper relating to peroxide of lead. Mr. Swinburne says, "Unfortunately, peroxide "of lead is reduced by hydrochloric acid, and it seems to be im- "possible to electrolyse salt without producing some free acid at "the anode." This statement as to the production of free acid is perfectly correct if you take, for instance, sodium chloride as supplied in commerce. Every sample I have examined has contained a considerable proportion of sulphate of soda. In that case sulphuric acid would be produced at the anode; and this acid, reacting on the sodium chloride, would, of course, produce hydrochloric acid. But in practice it is, of course, the easiest thing possible to maintain the solution at the anode in a neutral or alkaline condition. There need never be any free acid where chlorine is being evolved; but supposing that hydrochloric acid were produced, peroxide of lead in the form of lithanode is perfectly indifferent to the action of hydrochloric acid, so long as the current is continued. If you place peroxide of lead or a plate of lithanode in hydrochloric acid, you obtain chloride of lead, and chlorine is evolved; but if you make the peroxide the anode, chlorine is evolved, and no chloride of lead is produced. I have in my pocket, and will hand to anyone interested in the matter, a plate which, prior to coming here, I took out of a solution of hydrochloric acid, in which it had been used for eight or ten hours as anode in the production of chlorine. The plate is not affected, and that would also be the case if it had been used for a much longer period. Anyone interested in the matter can examine that. I think, however, that there is only one person here capable of speaking from actual experience as to the behaviour of lithanode when used as an anode on a large experimental scale. This is Mr. A. C. Falconer, a gentleman who has sent here some old anodes of lithanode which have been used in a solution of chloride of magnesium, and which have been exposed for a long time to the action of the hypochlorite of this metal. You will perceive that the plates are covered with a white

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substance which suggests chloride of lead; but that substance is magnesia and its hydrate. If that magnesia be scraped off, I think you will find that the peroxide underneath is perfectly intact. I will not detain you very long, but will give you as briefly as I can the result of my personal experience, although it may be in contradiction of that of others. I have come to the conclusion, and on grounds which seem perfectly satisfactory to myself, that the selection of magnesium chloride as a substance to be electrolysed has been an extremely unfortunate one. I can quite confirm, to the extent of my small experience in the matter, what has been stated as to the exceptionally high bleaching power of magnesic hypochlorite. But a circumstance which seems to me quite fatal to the use of this salt as a bleaching agent, is the fact that, even at ordinary temperatures, magnesium hypochlorite has a strong tendency to become converted into chlorate. I can speak with a little authority on this point, because after a certain experiment to which Professor Perry and Professor Meldola were called in, and in which we were all very much surprised to find that there did not seem to be any relation between the quantity of current expended and the quantity of available chlorine produced, I devoted a considerable amount of time and attention to the matter, in conjunction with my assistant, Mr. Burman, a rising young electrochemist. Hour by hour, and day after day, we compared the quantity of current expended and the quantity of available chlorine produced. After a certain percentage of hypochlorite had been obtained there seemed in some instances to be an actual disappearance of chlorine by the passage of the current. Ultimately the solution of the mystery was found to be that magnesium chlorate was produced at ordinary temperatures in considerable abundance. Magnesium chlorate is also formed when chlorine is brought into contact with magnesia in presence of water. I think, therefore, that magnesium chloride will have to be given up. In conversation with certain persons who have had experience on a large scale with this electrolyte, I have had my impressions confirmed, for I have heard of failures, considered at the time unaccountable, which I have no doubt may be traced

Mr. Fitz-Gerald.

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Mr. Fitz-Gerald. to this production of chlorates. Outside of the large operations which have been glanced at in the paper there are a great many small operations in which electrolysis might probably be advantageously introduced, and I would recommend the younger members of the profession to give attention to this. I may quote as instances the conversion of arsenious acid into arsenic acid, and that of ferro-cyanide (yellow prussiate) of potash into the ferri-cyanide (red prussiate) of potash; also the purification of sulphuric and hydrochloric acid on a large scale by the separation of lead and arsenic from the former, and of iron from the latter.

Mr. Mavor. Mr. A. E. MAVOR: I would say a word or two with regard to what Mr. Swinburne says in the sentences in which he deals with the electro-metallurgy of copper. He says, "Writers of books "on political economy are fond of saying that the chaffering of "the market makes people manufacture in the cheapest way," and so on; and at the end of the paragraph he says, "The first "manufacturers to take up copper refining by electrolysis on really "modern principles have a chance of making fortunes before the "market is disturbed." Now this would really be a most momentous sentence were it not for the fact that some manufacturers have already taken the matter up and are making fortunes. Some of the oldest copper manufacturers in the country have taken it up precisely on the principles Mr. Swinburne describes. I join with Professor Perry in the fervent hope that we may have full particulars and costs of all these processes; but I am afraid it is an academic hope, because costs are very difficult to obtain, and there are a good many commercial reasons which prevent such publication. But the copper makers have really taken this matter up, as I am in a position to know, with great enthusiasm and energy, and I do not think there is any danger that this particular branch of the electrical industry is in the least likely to fall asleep: the financial motives for the preservation of precious metals from argentiferous ores are so strong, and it is so much a matter of self-interest, that it is certain to be persevered with.

Mr. Falconer. Mr. A. C. FALCONER: I am not in any way connected with the electrical or chemical worlds, but I have carried out, with Mr. Fitz-Gerald, certain experiments extending close upon four years.

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Mr. Falconer.

The anodes that I have brought up here to-night have been in the solution nearly three years, and I do not think that any member who chooses to examine them will find that the material is injured. I may say this in the presence of Professor Perry, who is here, and who gave a report with regard to the process—that the lithanode that I used when Professor Perry was present and made his experiments was that which had been in the solution for nearly two years. I did not replace a single plate, except in case of breakage, and that happened occasionally through my very crude arrangement of having a false bottom covering the lithanode anodes. To keep down this false bottom I used bricks, so you may see how crude was the arrangement. On one or two occasions the bricks toppled down and broke the lithanode; the broken plates I replaced, but all the others were in the solution for nearly two years. I am sorry to say that to me there is a most unaccountable thing in Professor Perry's report, for whereas, after expending 4,000 ampere-hours, we got what Mr. Fitz-Gerald reckoned as the exact percentage of available chlorine that we ought to have obtained, or very nearly so, after expending a further 8,000 ampere-hours, what did Professor Perry find?—that the percentage of available chlorine had gone down more than one-half. There are, I think, only three ways in which that can be accounted for. First, that chlorates were formed in the bath. At the conclusion of the experiment Mr. Fitz-Gerald tested for chlorates, and found just the barest trace of them: that would not account for the loss of available chlorine. The second cause would be hydrogen combining with the chlorine. The third alternative would be the oxidation of organic matter in the bath. It puzzles me to know how it was that after 8,000 ampere-hours had been expended the percentage should be diminished by more than one-half. Mr. Fitz-Gerald has probably arrived at the right conclusion when he condemns *in toto* the magnesium chloride. But I am wandering into a subject I am not technically acquainted with, and therefore I had better confine my remarks simply to this—that my experience, extending over four years, is that lithanode is indestructible in chlorine solution; and I believe that Mr. Fitz-Gerald's observation is perfectly correct—that you can

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Mr. Falconer. even decompose hydrochloric acid by lithanode without injury to the anode.

Mr. SWINBURNE: I would ask Mr. Falconer whether there was oxide of magnesium on the lithanode.

Mr. FALCONER: We constructed the bath in this way: Our original intention was to put the anodes alongside of the bath opposite the cathodes; but Mr. Fitz-Gerald thought it would be better to have the anodes at the bottom of the bath, with plenty of magnesia on them, and that plan we carried out. I worked with a 300-gallon bath. Unfortunately, the construction of the bath was not of the most satisfactory description. I am surprised to hear Mr. Cooper defend so strongly the use of platinum. My experience is that platinum is attacked by chlorine. Our platinum connections, if not effectively insulated, would give way; and if it is so in making simple connections, what must it be where you have a platinum anode? It must give way sooner or later.

Mr. SWINBURNE: I do not wish this discussion to degenerate into a discursive conversation, but it is a very important matter that Mr. Falconer is dealing with, and I would like to ask him whether the lithanode stood perfectly untouched when it was vertical, without any magnesia on it.

Mr. FALCONER: You have a specimen of the vertical anode on the table.

Mr. SWINBURNE: This platinum that was used, was it in contact with lithanode, or was it used as an independent anode?

Mr. FALCONER: It was not used as an anode at all. We connected up the lithanode in the ordinary way with platinum connections. We found if there was the merest pin-hole in the insulation the platinum would disconnect itself from the anode. If that was so in the case of connections, what must it be in a process like Hermite's, where the platinum is immersed in a chlorine solution?

Professor Thompson. Professor S. P. THOMPSON: I would rather direct the discussion towards some points perhaps of more general interest than the detail of a single process, however important that process may be in itself. I rather question whether details of processes are much

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worth discussing, unless one has the commercial basis, namely, the figures. I would like to point out that, when one comes to put any one of these processes on to a commercial basis, one is bound to meet with difficulties that cannot be anticipated from the laboratory, and which seriously interfere with the question of possible profit in working. I may illustrate my remark by reference to the Hermite process in one particular mill. I will not mention the mill, where it is not now used. Electric bleaching has here been given up, not because you cannot make bleach liquor electrically, nor because you cannot make it economically electrically, but for quite other reasons, namely, this—that in the mill in question, where paper was to be produced and the pulp bleached, the arrangements were such that the material to be employed for staining the paper was always added to the pulp in the same vat as that in which the bleach liquor was made; that is to say, the pulp was bleached and dyed at the same time. The bleach liquor was not affected, but the consequence was that this dilute liquor coming into the vat brought a large quantity of water with it, that took away much dye with it that was wasted afterwards. It was not that the bleach liquor was defective, but their way of using bleach liquor made the process one that did not pay. Professor Thompson.

Mr. Swinburne has not touched on one or two processes which I believe are used commercially. I think he does not mention the process of galvanising iron by the aid of a skin-deposit of zinc, deposited first of all electrically before the sheets are dipped into molten zinc. Then, again, Mr. Swinburne does not do me the honour of mentioning the process of depositing cobalt—I suppose for the very good reason that it does not come under the head of commercial electrolysis. I wish it did; I wished to make it so. I find from experience that the one white metal which will stand a London atmosphere, with fog and the products of combustion, is the metal cobalt. I have in my laboratory articles coated with cobalt that retain their colour marvellously. Had they been coated with nickel they would have been yellow in three months, or if with silver they would have been black in a fortnight; but the one metal that can be

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Professor Thompson.

deposited and will stand the London climate is cobalt. Why do not our nickel-platers use it? The process is just as simple as nickel-plating. Another point I would like to allude to, namely, the circumstance that many of these electro-chemical processes apparently depend upon something not very well recognised by chemists. You have an intermediate action of something in the bath which undoubtedly makes the deposition possible. For example, in the deposit of nickel you have ammonia salts, in the deposit of brass you employ ammonia salts, and in my cobalt process you employ magnesia salts. What have these salts to do with the effect? I believe the explanation is, you have in this intermediate state metals in a "nascent" state. Everyone knows that nascent hydrogen has more powerful properties than natural hydrogen, and I believe that other things in a nascent state have greater properties also.

In text-books on such subjects, figures are given for the electro-chemical equivalents of metals in their different states—of "ferrous" iron, for example, and for "ferric" iron. I am not at all sure whether those distinctions are not absolutely absurd; at any rate, they are so in the case of ferric salts. You are not able to get a deposit of iron from ferric salts. What happens is this: Before you can obtain a deposit from ferric salt it has to be reduced to the ferrous state, and you only get a deposit from ferrous iron, and not truly from ferric iron. Another point which ought to be borne in mind is the necessary minimum electro-motive force to produce any action whatever of a chemical nature. Probably it may make the whole difference between its being commercially valuable and commercially valueless, that you should know that minimum electro-motive force in each case. In some cases quite small electro-motive forces—small fractions of a volt—are all that is required to carry the process out. There are certain reactions which chemists classify at present as molecular combination, or as some lower kind of chemical combination, that have as truly their electro-chemical equivalent in volts as have the more strong chemical combinations. Why, for example, in the deposit of copper, do you have the weakening of the solution at one end and the strengthening of the solution at

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the other, unless you stir up the liquor mechanically? Apparently something is going on besides the mere transfer of copper from anode to cathode; apparently the salt is carried along in the solution. If you have a reversible thermal reaction, it will necessarily have a corresponding electrical equivalent. It is quite possible that there are a large number of these chemical reactions. If we are ever to extract gold from sea-water, it must be by means of some exact electro-motive force less than that necessary to electrolyse water, but great enough to separate gold from its chloride. I need not say more upon this head, because it is obvious there must be a number of actions of this kind which merely require to be hunted up. Lastly, I would say a word about processes, such as that of Hermite, which are cyclic processes, wherein the material goes round and round and is regenerated after being used. The difference between a non-cyclic process and a cyclic process makes all the difference between success and failure. I believe Dr. Hurter's mistakes have largely arisen, at first undoubtedly, from his not recognising the difference between a cyclic process and a non-cyclic one. Let me illustrate that commercially by another process—the manufacture electrically of chlorate of potash from the chloride of potassium. Take a solution of chloride of potassium and begin to electrolyse it, and you get a certain amount of chlorate from it. But, however you go on electrolysing chloride of potassium, you cannot get more than a certain percentage—say 20 per cent.—converted into chlorate; if, however, you begin with a saturated solution of chlorate and electrolyse that, you get what you do not want, namely, chloride. People who do not understand the difference between cyclic and non-cyclic will say, "What is, then, the "use of trying to convert chloride of potassium into chlorate of "potash?" The answer is, Convert your 20 per cent. and let that stuff go away from the vat; crystallise out of it the less soluble chlorate; let the chloride which is not crystallised return back into the vat, and convert 20 per cent. of the residue. You skim off the cream, as it were, every time, and bring the milk back ready to be regenerated again and made creamy. A cyclic process will pay where a non-cyclic process possibly cannot.

Professor Thompson.

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I wish further to remark that, as in electric tanning, in which electrolysis plays so small a part and electric osmose plays so important a part, so, I believe, there are other cases in which osmose is the important part. For example, it is quite possible to separate to a certain extent animal and vegetable oils from mineral oils by subjecting them to a species of electric osmosis.

ner.

Mr. FALCONER: I wish to make a statement which I omitted to make in the earlier part of the discussion. I do not think the chief difficulty is as Mr. Swinburne states. Of course I speak from a non-professional standpoint, and with all deference to Mr. Swinburne—but what I do say is this—that I do not think there is so much attention required to the anode as to the cathode. If we can get a cathode that will not give off hydrogen in electrolysis, we have solved a great difficulty.

The meeting then adjourned.

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The Two Hundred and Forty-second Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 17th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 10th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Mr. Fairfax and Mr. Heaviside were appointed scrutineers of the ballot for new members.

The PRESIDENT: We will now resume the discussion on Mr. Swinburne's paper, "The Problems of Commercial Electrolysis."

Mr. G. L. ADDENBROOKE: I really do not know whether I have very much to say, at least directly, on the subject, because the particular point in which I am interested has been so discursively treated. I may say for the last two years I have devoted a great deal of my spare time to the consideration of aluminium, and have performed a large number of experiments on the metal—not, however, so much on the reduction of the metal, as on its alloys and its utilisation. Certainly I rather agree with what Mr. Swinburne says about the methods of reduction used at present—viz., that they are due to the heat of the arc, and not to any chemical action—but unless one happens to belong to a company which is making aluminium, it is very difficult to get any reliable information. At Neuhausen, on the Rhine, they are keeping everything very secret; and in England, too, the work done does not come largely before the public. Certainly splendid results have been obtained, and the price of aluminium six months ago was brought down to 1s. 9d. for large quantities; it has now gone up

Mr. Addenbrooke.

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Mr. Addenbrooke. again to 3s., and my impression is that it will go up a good deal further, because the demand is greater than the apparent supply for the next year or so. I understand that there is a very large number of people working at it, and no doubt large works will before long be started; still, the actual question of reduction is in a very chaotic state at present. Although the electric process of reduction has so far superseded the old sodium process, yet there are other processes which may still supersede the electric, and it is really impossible at present to predict what will be the ultimate outcome. I think, however, we may go so far as to say this: There is no doubt that within the next 25 years aluminium will not be very much more expensive than copper; and as it is a very good conductor—that is to say, for equal weight it has double the conductivity of copper—it is not improbable it may play an important part in the electrical industry, first as a conductor, and then as an alloy of copper. The aluminium bronzes form a very good series of metals—I have been working on them for a long time, with good results—and I think for tramway and other purposes these bronzes will be useful. At present, however, everything is in an experimental state, and a great deal still remains to be done, concerning which the present is not a suitable time to speak.

Mr. Lloyd. Mr. C. O. LLOYD: Mr. President and gentlemen,—I have read with great pleasure the very interesting paper by Mr. Swinburne, since it introduces a subject which has been partiularly interesting to me during the last twelve months. The title given to the paper being "Commercial Electrolysis," it embraces a very wide field; however, Mr. Swinburne apologises at the end of his paper if, out of the innumerable applications of electrolysis which have been patented, he has omitted to mention any that are of practical utility; I think, therefore, I may venture to make a few remarks on recent advances in electro-plating. Professor Thompson introduced the deposition of cobalt at the last meeting: the deposition of cobalt is a comparatively easy matter, suitable solutions, though not perhaps the best, being given in many text-books. A matter which to me, however, seems of far greater importance, is the deposition of alloys. It may be

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said that electro-plating is a comparatively old industry, and on that account Mr. Swinburne would have been going over old ground if he had introduced it into his paper; nevertheless, I think that electro-plating with alloys is a distinct advance. I refer now particularly to the deposition of silver alloys. When we come to consider the output of Sheffield, say for the year 1883, which amounted to upwards of 300,000 ounces of silver, and suppose that 20 per cent. of that amount consisted of a metal which may be only 50 per cent. cheaper, the saving at the end of the year amounts to some £5,000, while the outlay of capital on solution and anodes is greatly diminished. Suppose now that an alloy can be obtained at, say, 10 per cent. cheaper, and that that alloy possesses more valuable properties than the original metal,—that it is much harder, and does not tarnish so readily,—then I consider that a very great advance has been made. Mr. Lloyd.

On the table I have placed a number of samples of electro-deposited metals and alloys; among these I would call your particular attention to some alloy deposits named "hard standard "silver." These specimens consist of silver alloyed with a small amount of cadmium; the three samples shown containing 10, 20, and 30 per cent. By this means a considerable improvement on ordinary silver plate is obtained; while I think most of you will find it very difficult to distinguish any difference in colour between them and some samples of pure deposited silver, also shown.

The deposition of alloys, however, is by no means a simple matter. In depositing single metals there is really comparatively little for the electrician to do beyond the mere measurement of the current and the potential difference at the terminals of the bath, but in the deposition of alloys there are a variety of secondary actions which require great attention in order to understand how they vary the deposits produced.

I have also shown samples of electro-deposited brass. Brass has been deposited for some time past, though not to any great extent, and I think the samples laid before you are extremely good specimens of deposited brass.

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Mr. Lloyd. There is one other specimen I would like particularly to call your attention to, and that is a sample of palladium plating. The non-oxidisability of the metal, and its perfect adhesion to the underlying metal—when these properties become more widely known—point to a great future for palladium plating; and our experience with it shows it to be superior to any other metal for coating mirrors which have to stand the intense heat produced by arc lamps.

In conclusion, I would mention that I am extremely sorry that the spaces in one of the frames allotted to samples of platinum and iridium are not filled up. Unfortunately, I had not time to prepare them; indeed, I have only just had time to prepare those shown. We have successfully deposited both these metals, though we have not yet made very extensive use of them.

Mr. Bevan. Mr. E. J. Bevan: There are one or two points I should like to call attention to in Mr. Swinburne's paper; but before doing that, I should like to refer to the observations of Mr. Falconer in the discussion last Thursday. In that discussion he stated that the chief objection to the use of magnesium chloride was the fact that a large proportion of magnesium chlorate was formed—I mean in the Hermite process. The formation of chlorates, and even of perchlorates, is a very simple matter; in fact, the formation of potassium chlorate is one of the processes referred to by Mr. Swinburne. It depends entirely on the conditions. If you suit the conditions to make magnesium chlorate, you can make it in abundance; and if you take care that these conditions do not obtain, you get very little chlorate. The formation of chlorates was one of the chief objections of Dr. Hurter to the process, and if in practice the large amount was formed that he declared to be formed, of course the process could not possibly pay. On the other hand, we have it on the authority of Mr. Cooper that the process is a profitable one. With regard to the question of power, Mr. Swinburne takes it at a farthing per kilowatt, and several speakers were of opinion that this was an extravagantly low price. I am not an engineer myself, but I cannot really see how, if you make a liberal allowance for coal, labour, and all the

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other charges incidental to that matter, you can get the price very much more. It is interesting to note that within the last week or so Dr. Hopkinson has published a paper—delivered, I think, before the Junior Engineering Society—in which he states that the cost of a kilowatt should not exceed 0·33 of a penny, which is not very far from Mr. Swinburne's figure. I have no actual authority to say so, but I think it has been published before, that Mr. Preece is of opinion that that is about the price. Now, with regard to the production of oxygen by the electrolytic process, it is hardly fair to compare the cost of oxygen by the electrolytic process with the fancy selling price that the Brin Oxygen Company puts on it. In fact, oxygen can be prepared by the Brin process at certainly less than 3s. per 1,000 feet. If that be so, there is not a considerable margin, as Mr. Swinburne says, in favour of the electrolytic process. With regard to the production of alkali, I have had more practical experience of that than of these other processes. Mr. Swinburne assumes that a current-density of 3 volts is all that is necessary. I only wish it were so: it would reduce the cost of power very considerably. As a matter of fact, you can electrolyse sodium, or any other chloride, with about 2½ or 3 volts; but the yield per unit of plant is a very small one, and it would be necessary to have an exceedingly large plant to work at that voltage. In practice I have found that from 4 to 4½ volts is a very economical mean. With regard to the strength of solution, that is, of course, a very important point. Mr. Swinburne says he can electrolyse a salt solution until it reaches 30 per cent. caustic soda, but he does not tell us whether that has been done economically or not. As the electrolysis increases and the salt decreases the efficiency of the current—that is, the proportion of caustic formed to that which should be formed—this efficiency rapidly diminishes. Of course caustic soda acts, as Mr. Swinburne says, in solution as a conductor, and he seems to regard that as an advantage. As a matter of fact, if it is a conductor, it is also an electrolyte, and decomposes *itself* into the wasteful form of hydrogen and oxygen. With regard to evaporation, Mr. Swinburne says that the evaporation of the caustic liquor in a Yaryan

Mr. Bevan.

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Mr. Bevan. evaporator can be effected at such a price as would not raise the cost of the caustic more than a shilling or two per ton. I think that is underestimated; I do not see how it is possible. Another point, perhaps, which has not been sufficiently considered: For every ton of caustic produced you have to manipulate and get out in solid form several tons of salt, which, of course, is a very considerable matter. This can be got over by precipitating the alkali as bicarbonate, allowing the precipitate to settle, filtering the salt solution, and re-electrolysing that. With regard to the anodes, I agree with Mr. Swinburne. I have had an opportunity of examining the Greenwood anode, both before and after running, and as far as experience goes it is calculated to stand the wear and tear put on it. In the Le Sueur system of electrolysing salt, a plan of which is on the screen, the anodes are ordinary gas carbon, and an arrangement is made by which these anodes, as they gradually wear away, can be brought down close to the cathode, which is right underneath the diaphragm. Now, as to the commercial uses of the soda produced, Mr. Swinburne says that the presence of salts will not interfere with the action of the alkalies. I venture to think he is somewhat in error. I made a number of determinations some years ago with the view of showing that the purest form of caustic soda was the most economical form, and I found the presence of, say, 10 per cent. of salt, which is not apparently a very large amount, seriously impaired the efficiency. The results I arrived at are recorded in the *Journal of the Society of Chemical Industry*, and anyone interested in the matter can refer to it there. I believe this loss of efficiency in soda would be the same in the paper trade, or any other trade in which caustic soda is used. Another serious point is the presence of a large quantity of salt, seriously affecting the recovery of the soda. I should not like it to be thought that in venturing to bring these criticisms before the meeting I am of opinion that the electrolytic production of soda will not pay. I believe it will pay very handsomely; I believe it will be the process of the future; and I think electricians and chemists owe a debt of gratitude to Mr. Swinburne for bringing the matter before us in such a concise form.

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Mr. A. P. TROTTER: It was pretty well decided by Dr. Rideal, who conducted the chemical part of an investigation which we made some time ago on electrical tanning, that there was practically no true electrolysis in that process. The suggestion of Professor Thompson that an electric current facilitates osmosis, in some way not yet properly understood, may have extremely important effects upon industries in which electric treatment has not yet been even attempted. I very much hoped, when engaged in examining a process of electric tanning, that we might find some theory to account for it. But in the paper which was communicated to the Society of Chemical Industry by Dr. Rideal and myself, nothing in the nature of a theory was put forward. We examined various theories, from the time of Grotius up to the present date, on electrolysis, with the hope of finding one that would fit the results, and perhaps the most plausible one was that of Clausius, who suggests that the molecules are always changing partners, and disassociation is always present to some extent, and the effect of electro-motive force is to give direction to the movements already going on. There is, therefore, no real expenditure of work necessary; it is merely directive, and molecules are encouraged to go in one particular direction. That will partly account for the extraordinarily small amount of electrical energy required in electric tanning—a mere nothing at all, In the last paragraph, Mr. Swinburne alludes to dyeing, or, rather. to the electrolytic production of dyes; but the process of dyeing seems to me not at all unlike that of tanning. You have a fabric of organic materials, and many of the solutions are organic, and you get the one passing into the other, precisely as in tanning, and the time required in tanning is not long; but I would throw out the suggestion that this sometimes difficult process might be very much facilitated by some such treatment as this. Mr. Trotter.

Mr. Swinburne alludes in his paper to the fact that a process ought to be remarkably good on paper before you can expect anything at all of it in practice. I would say it should be not only remarkably good on paper, but also remarkably good in the laboratory, before you try to put in practice. I was connected some years ago with another process, which was first electrolytic,

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Mr. Trotter. and after merely chemical—the decortication, or de-gumming, of rhea fibre. The first process consisted of the electrolysis of a rather disgusting mixture of chemical substances. The rhea fibre subjected to this treatment in the laboratory in small bundles was remarkably encouraging. You take stuff like dried bean-stalks, and produce hanks of a fibre exactly like white floss silk. Afterwards, putting it on a large scale, as soon as we tried a hundredweight or more at a time, it came out a tangled mass. The machinery was clogged, and nothing could be done with it.

Mr. Andreoli. Mr. GABRIEL ANDREOLI: Sir,—I wish to take up a point in Mr. Swinburne's paper which has already been discussed both last week and this evening. He says that the only difficulty in the electrolytic production of alkali is in the anodes—that platinum is too expensive—that carbon is destroyed and that peroxide of lead is reduced by the free hydrochloric acid which is formed in the electrolytic solution. Mr. Fitz-Gerald has told us what peroxide of lead in the form of lithanode will do. If ordinary peroxide of lead will only behave as well, I do not think we need go any further, as it cannot get to a higher degree of oxidation and resists the action of chlorine. I believe M. Planté was the first to use it in lieu of platinum as an anode in electrolysing solutions. He used it in connection with electro-deposition. All those who are interested in the question know of Professors Bartoli and Papasogli's researches on the behaviour of carbon in a solution which evolves oxygen at the anode. The carbon is soon destroyed, disintegrated, and mellitic acid and mellogen are formed; but it is not generally known that, according to the same scientists, if you use denser solutions of salt, less oxygen is given off and the carbon is not nearly so much attacked. If you go on in increasing the density of an hydrochloric acid or chloride solution, you get chlorine only and very little oxygen, and the carbon is hardly attacked at all. Really there is no more necessity of having an absolutely indestructible anode than we should expect machinery to go on for ever without standing a certain amount of wear and tear and doing their work without renewing them. The idea of replacing the ordinary bleaching process by an electrolytic hypochlorite presents so many difficulties that it has

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been practically given up in America as well as in this country. There is such a large bulk of liquor to deal with that you lose a large amount of salt in the bleaching operations, and to avoid this loss you have to electrolyse weaker solutions which give off oxygen, and with carbon anodes the carbon is destroyed; if your solutions are weaker, you decompose the water and you will have a loss of power, because you waste your current up by decomposing this water, and that diminishes your yield. Another cause of diminution of the yield of chlorine per unit of current is that when you have once sent the solution through the bleaching potcher, that solution becomes charged with organic substances, some of which are dissolved; and unless you have an extraordinarily good filter, you will also have some of these organic substances held in suspension in the solution to be electrolysed over and over again. Those which are in solution are acted upon by the current, and here again is another loss of power. The organic substances mechanically suspended are seized by the hydrogen evolved at the cathode and froth several inches thick is formed on the top of your electrolysers; hydrogen also diminishes your chlorine yield, because the hypochlorite is reduced. It is therefore evident that in the electrolysis of a chloride solution there are many difficulties more serious, greater, and more complicated than the so-called anode difficulty. The solution of this problem seems to be more in the production of chlorine and of caustic alkali. You may then electrolyse concentrated solutions by means of carbon anodes. Then the hydrogen does not reduce any hypochlorite, because you are supposed to liberate the chlorine in the free state. You have a strong solution and very little decomposition of water. Hydrogen, however, is the real enemy, and the only difficulty, as far as I can see, is that the hydrogen which is liberated by the sodium dissolving in oxidising is liable to unite with the chlorine and form hydrochloric acid, which reduces the alkali to the state of salt again; when liberated from the solution and mixed with the chlorine, it forms a highly explosive mixture. There may be other difficulties, but I have not come across them yet, although for several years I have been working on a practical scale this question of production of hypochlorite and chlorine by

Mr. Andreoli.

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Mr. Andreoli.

electrolysis. Another question in which I am interested in Mr. Swinburne's paper is the question of ozone. The quantity of ozone produced by electrolysis proper is so very little that it may be almost neglected. It can only be produced on a larger scale by silent discharges. That we can do very easily with the transformers that we have now. There are any amount of applications of this ozone, as Bunsen said, but we cannot call this really an electrolytic operation. Hardly can we say that it is an electro-chemical one. No doubt the oxygen molecules which are transformed into ozone are condensed; at least, there is one theory which says the molecules are split up and the separate atoms fix themselves on the unaltered molecules. Ozone can be applied in very many instances, more particularly to bleaching molasses and sugar, and it is a very powerful disinfectant; it kills bacteria, and is used in hospitals, and we can now generate it in almost unlimited quantities at a very low cost. The production of ozone is no longer a problem; the real problem lies in its numerous commercial applications.

Mr. Cooper.

Mr. C. O. COOPER: Mr. Andreoli has brought up the subject of the difficulties of electro-chemical bleaching, and what he has said would lead one to suppose, had they never heard of it before, there were difficulties; and I agree that there have been difficulties such as he mentioned, which in the Hermite process have been got over. In nearly every instance Mr. Andreoli has mentioned we have, I admit, found the difficulties he states he has found, but we have got over those difficulties some years ago. I will not detain you, for I feel I am a little out of order, but I will make a statement which I am prepared to prove—that at the present moment, on the Continent, we are replacing, in the actual fact of bleaching in mills, bleaching paper pulp, cotton, and yarn equal to replacing 3,000 tons of bleaching powder per annum. Professor Perry suggested at the last meeting that it would be interesting to give some figures of actual cost. I feel that this is a somewhat difficult matter, because it necessitates so much detail; but I can, and shall be pleased to show Mr. Swinburne, or any gentleman, figures given to me by practical men working in their own mills, which show a saving over the old methods of bleaching paper pulp

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and cotton with bleaching powder amounting to 80 per cent., exclusive of water power, for some of these gentlemen write down their water power as costing nothing, but they say they often make an 80 per cent. saving; and in other cases where they use steam power, and taking one horse-power on the pulley of dynamo equal to 1 franc per horse-power for 24 hours' running, they make a saving from 55 to 45 per cent. I will not trouble you with any more, but these are the practical results. Mr. Cooper.

Mr. B. BLOUNT: I should like to touch upon a matter which Mr. Swinburne has treated adequately enough, but has scarcely received in the discussion the attention it deserves. I refer to the obtainment of copper from the ore or from the matte direct. The refining of copper is quite a different thing from the winning of it, either from the matte—which may be regarded as a sort of half-way house between the ore and the crude copper—or from the ore itself; and it appears to me that although the refining of crude copper cast into suitable anodes is a very hopeful and excellent proceeding, and one likely to relegate the ordinary dry methods, even of the best and most modern type, into the background shortly, yet that still greater advances will be made when the ore itself is treated directly with the same success. Mr. Swinburne gives details of the Siemens-Halske and of the Höpfner process, and with respect to the former little need be said; but with regard to the latter the following statement in Mr. Swinburne's paper requires comment:—"I do not know why the "chloride should be preferred to the sulphate; and no infor- "mation is given as to what is done with the sulphur in the ore, "and with the excess of ferrous chloride that must be produced." It seems to me, as a chemist, that the chloride is preferred to the sulphate because of the ease with which cuprous chloride is formed, whereas cuprous sulphate is not known to exist; and advantage is taken of the fact that cuprous chloride will yield more copper per unit of currrent than would cupric sulphate, because the copper is already present in a partially reduced state. Mr. Blount.

The whole object of the Höpfner process, as I understand it, is to take advantage of the reducing action of the cupriferous material of the ore, and thus get part of the work of reduction

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Mr. Blount. effected outside the decomposing cell. The chief difficulty in the Höpfner process appears to be that indicated by Mr. Swinburne in his reference to the electrolytic production of alkali, namely, the provision of a suitable anode. The anode is always a source of trouble: either it will not stand, or, if it stand, it will not conduct. The crux is to obtain an anode that will fulfil both requirements. Mr. Höpfner has lately selected a curious material, viz., ferro-silicon, but it is scarcely one which will commend itself to the chemist. Possibly the Greenwood anode would do better for this purpose, as I have heard favourable reports of it recently.

Mr. Swan. Mr. J. W. SWAN [*communicated*]: The following are some of the points that call for comment in Mr. Swinburne's very suggestive paper:—

Section 1. In estimating the cost of a kilowatt at a farthing an hour it would be useful to know how much of this was for coal —and the price of the coal—and the other elements of the cost in detail; also, whether there is any instance of this low cost having been realised in actual practice.

Section 3. Under the heading "Aluminium" the author says, "It seems to be impossible to deposit aluminium from any of "its solutions." There has been a remarkable incertitude as to this point on the part, at least, of the authors of books on electro-metallurgy, but in the face of the following statement which appears in the *Scientific American* of October 22nd, 1892, in connection with a detailed description of plating large pillars with aluminium, it seems hardly possible to disbelieve any longer. It is said on page 262: "The fact is, that when two substances are "present requiring different E.M.F. to decompose them, if the "E.M.F. is high enough to decompose the higher compound, "the current is divided between them in some ratio decomposing "them both; and I find that by using a solution of aluminium "that has but a slight dissolving effect upon aluminium, with a "density of current of 8 amperes to the square foot, with sufficiently "high voltage (6½ to 7), aluminium can be deposited on the "cathode at the rate of one gramme per hour per square foot, in a "reguline state, and with higher currents it can be deposited much

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"quicker, but will be in a pulverulent state, which does not "adhere." Mr. Swan.

Section 3. In discussing the question of the extraction of copper from copper matte, I observe—speaking of Dr. Höpfner's method—Mr. Swinburne says, "I do not know why the chloride "of copper should be preferred to the sulphate." The main features of that process are: Instead of electrolysing sulphate of copper solution, Dr. Höpfner employs a solution of *cuprous chloride* (Cu 2 Cl 2) as the electrolyte, and after the partial deposition of the copper and raising of the cuprous chloride to *cupric chloride* (*Cu Cl*), he reproduces the *cuprous chloride* by treating the *cupric chloride* with ore, obtaining sulphur at the same time.

Formulæ of the decompositions—

I. Electrolysis of the *cuprous chloride:*

$$Cu\ 2\ Cl\ 2 = Cu = Cu\ Cl\ 2.$$

II. Regeneration of *cuprous chloride:*

$$Cu\ Cl\ 2 = Cu\ S = Cu\ 2\ Cl\ 2 = S.$$

The chief claim of Dr. Höpfner seems to be that he obtains a quantity of copper doubly larger than what is usually obtained by a given current. He says, "For each ampere of current I "obtain 2·36 grammes of copper per hour, instead of 1·18 grammes "yielded by the electrolysis of the sulphate." The reason of this is, of course, that he electrolyses a *cuprous* compound.

Section 4. In connection with the question of the electrolytic extraction of zinc, I may remark that there is great room for improvement on the ordinary process of zinc smelting—it is a most wasteful and altogether imperfect process. There is in it an immense consumption of coal—for calcination and for smelting —and a great loss of metal, 10 to 15 per cent. of the 40 to 50 per cent. in the arc being unrecovered, making a loss of 20 to 30 per cent. of the metal. The inducement to effort to improve this process by electrical or any means is very strong. Among the electrolytic processes one of the most recent, and perhaps one of the most hopeful—Burghardt's—is not named. Burghardt proposes to deposit zinc from a solution of zincate of soda.

Section 5. A clear distinction should be drawn between the electrolytic extraction of the metals from their ores and the

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Mr. Swan. electrolytic refining of the crude metals. Electrolytic refining has been attended with great success in a commercial sense in the case of copper refining, and probably the same principle can be used with success in the case of other crude metals.

Mr. Mordey. Mr. MORDEY: On the question of carbon anodes, I notice several passages in the paper indicating that there is some uncertainty as to what takes place when carbon is used as an anode. Some 12 or 14 years ago I found that CO_2 was formed pretty abundantly. I was making a class experiment on the electrolysis of water acidulated with a little sulphuric acid. I had carbon electrodes, and when I attempted to show the explosion of the mixed gases they did not explode. I took the thing home and investigated it, and I found that on passing the gases through lime-water there was an abundant whitening of the liquid—the ordinary test for CO_2. I searched all the books I could, and found no reference to this action in electrolysis, until I looked—where I ought to have looked at first—in the "Experimental "Researches" of Faraday. I have looked up the reference again. It is in paragraph 560 of the first volume, dated June, 1833. He points out that CO_2 and CO are given off freely when water is electrolysed with charcoal electrodes. It now appears that in bleaching operations trouble is caused by the deposition of carbon. I noticed this deposition. Now, I did not understand that, and I should be very glad if it could be made clear in this discussion. Of course, if the carbon is merely oxidised, and if carbonic anhydride and carbon monoxide are formed, then these are, I suppose, given off as gases. But finely divided carbon appears to get into suspension. In my experiments I was using extremely dense gas carbon, very hard indeed, and it was scarcely likely that the almost impalpable carbon powder that was deposited was due to disintegration of the carbon. I thought at the time, and I do not see now why it may not be, that there is some other action going on—that the oxides are decomposed in some way, and that free carbon is thrown down in a very finely divided state in the liquid. Perhaps Mr. Swinburne can give us some information on this subject.

The President. The PRESIDENT: Might I ask Mr. Mordey whether carbon

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monoxide or carbon dioxide—whichever it may be—is produced in the electric circuit, or is it a product produced chemically outside the electric chain? Does it affect, in other words, the potential difference required for the decomposition of the dilute sulphuric acid? The President.

Mr. MORDEY: My experiment was a very ordinary one. I had a closed electrolytic cell, with a couple of electrodes, in this case of carbon, and a tube to collect the gases. The lime-water was very quickly whitened, and the gases, instead of exploding sharply, went off with a very faint puff indeed, showing that it was almost entirely non-explosive. Mr. Mordey.

The PRESIDENT: What I meant was this: In the ordinary decomposition of water electrolytically the result is simply oxygen and hydrogen, but you have burnt carbon during the process: you have therefore produced heat, or have diminished the electrical energy which is required to split up the hydrogen and oxygen; in other words, the potential difference to produce the chemical action would be different if there were not carbon but platinum, otherwise the formation of dioxide would produce heat and be outside the electric circuit. You do not know, perhaps, which is the case?

Mr. MORDEY: I can give no information on that point; I simply investigated the product to find what really came off, and I do not know whether the P.D. across the electrodes was the same as with platinum electrodes or not.

Mr. RICHARDSON: Might I ask if the last speaker measured the quantity of gas that was given off?

Mr. MORDEY: The experiment was not quantitative.

Mr. RICHARDSON: I mean the temperature due to the CO_2 in the water expelled by the other gases.

Mr. MORDEY: I am afraid I have given you all the information I can on the subject. When I found that Faraday had found the same thing, and had quite clearly stated, in 1833, that CO_2 and CO were abundantly formed when slightly acidulated water was electrolysed in this way, I gave no further attention to the subject. I was only surprised that there was no mention of this action in the books I was able to refer to. It is apparently not

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Mr. Mordey

generally known even now, and Mr. Swinburne says he was not aware of it.

Mr. Richardson.

Mr. J. C. RICHARDSON: I am connected with a process for decomposing common salt, expressly with a view of getting products in useful form suitable for paper manufacture, and what Mr. Andreoli has said in reference to carbon anodes I can quite bear out, namely, that using the carbon in a dense common salt solution considerably reduces its rate of disintegration, which is proportional to the density of current and strength of solution. I find this by the action which is shown on the carbon itself, by the depth of the layers taken off at those points and surfaces nearest the opposite electrodes; and I think Mr. Andreoli is quite right in what he says in reference to weakening the solution—that where you are liberating chlorine from common salt in weak solutions there is a great tendency for a portion of that chlorine to decompose water, and instead of liberating chlorine you may get a solution of hydrochloric acid and oxygen. In the case of the formation of hypochlorites, it is very probable that where you are using a very dense current, with small section of liquid between electrodes, you may get the chlorine in such a form as to form hydrochloric acid, which, being in the neighbourhood of hypochlorites, would, by electro-molecular action, decompose them, with a re-formation of water and the reduction of hypochlorite to the ordinary chloride solution. If we have that action, or some such action, going on at the anode, we have a very similar action going on at the cathode. I may here state—what probably has been often stated before—that, in regard to chemical change in bleaching by means of bleaching powder, it is not available chlorine at all, but rather available oxygen, which we have to consider. If this oxygen be held, as it is no doubt held, very loosely, the hypochlorite is in a very unstable condition. If you reduce the oxygen to the condition of water by hydrogen, you have the formation of ordinary chloride solution and a waste of energy. We have heard it said that chlorates are formed, and not only chlorates, but probably chlorites and hypochlorates, and these reduced again to chlorides. The change goes on in a very peculiar way, and that is why I think that Mr. Swinburne at the outset of his paper

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made a mistake by not giving more consideration to the changes which do take place in electrolytes, not only at the electrodes, but in the body of the solution. Theoretical calculations in reference to the electrical energy required for certain practical results are, as Professor Thompson remarked, no doubt very misleading. In regard to my connection with a concern which claims to be proceeding upon lines which are of a practical character, and having at its back the use of a paper mill in which the products can be employed in a practical way, I think that I may be able to put gentlemen here on the right track by explaining the lines upon which we are proceeding. I do not think for one moment that the formation of hypochlorite solution for bleaching operations will pay, unless its application is to some refined or fancy purpose in which you require small quantities of solution at a time, and which have not particularly extensive applications with regard to the material that requires to be bleached; as a gentleman on my right says with regard to his beautiful "china grass," rhea fibre, or something of that kind. The lines on which we are proceeding are: We take an *almost* saturated solution of salt—we do not take a saturated solution, for the simple reason that the soda as formed at the cathode has a great affinity for water and throws a small quantity of salt out next the cathode, which tends to incrust it, unless you add water to the cathode compartment. To avoid that, we use a solution not quite saturated, by which the salt is not thrown out at the cathode. I may say with regard to the polarisation at the cathode, touched upon, that may be overcome by using oxide of copper (Cu O); and if the electrodes be used horizontally the soda formed at the bottom may be run off without employing any kind of partition between electrodes, and if used vertically it descends to the bottom, where it may be drawn off as a concentrated solution. I do not quite see how Mr. Swinburne—and of course I want to be friends with him—can have obtained a solution of 30 per cent. soda so very easily, because strong soda solution has such an affinity for water that it will not allow much salt to exist in its presence. With regard to the chlorine given off at the anode, we take our chlorine off as gas at the anode, and lead it

Mr. Richardson.

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Mr. Richardson.

into "lime boxes," or lime chambers; but as this subject will no doubt have its application mainly to paper manufacture, our plan generally is to use it in the form of a bleaching solution, and this is made by leading the gas into a series of milk of lime tanks fitted with agitators; and the solution may be drawn off when of the required strength, or made stronger when it only requires the addition of water to make it serviceable for direct use. As this solution of hypochlorite is considerably more efficient for bleaching, in proportion to its specific gravity, there is a greater bleaching action in it than from the solution used from dry chloride of lime. Dry chloride of lime has usually to be conveyed a considerable distance before use; and if stored for any length of time it decomposes and re-forms chloride of calcium, which, of course, is practically valueless, and sometimes it will lose as much as 30 per cent. of its efficiency in this way.

As regards the anode, we have proceeded on the lines of the use of gas-retort carbon, and we find that carbon disintegrates at a rapid rate under certain circumstances. But we find, after working night and day for some months past, that its rate of disintegration does not by any means preclude its application to the particular purposes now applied for decomposing common salt. The carbon mud produced—regarding which a speaker, in alluding to the liberation of $C O_2$ given off from some experiments he had tried, remarked that it comes off in a very fine, almost infinitesimal, division—sometimes it comes off in lengths which are distinctly measurable, sometimes 1-16th of an inch, and sometimes more, as though there were some action really cutting away or dissolving portions of it, or removing another compound not pure carbon, but a hydrocarbon, a metal, or an oxide. With regard to the soda solution, we draw it off from our apparatus ranging between 8 to 10 per cent. of soda—sometimes less and sometimes more—thus making it useful for paper manufacture; and an undecomposed salt we recover, if necessary, by evaporating down the solution to a twaddle of 50 or 60 or more. That solution passes on to the boilers, being diluted down to about a twaddle of 12. The recovered salt is again used in the electrolytic tanks. With regard to the solidification of soda, of course that is not necessary in a paper.

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mill. Chloride of lime in solution and soda in solution have a maximum value of application in paper manufacture. So far as our operations have gone we have met with most encouraging results, and as a proof of that I may say that the gentlemen who have put their money into this concern have guaranteed as much money as may be required to enlarge the plant. Mr. Richardson.

Mr. A. C. FALCONER: I wish simply to say a word about what Mr. Cooper and Mr. Andreoli have said. I do not quite agree with our friend Mr. Andreoli in what he said about electric bleaching. Mr. Fitz-Gerald and I have worked for four years, and our results have always been better than our calculations. I have not had time to read carefully a very interesting paper by Mr. Andreoli's father in the *Electrical Review;* but there are certain statements there which ought to be investigated. In reference to what Mr. Cooper said: If Mr. Cooper can possibly make electric bleaching pay when using a platinum anode, then I say that with peroxide of lead as an anode it will pay very considerably better. In common with Mr. Fitz-Gerald, I feel certain that lithanode will remain unattacked in the presence of hydrochloric acid, so long as the current passes. Mr. Falconer.

The PRESIDENT: My reason for asking Mr. Mordey about the formation of the carbon dioxide is because the question whether it is an electrical, chemical, or an electrolytic action seems to be one of very great importance. If it is only a chemical action, the cell is simply heated, and the action is like any ordinary burning, and is not of importance. What I mean is this: If it is really an electrolytic action, then what he has done is this—he has decomposed water, and formed carbon dioxide. Of course the heat of combustion of hydrogen is greater than the combustion of an equivalent amount of carbon, and therefore energy is required to produce that process. If, however, you employ the oxide of a substance which has a less heat of combustion than carbon, then, if what he found was really an electrolytic process, it would be possible to obtain electrical energy by the combustion of carbon in an extremely easy way, which is the one thing which so many of us have been looking for for so many years. It would mean, therefore, combining with your carbon The President.

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The President.

electrodes some material—some other oxide—the heat of the combustion of which—the energy, in fact, required to split it up—was less than the energy given out by the burning carbon: you would then have a very simple electric cell, in which the positive substance would then be carbon. I can hardly hope for such an easy solution of the process, and therefore am very much afraid that the E.M.F. required to produce the decomposition will not be altered by the production of the carbon dioxide, and that the formation of the carbon dioxide which Mr. Mordey observed must be a secondary chemical action, and the equivalent amount of energy goes to slightly warm the cell.

Another point: The action of the current in producing an electric transference—which is said, for example, to occur in electric tanning—is quite distinct from electrolysis. There was an example I came across several years ago. I wished to make experiments on the process referred to by Mr. Swinburne for—I think it was—an Australian firm. It was to test a method for preventing the mercury from becoming "sick," as it is technically called, by the formation of the gold amalgam. They used rain water running over large troughs of mercury: the mercury picks up the gold, which is easily extracted by the subsequent evaporation of the mercury. But after a time the mercury has collected so much gold that gold particles may be poured over the surface of the mercury amalgam without being caught. To overcome that difficulty it was proposed to send the current through the water to the mercury. That produces hydrogen at the surfaces of the mercury, which cleans the mercury, and so exposes a fresh surface for the fresh tracking of gold. A great objection to the method was the very large E.M.F. required to produce sufficient decomposition of pure water. They used, I mean, ordinary culvert water running over the mercury, and to decompose sufficient to clean the mercury was expensive.

A thing occurred to me to try which had nothing to do with electrolysis atall. I wondered whether the cleaning of the mercury might be due to any cause altogether different, and not merely to the hydrogen being produced, but possibly

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to the gold itself being carried down through the mercury —that is, as if the current were in fact a vehicle for taking the particles of gold and forcing them through the mercury. I made experiments, and prepared tubes of similar mercury, and put similar particles of gold on the top, and then sent a current for a certain number of hours through one tube, and no current at all through the other. In the morning of the next day, or the day after, I analysed the layers of mercury in each of the two vertical tubes; and in the case of the one without current the mercury at the bottom was pure, and in the one with current the mercury at the bottom contained a good deal of gold, showing that the mere passage of current apparently conveyed gold down to the bottom of the mercury. The President.

When Professor Perry and I were in Japan, we made a number of experiments to see if it was not possible to produce a voltaic cell—a primary cell—using this idea of transference of one material through another, so as to obtain an E.M.F. and currents without any electrolysis at all. We got such a cell—a cell of mercury and magnesium—and a full account of it was given to the Royal Society. I believe it was wrong. Nobody else has suggested that, but I suggest it now. The paper was read and discussed in the usual way, I believe. I was abroad, and therefore did not take part in the discussion. The action was that of a voltaic cell unquestionably, but I am inclined to think the action we found was really produced by the temperature of the mercury being slightly different from what we supposed. We afterwards found small differences of temperature in mercury produced large E.M.F.'s, and it is probable that after distilling the mercury it had not been allowed time to properly cool. Though that particular cell was a cell in two senses of the word, still I think it made it possible to really get an E.M.F. without electrolysis, because we certainly seemed to be able to do the converse by means of the current—to pass one substance through another without any electrolysis in any sense of the word.

Mr. A. SINCLAIR [*communicated*]: Mr. Swinburne has undoubtedly brought forward a subject well worthy the notice it is likely to receive at the hands of the Institution. It is a subject Mr. Sinclair.

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Mr. Sinclair. fraught with many problems, many of which at first glance appear simple, though when taken in hand they are often almost unsurmountable.

The first point on which I find myself at variance with Mr. Swinburne is the cost per kilowatt-hour, which he places so low as 0·25d. True, a works run on the line he indicates is naturally an efficient one; still, one farthing per kilowatt seems absurdly low, for, 1 kilowatt taken as 1·34 electrical H.P., and taking an efficiency from steam to external work to be 75 per cent.—an efficiency seldom attained—the I.H.P. would be 1·8, and suppose coal to be 1·75 lbs. per I.H.P.—a very low figure—it would therefore be required, to produce 1 kilowatt-hour under the favourable circumstances which I have above indicated, 3·15 lbs. of coal, which, taken at 10s. per ton, would give 0·168d. for coal alone. Taking coal to represent one-half—a figure I have found fairly regular—the kilowatt will cost 0·336d. for simply producing, without taking into consideration depreciation, interest on capital, rent, taxation, repairs and depreciation of buildings. Taking all these things into consideration, 0·5d. would seem to be the figure nearer correct; even this I believe to be low. Again, Mr. Swinburne assumes generally that 1 kilowatt is equal to a certain amount of electrolytic work. So it is; but, unfortunately, that kilowatt contains E.M.F., a constant varying quantity, according to the materials used as electrolyte or electrodes. I have found repeatedly that it is almost impossible to obtain two sets of presumably the same material, and in presumably the same electrolyte, to give a certain current with the same electromotive force.

With regard to the production of oxygen, there seems to be a good future, though 2 volts, I think, would be found low, as the back E.M.F. generated would have little left for production of current. Again, is not some of the oxygen redissolved by the water?

In the electrolytic separation of most salts by insoluble anodes the great troubles seem to be the anode, and in most cases the porous diaphragm as well; all I have seen or heard of are subject to depreciation and decay; platinum, of course, will last,

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but its price makes it prohibited. Should a really good anode and diaphragm be discovered, the future in this branch would be unlimited. Mr. Sinclair.

The electrolytic treatment of zinc suffers very much from the effects just mentioned—insoluble anodes and high power. What is required here is a good anode, homogeneous deposit, and a means to prevent a decomposition of water and consequent loss of power.

The refining of copper by Mr. Hayden's process—which differs only from others by being connected in series, whilst they are connected in parallel—being taken as from 20s. to 25s. per ton, certainly does not seem tenable. Under the most favourable circumstances 400 kilowatt-hours are required to deposit 1 ton of copper, which, taken at $\frac{1}{2}$d. per kilowatt, would be 16s. 8d.; again, the fact of having to roll the copper to $\frac{1}{4}$ or $\frac{3}{8}$th inch thick, and then cast to shape, as many will readily see, will cost from 15s. to £1 per ton. These items in themselves come to more than the price given; probably some three to four times that figure would be the better value, and even on to that must be added the after treatment of the copper and bullion recovered.

The treatment of scrap tin seems to always fail on account of the impurity of the iron recovered; could pure iron be produced, a great future could be looked for in this direction.

In conclusion, I would remark that what is required is not so much what it will cost to produce any material, but the simplification of production; the cost will take care of itself by the mere fact of the modification of the method. Some of the problems required to be overcome are: the production of a really good insoluble anode, the discovery of a good diaphragm which will stand the test of time, the prevention of the formation of a back E.M.F. when insoluble anodes are used, the prevention of decomposition of water, the production of pure deposits from impure materials, the combinations and effects of such impurities on the resistance of the electrodes, and the means to reduce the resistance of electrolyte so as to minimise the quantity of power required. These problems overcome would render commercial electrolysis an accomplished fact, as it then could be applied to

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Mr. Sinclair. most processes mentioned by Mr. Swinburne and a host of others.

Mr. Swinburne. Mr. SWINBURNE: Before replying to the discussion, I must thank you for the kind and lenient criticism with which my paper has been met. The subject is out of the run of papers read before the Institution, and bristles with difficulties. We are also fortunate in attracting several chemists who are specialists in different branches of this subject; they have made most important additions to the discussion.

Alkali.—The first criticism refers to my estimate of the cost of power. My figure of one farthing per kilowatt-hour was obtained as follows:—The output of a million watts at the terminals was taken, and the combined efficiency of the engine and dynamo was taken at 85 per cent.—a figure which has been frequently obtained with much smaller outputs than these. The coal was taken at 10s. per ton, and 2 lbs. per indicated H.P. This is a result which is often reached in practice. We thus get—

Yearly Cost of a Million Watts.

Coal	£5,850
Oil, waste, and petty stores	700
Three shifts of engineers, at 35s.	273
Two shifts of three stokers, at 24s.	384
Interest on £20,000, at 3 per cent.	600
Depreciation on £20,000, at 5 per cent. ...	1,000
	£8,807

This comes to 0·241d. per kilowatt-hour. This estimate is of course close, but it is not out of the way, and I have made great allowances later on in the paper, as I have strongly urged the importance of having a very large margin.

The next point is the anodes. It seems generally admitted that carbon is eaten away, but some seem to think this is a small matter, and corresponds with the wear and tear of ordinary machinery. My answer to this is, that caustic soda and bleaching powder cost very little per ton, and that a very small wasting away of carbons will very soon run away with the profits. It takes a great deal of bubbling to produce a ton of bleaching

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powder, and the fact that this or that anode has been known to stand for a month without being eaten away to any great extent gives us no information, unless we know how much chlorine has been given off per square foot in the time. It is also by no means easy to make good electrical connections to carbon. This is not of great importance if the anodes are permanent, but if they are to be changed at intervals it means a very serious loss. Making such connections may not be troublesome in experimental plants; but when the novelty has worn off, and the electrolytic works degenerate into the barbarous state that prevails in ordinary chemical works where there are no enthusiastic inventors at hand to look after things, men will make the connections carelessly, and the chlorine will leak out and eat away the metal work. I have been told that I have said a great deal about the anodes, but have said little about the other difficulties in making electrolytic soda. This is because the anodes seem to me to be the real bar to progress. If durable anodes can be made, I believe caustic soda can be made commercially; if they cannot, I do not think we need spend time over the other minor difficulties. It has been said that we can always use platinum, and lithanode has also been suggested. There is all the difference in the world between using an anode for making a hypochlorite and for giving off chlorine. Platinum may be excellent for making bleaching fluids such as magnesium hypochlorite, but that is quite a different matter from giving off chlorine. The error arises from the old way of looking at electrolysis. It is said that nascent oxygen is evolved, and that this attacks the chloride and changes places with the chlorine, giving it off. The nascent chlorine in its turn produces hypochlorite. I have so frequently given my reasons for holding that there is no such thing as a nascent state, that I need hardly repeat them here. It is enough to say that oxidising a chloride into a hypochlorite is not the same as evolving gaseous chlorine, and that platinum will not work as an anode in a solution of common salt, unless it is alkaline, so that no chlorine is evolved.

Mr. Swinburne

It has been said that it is impossible to produce 30 per cent. caustic. I have, however, made caustic of this strength experimentally. I do not know that it can be made commercially.

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Mr Swinburne

The cathode solution was passed over salt, then electrolysed, then passed over more salt, and so on. The cost of evaporation, at 2s. per ton, was taken with relation to this 30 per cent. caustic, and was not meant to apply to 4 per cent. solutions. As my apparatus was very rough, it seems to me quite possible to produce 30 per cent. caustic electrolytically. It was suggested that such strong caustic would get to the anode and be reduced. It would not be reduced at the anode; it would be converted into hypochlorite. There is no reason to have such a strong solution anywhere near the anode. In the experiment mentioned, the cathode solution was strong, but the anode solution was weak.

My contention as regards electrolytic soda and bleaching powder is that the anodes are the most important difficulty, and that until anodes are produced which will last there is no use paying any attention to the other difficulties.

Zinc.—Mr. Swan complains that I have not mentioned Burghardt's process. It is mentioned on page 517, but electrolysing an alkaline solution of zinc seemed to me so obvious that I did not think of attaching anyone's name to it as an inventor. Mr. Rawson's statement that treeing of zinc can be prevented by the use of a burnisher is of the highest importance, both with regard to zinc and lead.

Copper.—The reaction given as the basis of the Höpfner process still seems to me most strange. That cupric chloride should convert a low into a high sulphide, thus taking up some of the copper, seems possible; but that it should attack a high sulphide and give off sulphur seems to me somewhat extraordinary. Of course cuprous salt gives a double allowance of copper when electrolysed, as far as the quantity of electricity is concerned; but it does not follow that this is an advantage, as the resistance of the solution may be so great as to compensate for it. I do not know the specific resistance of the double chloride of copper and sodium.

With regard to the refining of copper, it is satisfactory to hear from Mr. Mavor that this trade is developing so rapidly. I was aware that certain copper works are refining extensively, and trust they may find the business as lucrative as they

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deserve; but as a general rule industrial chemists do not treat electro-metallurgy seriously. Mr. Sinclair seems to think my figures as regards the Hayden process are wrong. I can only say that they were taken from actual practice. The books of the Copper Company were examined by Mr. Hays, a chartered accountant, and there is no room for error in this case. Mr Swinburne.

It may seem strange to some that I have not mentioned Mr. Swan's process for rapid deposition: his paper was not published at the time mine went to press. It must be remembered that this paper was really written during the spring.

Tanning.—Mr. Falkenstein says that tannin is a colloid, and then goes on to say that electrical tanning depends on electrical osmosis. This seems to me to be a contradiction. Mr. Trotter says there is no theory of electric tanning; but I have given a theory, and he does not criticise it. Of course my theory does not go into the theory of the process known as electric osmosis, any more than in the rest of the paper I go into the latest theories regarding the nature of electrolysis. That electrolysis produces a slow displacement of the radicles throughout the electrolyte is well known; and this, even if the motion is excessively slow, accounts for the effects in electrical tanning. The discussion of the movements of the radicles and the causes of these movements is a question of theoretical electrolysis, and not specially of electrical tanning.

Ozone.—Mr. Andreoli has urged that the production of ozone by high-pressure discharge is not electrolysis. I can only say that, as far as we know, the process is more nearly analogous to electrolysis than to anything else. It seemed, at any rate, sufficiently within the scope of my subject to be discussed.

Bleaching.—The information we have obtained on this subject seems particularly important. The lithanode plates shown, which have withstood several years' constant work without any apparent destruction, show that at any rate we are in possession of anodes which are suitable for producing hypochlorites. It seems to be generally assumed that making hypochlorites is on the same footing as electrolytic soda and bleaching, apart from the question of anodes. It must be remembered, however, that

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Mr. Swinburne.

in the case of hypochlorites we only get half the product. In one case we get bleach and soda, in the other bleach only, with approximately the same expenditure of power. Another point that is often overlooked is that the various anodes take different electro-motive forces. Mr. Cross refers to the production of an oxidising body at the cathodes. This seems an odd statement. One can understand a reducing agent produced at the anodes, as such bodies as peroxide of hydrogen act as reducing agents in certain cases; but the production of an oxidising body at the cathodes seems impossible. I can quite agree with Mr. Cross that it is possible that magnesium hypochlorite may be more useful than the hypochlorite of a stronger base, as it may not have enough oxidising power to waste itself by attacking bodies which would reduce such hypochlorites as that of sodium. Magnesium hypochlorite may thus easily devote itself entirely to the useful occupation of bleaching, without wasting its energies on the oxidisation of useless materials. That electrolytically prepared hypochlorite should bleach better than hypochlorite prepared chemically, seems, I must say, highly improbable.

Electro-Plating.—In answer to Professor Thompson and Mr. Lloyd, I designedly left electro-plating out of account, as the subject is too large. I could not find any title that would cover the subjects with which I intended to deal, and would exclude plating.

Miscellaneous.—With regard to the evolution of CO_2 from carbons, I do not think there is the least chance of being able to use carbon as a positive element in an aqueous solution. The mere fact that it serves so excellently as a negative element is enough to show that it will not do instead of zinc. Mr. Willard E. Case has made some experiments on this subject, but they do not seem conclusive either way. (See *Electrician*, January 20th, 1888.) The question as to whether a fluid alloy is an electrolyte has also been investigated by Professor Chandler Roberts.

The PRESIDENT: I have pleasure in moving a vote of thanks to Mr. Swinburne for his very interesting paper, which has given rise to two evenings' interesting discussion.

The motion was carried unanimously.

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The PRESIDENT: I have also to announce that, owing to the death of Mr. Edward Graves, it has been found necessary to appoint a temporary Honorary Treasurer until the Annual General Meeting, and Sir David Salomons having been good enough to consent to act in that capacity, he has been appointed by the Council accordingly.

A ballot for new members took place, at which the following candidates were elected:—

Foreign Members:

C. E. L. Brown.
M. A. van der Sprong.

Associates:

Harold A. Clegg.
William Corkhill.
Edward Henry Draper.
Edward Somerset Eccles.
Arthur H. M. Francis.
Charles Jones.
Francis Hastings Medhurst.
Thomas Oddy.
Edward T. Parker.
H. Faraday Proctor.
William Sillery.
John Edwin Tongue.
Howard B. Trotter.
Archibald Wilson.
Herbert Wooler.

Students:

John George Burchell.
Frank Fairley.
Frederic Digby Latimer.
Hugh Bernie Player.
Charles Henry Stobart.
Charles Barnard Wigg.

The meeting then adjourned.

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The Two Hundred and Forty-third Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 24th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 17th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council:—

From the class of Associates to that of Members—
Edmund P. Jackson.

Mr. R. A. Smith and Mr. J. O. Girdlestone were appointed scrutineers of the ballot.

Donations to the Library were announced as having been received since the last meeting from the Astronomer Royal; the Director-Gèneral of Telegraphs in India; Mr. John Aylmer, Local Honorary Secretary in Paris; Dr. John Hopkinson, F.R.S., Past-President; and Mr. Perrin Maycock, Member; to whom the thanks of the meeting were accorded.

The following paper was then read:—

EXPERIMENTAL RESEARCHES ON ALTERNATE-CURRENT TRANSFORMERS.

By J. A. FLEMING, M.A., D.Sc., F.R.S., Member.

1. So much has been already written on the theory and performance of real and imaginary transformers, that some excuse might seem necessary for venturing to add to the literature of the subject; and yet, when we come to review that which is

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absolutely accepted as settled truth with regard to transformers, we find acknowledged authorities were no long time ago, and perhaps are now, in entire disagreement with each other on matters which seem vital and fundamental. At the time when the investigations described in the following paper were commenced, the matters in high dispute involved such practical questions as the relative superiority or advantages of open and closed magnetic circuits, the relative magnitude of the core losses at full and at no load, and the most trustworthy experimental methods of determining these transformer losses. During the many months that these experiments have been in progress, important contributions have been made to the solution of these questions in papers by Dr. Hopkinson, Professor Ayrton, and Dr. Sumpner. Applying their own particular methods, they have reached conclusions on some of these points which it will be seen are similar to those arrived at in the following pages. As my own experiments, which were almost complete before these papers appeared, have been made with apparatus and instruments obtainable in most electrical workshops, and have not been limited to one or two special transformers, but have been made with stock sizes of many different kinds and types of transformers, it seemed likely that they might have sufficient independent value to justify me in bringing them before you.

These experiments had their origin partly in my own desire for definite and trustworthy facts as to actual transformers in use, and partly arose out of an invitation of Mr. Swinburne to examine his "hedgehog" transformer, and compare its performance with that of some closed magnetic circuit transformer of equal size. They have, however, gone beyond that last-mentioned aim, and it forms no part of my purpose to draw comparisons between transformers from a commercial point of view, but simply to record facts of observation and leave them to speak for themselves. As a matter of fact, it is a very difficult thing to institute exact comparisons between commercial transformers as made by different makers. In the first place, they take different stock sizes, and they design their instruments for different frequencies. Hence, if we procure stock sizes for experiment, they are not directly comparable. In

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the second place, if special sizes of equal output are specially made, the maker cannot generally do himself justice unless he has *carte blanche* as to price, and from a commercial point of view price is everything; and, lastly, as such experiments cannot be done all in a minute, but generally take many months to complete, the experimentalist finds himself often, when all is said and done, met by the progressive manufacturer with the placid remark, "Yes, but the transformer you have been testing is our *old* "pattern; you have not seen the one we are making now." One of the objects of this investigation was to arrive at some definite certainty on the best practical method of quickly testing transformers in actual use and *in situ*. The particular methods employed by Dr. Hopkinson and Professors Ayrton and Sumpner involve the possession of two transformers of equal size. These methods are in principle an extension to transformers of Dr. Hopkinson's method of measuring the efficiency of dynamos, and they are based upon the obviously right method of measuring directly the quantity we desire to know—viz., the power lost in the transformer—rather than measuring it as the difference of two very much larger quantities—viz., the power going in, and the power coming out. These methods, as well as others which depend upon the graphic description of the current and electromotive force curves, have the disadvantage that it is often impossible to apply them, because only one transformer is at disposal, or because the observer has not access to the dynamo, and accordingly the first matter which engaged attention was an examination of the degree of accuracy to be reached by the simple direct methods which can be applied under such conditions as can generally be obtained in practice and when using the commercial instruments to be found or obtained in every testing laboratory.

I.—Methods of Measurement Employed.

2. In order that an opinion may be formed as to the confidence to be placed in the quantitative experiments described, it may be well to give, in the first place, some details as to the methods adopted for measuring the potential differences, currents, &c., and processes of calibrating the instruments used. These

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instruments were, then, employed in measuring the power taken up by transformers with open, or loaded, secondary circuits. As single transformers only of each type were at disposal, we entered on a preliminary investigation on the degree of accuracy to be obtained when using such methods as the three-voltmeter, the three-ammeter, and the two-circuit electro-dynamometer watt-meter of various constructions.

In order to avoid tedious repetitions, it may be observed once for all that when mention is made of "the current," or "the "potential difference" of such and such an amount, the *effective* value is meant, or the square root of the mean of the squares of the equidistant instantaneous values, unless otherwise stated. The letters P.D. are used as an abbreviation for *effective* potential difference.

Mr. Swinburne placed at my disposal four of his electrostatic torsional voltmeters, and also two of his dynamometer watt-meters, and there were also at hand a sufficient supply of Kelvin current balances and electrostatic voltmeters and other appliances for the calibration of the above instruments. Since these Swinburne electrostatic voltmeters proved useful in much of the work, it may be advantageous to point out their good and bad points, and also to say how they were improved, calibrated, and used in these experiments. As they have been sufficiently described by their inventor, suffice it to say that they consist of two tiers of semi-cylindrical metal boxes, B (see Fig. 1), insulated from each other. A vertical metallic axis carries two semicircular aluminium "needles," set at different levels, so that one is enclosed in the upper pair of boxes and the other in the lower. This needle is strung at the top and bottom by a round phosphor-bronze wire 0·003 or 0·004 inch diameter, and the top end of the upper wire is held by a torsion head, T, the indicating needle of which moves over a graduated scale. In most of the experiments these voltmeters were used as idiostatic voltmeters, and the "needle" and one pair of semi-cylinders formed one terminal of the instrument, and the other pair of semi-cylindrical boxes the other terminal. When a definite potential difference is made between the two terminals, the needle is displaced from its normal

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position, and the semicircular needles attracted into the semi-cylindrical enclosing boxes, which are at a different potential. The upper torsion head is then turned until the needle comes back to its initial position. This is accurately determined by affixing a mirror to the axis of the electrometer needle. The image of the filament of an incandescent lamp is then thrown on the mirror by a lens, and, after reflection, received on a paper scale about one metre away. By a simple reference point on this scale it is easy to bring back the line of light accurately to its original position. In first working with these instruments we were greatly troubled by electrical discharges passing across from the "needle" to the cylinders and destroying the suspending wire by the resulting current. The simple remedy for this was found in enclosing each semicircular aluminium disc of the "needle" between two thin sheets of mica which projected slightly over the edge of the sheet of metal. The instrumental utility of the voltmeter was not at all impaired, whilst at the same time discharges between the needle and enclosing boxes were prevented, and much labour thereby saved. The sensitiveness of the voltmeter was such that when used to read an alternating potential of 2,400 volts, a difference of 3 or 4 volts was easily detected, and with sufficiently steady potential there was no difficulty in taking the reading of about this number of volts to one-quarter per cent. accuracy. The great defect of the instrument as it stands is the large moment of inertia of the needle, and this, in the absence of any effective damping arrangement, creates a great waste of time in getting readings.

The instruments were provided with an electro-magnet intended to be used for creating a magnetic damping field. This, however, proved to be ineffective. A far better plan when such a heavy needle is employed is to damp out the oscillations by a suitable mica vane dipping in oil or water.

3. In calibrating these voltmeters, the first step was to prepare a standard inductionless resistance capable of carrying, without sensible heating, a current of at least a quarter of an ampere, and which could be subjected to an alternating pressure of 2,000 to 3,000 volts without risk of short-circuiting. This was formed by

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winding double silk-covered platinoid wire (No. 36 B.W.G.) round a wooden frame. The wire windings were each kept separate by being wound in serrations, or teeth, cut in the edge of varnished slate slips fixed at the top and bottom of the frame. The frames were about 37 inches long and 14 inches wide, and the wire made 56 turns round the frame, each turn of wire being separated by about a quarter of an inch from its neighbours. The length of wire on each frame was about 126 yards, measuring some 1,600 ohms. Six of these frames were provided, giving us, therefore, about 9,600 ohms resistance in the form of a wire perfectly insulated in every part and between every turn. This wire was capable of carrying about one-quarter of an ampere without any very sensible heating, and about three-quarters of an ampere without exceeding safe limits. Every part of the wire was accessible, and exposed to the cooling action of the air. The six frames were fastened up to the ceiling of the laboratory and arranged so that the direction of the wire windings was alternately right and left. The resistance of all these coils was measured when at 15° C. and found to be as follows :—

Coil 1	1,607·2	ohms.
„ 2	1,505·5	„
„ 3	1,525·5	„
„ 4	1,649·0	„
„ 5	1,610·1	„
„ 6	1,645·7	„
Total ...	9,543·0	„

On coil 1 two terminals were placed, one at a distance of 394·7 ohms from the end, and one at a distance of 1212·5 ohms from the end. By taking leading wires from these points it was possible to divide the whole resistance of 9,543 ohms into two sections, having in one case resistances in the ratio of 23·175 to 1, and in the other case a ratio of 6·87 to 1. This divided resistance was a practically non-inductive resistance of about 10,000 ohms, divisible into two sections in the ratio of 23 to 1 or 7 to 1. If a measured alternating pressure of 2,400 volts was

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applied to the ends of this resistance, the wire came to a steady temperature of about 20° C. to 25° C. above the atmospheric temperature (15° C.), as nearly as could be judged by a thermometer. Taking the coefficient of change of resistance with temperature for platinoid as equal to 0·021 per cent. per degree, it will be found that at 40° C. the whole coil would have a resistance of 9,591 ohms, and hence, if truly non-inductive, the current through the coil would then have a value of $\frac{2,400}{9,591}$ ampere, = 0·250 ampere. The actual current, carefully measured by a Kelvin deciampere balance, was found to be exactly 0·250 ampere, thus showing that the resistance was, for all practical purposes, non-inductive.

In all subsequent experiments it was therefore taken that when an alternating pressure of 2,400 volts was applied to this resistance, it passed a current of 0·25 ampere and absorbed 600 watts. This resistance will be hereafter called the "No. 1" resistance for reference. By the aid of this divided resistance the Swinburne voltmeters were easily calibrated.

A 24 to 1 transformer, T (see Fig. 2) was connected to 100-volt mains through a rheostat, R, so as to raise the pressure and enable it to be regulated within limits. The above standard divided resistance was placed across the high-tension terminals, and a Kelvin multicellular electrostatic voltmeter, Th, and a Swinburne electrostatic voltmeter, S, connected respectively to the small section and to the whole length of the resistance, as shown in Fig. 2.

By taking the readings of the Kelvin electrostatic voltmeter corresponding to various pressures and multiplying these readings by 24·175 we obtained values for the P.D. at the terminals of the Swinburne voltmeter, and hence the true values of the instrumental readings of the last-named instrument. For each Swinburne voltmeter a calibration curve was then prepared in which the horizontal abscissæ were the torsion-head readings, and the corresponding ordinates the true P.D. in volts at the terminals of the instrument. This curve (see Fig. 3) was found to be a very exact parabola. Having proved this to be the case by many experiments for each of the four Swinburne voltmeters, we

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subsequently adopted for each voltmeter an instrumental constant, the product of which by the square root of the torsion-head reading gave the terminal P.D. on the instrument. These constants were determined for each instrument frequently, as required.

In addition to the above standard resistance, another set of six similar coils was provided, each coil of which consisted of No. 32 B.W.G. platinoid wire wound on a frame non-inductively. Each of these coils had a resistance of about 500 ohms, and each coil could carry comfortably, without any very sensible heating, about half an ampere. These last six coils were specially intended to be used for current measurement, as follows:—The coils were arranged either two, four, or all six in parallel, or one or more in series, and the current to be measured passed through them. The P.D. at the ends of the compound resistance was measured by a Kelvin electrostatic voltmeter. The arrangement was standardised as an ampere-meter by passing known currents through the coils, measured by a Kelvin ampere balance, and then observing the P.D. at the ends of the compound resistance. In this way the true resistance of the coils grouped in various ways when carrying currents was found, and at any subsequent time the mere reading of the P.D. at the ends of the resistance gave the current passing through the coils. The reason for employing an electrostatic voltmeter and resistance in this way to measure current was that we thereby obtained an inspectional ampere-meter much more convenient in many experiments than an ampere balance or dynamometer. It will be seen, therefore, that all the measurements were ultimately referred to one Kelvin electrostatic voltmeter and one Kelvin ampere balance. These instruments were found to agree with one another. Any other instruments, such as Siemens's dynamometers or an Ayrton and Perry twisted strip ammeter, sometimes used, were standardised and referred to the above-mentioned Kelvin balance, which was one specially constructed for alternating-current measurement.

This last-mentioned set of six resistance coils will be hereafter spoken of as the "No. 2" set. We frequently employed the No. 1 and No. 2 resistances in series, giving us a practically non-induc-

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tive resistance of about 12,000 ohms. The No. 1 resistance was also employed as an absorber of power. For if the six coils, each of about 1,600 ohms, were arranged three in parallel and two in series, we obtained an effective resistance of about 1,100 ohms, and this, when placed across a 2,400-volt circuit, passed a total current of rather more than 2 amperes, and absorbed about 5,000 watts. We found it very convenient to employ this resistance in this manner as a standard non-inductive power-absorber for obtaining the constant of various forms of wattmeter. Although using the No. 1 resistance in this way to absorb nearly 7 horse-power, the current passing in each single wire was not more than three-quarters of an ampere, or within the safe limits.

II.—Experiments with the Three-Voltmeter Method of Measuring Alternating-Current Power.

5. Having the necessary instruments all calibrated and compared, a series of experiments was made with three or four of the Swinburne voltmeters, using them in accordance with the well-known method, for measuring the power taken up in transformers on open secondary circuit.

Two 5-H.P. transformers were arranged with low-tension coils in parallel on a 100-volt circuit, and high-tension coils (2,400 volts) in series (see Fig. 4). The current through the low-pressure coils could be varied by a rheostat, *f*, and hence the P.D. at the high-tension terminals of the transformers given any desired value from 4,800 volts downwards.

In the diagram (Fig. 4) T is a transformer in which the power taken up is to be measured; T_1 and T_2 are the 5-H.P. pressure transformers just mentioned. The torsion voltmeters, V_1, V_2, V_3, were arranged as shown; V_1 being across the terminals of the transformer to be measured, V_2 being across a non-inductive resistance, r_1, put in series with the primary coil of the transformer T, and V_3 used to measure the total P.D. between the outside terminals of the two pressure-producing transformers, T_1 and T_2. Since T_1 and T_2 were identical transformers, it was generally quite sufficient to measure the P.D. between the terminals of one transformer, T_1 or T_2, and to take double this

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P.D. as the value of V_3. Sometimes two voltmeters were used, one across the terminals of each transformer. Under these circumstances, as is well known, the power, W, taken up in the transformer, T, under test is given by the formula,

$$W = \frac{1}{2 r_1} \left\{ V_3^2 - V_2^2 - V_1^2 \right\}.$$

In applying this method we had several difficulties to overcome before we could get any satisfactory results. In the first place, the transformer to be tested must have the potential difference, V_1, between its primary terminals always exactly maintained at the normal pressure, which, in all our experiments, was 2,400 volts. In the second place, the potential difference V_2 must always be made as nearly equal to V_1 as possible, in order to obtain the condition of greatest probable accuracy of measurement—that is, to obtain the condition in which given small percentages of error in the several voltmeter readings produce the minimum percentage error in the calculated power. In the third place, the resistance r_1 must be practically non-inductive, and must be capable of being varied in amount as required to fulfil the condition of making V_2 equal to V_1. We met these conditions in the following manner:—In cases in which the transformer T was not loaded on the secondary circuit, and in which, therefore, the current flowing through its primary circuit was not greater than could be comfortably carried by the No. 1 10,000-ohm standard resistance, we employed that resistance, arranging two or more coils in series. In series with this we placed some or all of the coils of the second resistance, arranged in series or in parallel, and we placed a Kelvin voltmeter to read the fall in volts down one of these last coils. In other cases, when the transformer T was loaded up on its secondary circuit, and when, therefore, a larger current was flowing through its primary circuit, we employed a series of 24 50-C.P. 100-volt incandescence lamps, or several series of lamps in parallel or series, adjusted as required to obtain the requisite resistance. When these lamp resistances were used, the second (No. 2) set of resistance coils, arranged with 2, 4, or 6 in parallel, as necessary, were placed in series with the lamps. By measuring

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the fall in volts down this known wire resistance by a Kelvin electrostatic voltmeter, *th*, and the fall in volts over the lamps and resistance taken together by means of one of the Swinburne voltmeters, V_2, we were able to determine at any instant the value in ohms of this non-inductive resistance from the readings of two electrostatic voltmeters and the known value of the wire resistance across the terminals of the Kelvin voltmeter.

The process of getting a reading was this: The resistance r_1 was varied suitably until the value of the P.D. V_1 was as nearly as possible equal to the value of the P.D. V_2. The feeding transformers T_1 and T_2 had a rheostat in their common low-pressure circuit, which was then varied until the P.D. V_1 came to the value 2,400 volts, and it was constantly maintained at that value. The reading of the other two voltmeters was then noted, as well as that of the Kelvin voltmeter Th. If we call A the ampere value of the current flowing through the primary coil of the transformer T, and therefore also that through the resistance r_1, it is obvious that the value of r_1 is determined at once from the value of the P.D. V_2 and the value of the current A. A is, of course, known by the previous calibration of the Kelvin voltmeter and associated resistance as a non-inductive ampere-meter. Hence, if V_1 is constantly maintained at the value 2,400 volts, we see that the power, W, taken up in the transformer is given by the formula,

$$W = \frac{A}{2 V_2} \left\{ V_3^2 - (2{,}400)^2 - V_2^2 \right\}.$$

If it had been possible to make V_2 also always exactly 2,400 volts, the formula would have been still more simplified.

It will be observed that we used a row or rows of 24, or less, 50-C.P. incandescent lamps as our non-inductive resistance, and determined the actual resistance at any instant by observing the terminal P.D. of the row of lamps, and the current flowing through them as measured by the fall in volts down a small known non-inductive resistance placed in series with them. We found this to be a better method than using any form of ammeter in series with the lamps—firstly, because we were more sure of not introducing prejudicial inductance into the lamp circuit; and, secondly,

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because we found we could obtain greater accuracy in the measurement of the current than by any other ammeter suitable for the purpose. The justification for using such a row of lamps as a practically non-inductive resistance is that if an incandescent lamp is placed in series with a dynamometer suitable for measuring both alternating and continuous currents, and if the terminal P.D. on the lamp is adjusted, say, to 100 volts as measured by an electrostatic voltmeter, no sensible difference can be found in the current as measured by the dynamometer, whether that current is alternating or continuous.

It will be seen that when the transformer under test is loaded on its secondary circuit, in addition to the ammeter and voltmeter measurements necessary to estimate the secondary load, we have four, and sometimes five, other voltmeter measurements to make simultaneously in order to get one observation of the power absorbed in the transformer. In dealing with the mere theory of an experimental method, nothing is easier than to say, "Let V be "a constant P.D.;" but in actual practice, and when working off the mains of existing public electric supply companies, this ideal condition is very difficult to realise, and the observers have their patience strained often almost up to the safe working limits to obtain any results worth having at all. We found that the multiplicity of readings to take simultaneously constituted a very great drawback to the utility of this voltmeter method. Having the apparatus, however, set up, and the instruments all calibrated, the method was applied to measure the power absorbed in a number of transformers with open secondary circuits, and in one or two cases the power taken up in the transformer corresponding to various secondary loads was also taken.

6. The results of some of these observations are given in the fourth column of Table I., in which the determinations of the power-absorption of various transformers at no load obtained by different methods are collected. In this table the first column gives the maximum output in watts of the transformer; the second gives the "magnetising current," or primary current, at no load; the third the primary volts, or P.D. at the primary terminals; the fourth, fifth, sixth, and seventh columns give various power

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determinations; the eighth gives the "apparent watts" absorbed at no load, or the numerical product of magnetising current and primary volts; and the ninth column gives a number which it is convenient to call the *power-factor* of the transformer at no load—it is the ratio of the *true* to the *apparent* watts. If the currents and pressures were simple sine functions, then the power-factor in that case would be the cosine of the angle of lag of primary current behind the primary terminal potential difference.

Table I.

Power absorbed by various Transformers with Open Secondary Circuits at a Frequency of 83 $\frown\!\!\!\smile$.

	1	2	3	4	5	6	7	8	9
Transformer.	Maximum Output in Watts from Secondary.	Magnetising Current in Amperes.	Primary Volts.	Power in Watts absorbed at No Load.					
				By Voltmeter Method.	By Ammeter Method.	By Dynamometer Wattmeter, W.	By Swinburne Wattmeter.	Apparent Power, W'.	Power-Factor, $\frac{W}{W'}$.
Ferranti (1885 pattern)	1,875	·18	2,416	288	—	—	—	432	·66
„ (1885 pattern)	3,750	·337	2,400	540	580	549	511	808	·68
„ (1885 pattern)	7,500	·25	2,435	444	—	—	—	600	·74
„ (1885 pattern)	11,250	·34	2,447	578	—	—	—	816	·70
„ (1885 pattern)	15,000	·57	2,389	1,019	—	—	—	1,368	·75
„ (1885 pattern, re-wound)	3,750	·11	2,400	—	—	233	220	264	·88
„ (1892 pattern)	7,500	·075	2,400	—	—	138	—	180	·77
„ (1892 pattern)	11,250	·076	2,400	—	—	148	116	182	·81
„ (1892 pattern)	15,000	·112	2,400	—	—	228	195	269	·85
„ (1892 pattern, re-wound)	11,250	·103	2,400	—	—	228	—	247	·92
Swinburne, "Hedgehog"	3,000	·74	2,400	112	111	112	74	1,775	·063
„ „	6,000	1·194	2,400	—	—	156	142	2,918	·053
Westinghouse	6,500	·05	2,400	93·5	97	95	95	120	·79
Mordey-Brush*... ...	6,000	·076	2,400	126	—	140	106	182	·77
„	750	·0317	2,392	61·5	—	—	—	76	·81
Thomson-Houston ...	4,500	·083	2,400	—	—	108	83	199	·54
Kapp	4,000	·145	2,400	—	—	152	112	348	·61

* The power determination by the three-voltmeter method, viz., 126 watts, was made on one transformer, No. 1952. The power determination by the dynamometer, viz., 140 watts, is the mean of four values obtained on another transformer, No. 1968, one value of which is also 126 watts.

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In criticising these results, it must be borne in mind that several of the transformers were intended to be used at a frequency of 100, and not at 83, and that therefore comparisons between the resulting numbers are not to be instituted without regard to this circumstance. Moreover, on different days the frequency varied from 83 to 88, and this appreciably affects the power determinations. As an example of a series of voltmeter tests, we give in Table II. the details of the observations on a series of transformers at no secondary load. In these tables V_1 is the voltmeter reading of the P.D. of the primary terminals of the transformer under test; V_2 is the P.D. between the ends of the non-inductive resistance, R; V_3 is the P.D. between the terminals of the pressure-supplying transformers; and W is the power in watts being taken up in the tested transformer, calculated by the formula from the three-voltmeter readings. The frequency employed in each case is stated, and the value of R, the non-inductive resistance, is also given in each case. The column in Table I. headed "By Swinburne Wattmeter" records the power readings taken with the first wattmeter in a brass case, sent with the 3,000-watt "hedgehog" transformer. Quite lately another wattmeter, with an ebonite outer case, was sent. This will be afterwards referred to as the "second wattmeter."

Table II.

Tests of Power absorbed on Open Secondary Circuit by various Transformers, taken by the Three-Voltmeter Method.

(1.) *Westinghouse Transformer*, 6,500-watt. R = 9,678 ohms; frequency = 86·7 $\sim$; magnetising current = 0·049 amp.; power-factor, 0·79; apparent watts taken up at no load, 118.

Voltmeter Readings.			Power absorbed in Watts on Open Secondary Circuit.	
V_1.	V_2.	V_3.		
2,403	490	2,793	92·3	
2,384	480	2,777	92·7	
2,379	480	2,775	93·5	Mean = 93·5 watts.
2,384	480	2,782	94·2	
2,386	480	2,786	95·0	

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(2.) *Mordey Transformer*, 6,000-watt. R = 12,620; frequency = 82·7 ∿; magnetising current, 0·0724 amp.; power-factor, 0·72; apparent watts taken up at no load, 173·7.

Voltmeter Readings.			Power absorbed in Watts on Open Secondary Circuit.
V_1.	V_2.	V_3.	
2,419	922	3,151	128
2,420	920	3,142	125
2,410	914	3,133	126
2,406	914	3,130	126
2,402	910	3,122	125
2,410	914	3,140	127
2,406	914	3,135	127

Mean = 126 watts.

(3.) *Ferranti Transformer*, 7,500-watt (old form, 1885 pattern). R = 8,000 ohms; frequency, 87·2 ∿; magnetising current, 0·25 amp.; power-factor, 0·74; apparent watts taken up at no load, 600.

Voltmeter Readings.			Power absorbed in Watts on Open Secondary Circuit.
V_1.	V_2.	V_3.	
2,404	2,039	4,134	447
2,439	2,042	4,158	448
2,416	2,042	4,131	442
2,445	2,042	4,160	447
2,445	2,039	4,148	442
2,450	2,035	4,158	438
2,455	2,018	4,153	446

Mean = 444 watts.

NOTE.—The 1892 pattern Ferranti 7,500-watt transformer only takes up 138 watts on open secondary.

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The foregoing extracts from the laboratory note-book will be sufficient to show the nature of the observations when typical instances of closed magnetic circuit transformers were under test. All these transformers have about the same power-factor at no load. Several other transformers were tested in the same way, and the mean results are given in the fourth column of Table I.

7. The application of the three-voltmeter method to closed magnetic circuit transformers, or transformers with large power-factors at no load, showed that it was possible with care to obtain very fairly good power determinations agreeing well with one another; although the number of instruments to be read rendered it exceedingly difficult, unless the pressure was absolutely steady, to get the benefit of the full possible individual accuracy of each instrument. The moment, however, that we turned attention to transformers with small power-factor, such, for instance, as the Swinburne "hedgehog," the difficulty of obtaining very consistent results was considerably increased. The reason for this is fully explained in a paper by Professor Ayrton and Dr. Sumpner, "On the Measurement of the Power given by any Electric "Current to any Circuit" (*Proc. Roy. Soc.*, London, vol. xlix., p. 434, 1891).

Assuming currents and potentials to vary harmonically, they show that if the same percentage error, e, is made in reading each voltmeter, the percentage error in the calculated power will be

$$\sqrt{\frac{2 + 4(1 + \cos\phi)^2}{\cos^2\phi}}$$

times e, where $\cos\phi$ may be approximately taken to be the same quantity as that which we have above called the *power-factor*. Hence, if $e = 1$ and $\cos\phi = 0{\cdot}75$, the multiplier is found to be 5; but if $e = 1$ and $\cos\phi = 0{\cdot}06$, then the multiplier is 40. In other words, a uniform error of 1 per cent. in the voltmeter readings makes only 5 per cent. error in the power when operating on a closed-circuit transformer; but a 1 per cent. voltmeter error means a 40 per cent. error in the power when testing a "hedgehog." Hence, unless the very greatest constancy in the alternating pressure can be obtained, the three-voltmeter method

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will not give good results when applied to an open magnetic circuit transformer. In order to show the extent of variation of the power determinations from the mean value, when applying the three-voltmeter method to a "hedgehog" transformer on open secondary circuit, the individual values obtained on different occasions are grouped together in Tables III. and IV.

Table III.

Power in Watts absorbed by Swinburne "Hedgehog" Transformer (3,000-watt) at No Load. Taken by the three-voltmeter method. Frequency = 82·7 ∿; R = 3,297 ohms; magnetising current, 0·73 ampere; power-factor, 0·088; apparent watts taken up at no load, 1,752.

Voltmeter Readings.			Power absorbed in Watts.		Frequency = 82·7 ∿.
V_1.	V_2.	V_3.			
2,379	2,408	3,540	162	Mean value = 155.	R = 3,297 ohms.
2,380	2,412	3,542	160		
2,387	2,415	3,547	159		
2,381	2,413	3,546	163		
2,380	2,411	3,520	138		
2,394	2,423	3,547	148		
2,382	2,415	3,542	151		
2,380	2,412	3,532	150		

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Table IV.

Three-Voltmeter Readings on "Hedgehog" Transformer at No Load.—Second Set. Frequency = 86·4 ∿; magnetising current = 0·716 ampere.

Voltmeter Readings.			Power absorbed in Watts.		R.
V_1.	V_2.	V_3.			
2,400	1,219	2,798	171	Mean value = 101.	1,700 ohms.
2,400	1,206	2,762	123		1,682 „
2,400	1,200	2,723	64		1,678 „
2,400	1,200	2,736	85		1,676 „
2,400	1,200	2,733	80		1,676 „
2,400	1,200	2,736	85		1,678 „

Third Set. Frequency = 83·2 ∿; magnetising current, 0·765 ampere.

Voltmeter Readings.			Power absorbed in Watts.		R.
V_1.	V_2.	V_3.			
2,400	2,342	3,436	90	Mean value = 85.	3,069 ohms.
2,400	2,372	3,447	80		3,080 „
2,400	2,372	3,472	87		3,069 „
2,400	2,361	3,441	82		3,078 „
2,400	2,354	3,441	87		3,069 „

In the first set of observations (Table III.) R was kept constant and the values of V_1 varied slightly; in the second and third set (Table IV.) V_1 was kept absolutely constant, and R was adjusted to make this the case. In the third set the conditions for greatest accuracy were best obtained, V_1 being kept constant, and V_2 made as nearly as possible equal to V_1 by varying R as required. It will be seen that these results

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were obtained at slightly different frequencies, and that hence they are not, strictly speaking, quite comparable. But as the divergency of results due to change of frequency is quite masked by other causes of error, we need not attend to it. The mean value of the power absorbed by the open-circuit "hedgehog" in these experiments was respectively 155, 101, and 85 watts at no load. The mean of the means is 114 watts. Besides the above experiments, a number of other quite similar ones were made with the same method, which gave results as follows, viz.: 111, 100, 132, 97, 118, 118, 119, 114, 98, 95, 103, and 88 watts. The mean of all the 30 determinations made with the three-voltmeter method is 112 watts. This is, therefore, the mean result for the power in watts absorbed by the 3,000-watt "hedgehog" transformer on open secondary circuit when worked at a frequency between 83 and 88. The greatest value of the watts is 171, and the least 64; and the greatest deviation from the mean is 49 watts, or, roughly, 50 per cent. The transformer was marked by the maker to be worked at 100 periods per second. We used it at 83 to 88. It appears, therefore, that, owing to the low power-factor of this transformer, it is a bad subject for the three-voltmeter method, and that even with the greatest precautions considerable deviations from the mean may occur in making a series of power measurements in its case. Nevertheless, it will be shown later on that there is reason to believe that the mean value so obtained of the power absorbed at no load—viz., 112 watts—is not far wrong.

8. As regards the three-voltmeter method itself, considered as a practical method of measuring alternating-current power, the following conclusions were impressed upon us:—

(1.) It is only applicable in cases when a very steady alternating pressure can be obtained of the requisite amount, and it is therefore unsuitable in general for working as a workshop method, or off commercial alternating circuits.

(2.) Owing to the number of instruments to read, it is difficult to carry it out as a practically useful method without the assistance of several observers.

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(3.) It is especially difficult, if not impossible, to get good results with it when applying it to test transformers of open magnetic circuit type, and having therefore a small power-factor.

(4.) Unless all the conditions for maximum accuracy can be secured, owing to the nature of the formula for the calculation of the power, the results will not be very trustworthy.

9. In applying the method to measure the power taken up in the primary circuit of a transformer when the secondary circuit is loaded up, the non-inductive resistance R must be capable of passing the full primary current required, and yet be capable of being so adjusted that at each stage of the load the fall of volts, V_2, down this resistance is approximately equal to the pressure, V_1, on the terminals of the primary of the transformer being tested. It is essential that this last condition shall hold good, or else we do not obtain the same probability of accuracy in the calculated value of the power given to the primary circuit at all loads. It was found, therefore, that the most convenient arrangement to employ for the non-inductive resistance was one or more parallel series of incandescence lamps. We employed one series of 24 50-C.P. 100-volt lamps, and two series of 24 16-C.P. 100-volt lamps, the three series being arranged in parallel when required, and any required number out of the 24 lamps in each series taken. In order to measure the resistance at any instant of this bank of lamps, the second set of six non-inductive resistance coils was arranged in series with the lamps, and the drop in volts down this known resistance measured with the Kelvin electrostatic voltmeter. This gave us the current flowing through the lamps, and the Swinburne voltmeter gave us the P.D. over lamps and resistance. Hence, knowing the terminal P.D. and the current, we have the value of the non-inductive resistance r_1 at any instant given by the inspection of two voltmeters. In making the full tests at all loads of a transformer by this method, the power given out from the transformer was taken up on a bank of incandescent lamps, and the current and terminal volts measured by a

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Kelvin balance and Kelvin multicellular electrostatic voltmeter. The results of a double series of observations on a Ferranti 5-H.P. 1885 (old form) transformer, and those on a Swinburne 3,000-watt "hedgehog," are tabulated in Tables V., VI., VII., and VIII.

In these tables the first three columns give the volts, current, and watts output, W_2, of the secondary circuit; the fourth column gives the primary current; the fifth the value of the inductionless resistance in series with the primary; the sixth, seventh, and eighth columns give the values of the voltmeter readings, V_1, V_2, and V_3; the column headed W_1 is the calculated value of the power given to the primary circuit; the last two columns give the value of $W_1 - W_2$, or the total power in watts absorbed in the transformer, and the ratio $\frac{W_2}{W_1}$, or the efficiency, expressed as a percentage. The values of $W_1 - W_2$ are the total power in watts dissipated in the transformer, partly as copper frictional losses ($C^2 R$ losses), partly as iron losses (eddy-current and hysteresis), and in any other way, such as eddy-currents in the copper circuits by which internal work is being expended.

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Table V.—Ferranti Transformer (1885 *pattern*).

No. 747. Power = 5 H.P. Secondary volts = 100.
Frequency used, 81·7 periods per second.
Average final temperature of transformer, 212° F.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 12·50 ohms at 212° F.
Secondary circuit resistance = 0·0252 ohm at 212° F.
Results plotted in a curve in Fig. 5, Plate 1.

Secondary Circuit.			Primary Circuit.						Total Power taken up in Transformer = $W_1 - W_2$.	Efficiency, $= \frac{W_2}{W_1}$, in per cents.
Volts.	Amperes.	Watts, W_2.	Amperes.	Resistance r_1.	Observed Volts.			Watts, W_1.		
					V_1.	V_2.	$\frac{1}{2} V_3$.			
100·4	0	0	0·336	7,595	2,400	2,552	2,242	516	516	0
100·4	0	0	0·341	7,580	,,	2,585	2,242	516	516	0
100·2	0	0	0·336	7,600	,,	2,555	2,242	504	504	0
100·9	0	0	0·341	6,190	,,	2,111	2,056	540	540	0
100·8	0·57	57	0·363	6,129	,,	2,225	2,103	569	512	10·0
100·7	1·57	158	0·400	6,037	,,	2,415	2,234	693	535	22·8
100·7	2·72	273	0·443	5,988	,,	2,653	2,374	815	542	33·5
100·5	3·82	384	0·481	4,625	,,	2,225	2,187	910	536	42·2
100·4	4·87	489	0·525	4,537	,,	2,382	2,278	1,020	531	48·0
100·3	7·20	722	0·587	3,901	,,	2,290	2,266	1,222	500	59·1
100·3	8·72	874	0·661	3,821	,,	2,526	2,393	1,409	535	62·0
100·5	10·58	1,063	0·735	3,696	,,	2,276	2,278	1,584	521	67·2
100·0	12·49	1,249	0·802	3,045	,,	2,442	2,385	1,810	561	69·0
100·0	14·28	1,428	0·867	2,651	,,	2,299	2,313	1,954	526	73·0
100·0	16·00	1,600	0·936	2,285	,,	2,139	2,238	2,149	549	74·4
99·9	17·68	1,764	1·010	2,253	,,	2,276	2,313	2,321	557	76·0
99·8	19·80	1,976	1·094	1,900	,,	2,079	2,207	2,476	500	79·8
99·5	22·72	2,261	1·215	1,865	,,	2,267	2,309	2,790	529	81·1
99·5	24·82	2,470	1·298	1,851	,,	2,403	2,389	3,050	580	81·1
99·4	27·64	2,747	1·412	1,502	,,	2,121	2,242	3,284	537	83·6
99·2	30·07	2,983	1·516	1,486	,,	2,253	2,313	3,554	571	84·0

Note.—The column headed $\frac{1}{2} V_3$ gives the values of half the total pressure across the terminals of the two pressure-producing transformers.

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Table VI.—Ferranti Transformer (1885 pattern).

No. 747. Power = 5 H.P. Secondary volts = 100.
Frequency used, 84·8 periods per second.
Average final temperature of transformer, 212° F.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 12·50 ohms at 212° F.
Secondary circuit resistance = 0·0252 ohm at 212° F.

Secondary Circuit.			Primary Circuit.					Total Power taken up in Transformer $= W_1 - W_2$.	Efficiency, $\frac{W_2}{W_1}$, in per cents.
Volts.	Amperes.	Watts, W_2.	Amperes.	Resistance r_1.	Observed Volts.			Watts, W_1.	
					V_1.	V_2.	$\frac{1}{2} V_2$.		
100·2	0	0	0·332	7,512	2,400	2,494	2,221	516	516	0
100·5	0	0	0·328	7,603	,,	2,494	2,221	509	509	0
100·9	0	0	0·329	6,209	,,	2,043	2,039	588	588	0
100·9	2·10	212	0·403	6,079	,,	2,450	2,291	759	547	28·0
100·1	3·99	400	0·474	4,109	,,	1,948	2,078	839	439	43·0
100·2	6·33	634	0·554	3,776	,,	2,190	2,217	1,205	571	52·0
99·9	9·80	979	0·686	3,495	,,	2,395	2,345	1,503	524	65·0
100·0	12·65	1,265	0·795	2,700	,,	2,148	2,236	1,786	521	71·0
99·8	14·70	1,467	0·888	2,630	,,	2,338	2,329	1,989	522	74·0
99·7	17·44	1,738	1·000	2,258	,,	2,258	2,299	2,283	545	76·0
99·7	21·26	2,120	1·155	1,879	,,	2,163	2,260	2,658	538	83·0
99·6	23·07	2,298	1·236	1,870	,,	2,306	2,337	2,880	582	80·0
99·3	25·38	2,520	1·323	1,510	,,	1,996	2,181	3,075	555	82·0
99·1	26·46	2,622	1·368	1,500	,,	2,055	2,217	3,226	604	81·5
98·9	28·00	2,770	1·430	1,498	,,	2,140	2,236	3,344	574	83·0
99·0	29·53	2,924	1·493	1,505	,,	2,230	2,295	3,434	510	85·0
99·0	30·13	2,983	1·517	1,485	,,	2,253	2,325	3,630	647	82·0
98·9	31·11	3,077	1·559	1,482	,,	2,309	2,345	3,680	603	83·5
98·9	32·06	3,171	1·601	1,472	,,	2,361	2,369	3,775	604	84·0
99·0	33·41	3,308	1·659	1,290	,,	2,140	2,256	3,953	645	83·5
99·0	34·35	3,401	1·695	1,269	,,	2,181	2,291	4,066	665	84·0
98·9	35·50	3,511	1·748	1,289	,,	2,242	2,322	4,182	671	84·0
98·8	36·00	3,557	1·763	1,283	,,	2,264	2,329	4,218	661	84·4
98·8	36·61	3,617	1·796	1,276	,,	2,291	2,341	4,275	658	84·5
98·7	37·09	3,669	1·821	1,268	,,	2,312	2,361	4,414	745	84·5
98·6	37·58	3,718	1·839	1,272	,,	2,341	2,369	4,406	688	84·5
98·5	38·11	3,754	1·861	1,270	,,	2,364	2,380	4,453	699	84·5
98·5	38·57	3,800	1·875	1,274	,,	2,389	2,395	4,503	703	84·5
98·5	39·19	3,860	1·900	1,267	,,	2,407	2,406	4,580	720	84·0

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Table. VII.—Swinburne "Hedgehog" Transformer.

No. 342. Power = 3,000 watts. Secondary volts = 100.
Frequency used, 83·7 periods per second.
Average final temperature of transformer, 147° F.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 24·00 ohms at 145° F.
Secondary circuit resistance = 0·051 ohm at 145° F.

Secondary Circuit.			Primary Circuit.						Total Power taken up in Transformer $= W_1 - W_2$.	Efficiency, $\frac{W_2}{W_1}$, in per cents.
Volts.	Amperes.	Watts $= W_2$.	Amperes.	Resistance $= r_1$.	Observed Volts.			Watts $= W_1$.		
					V_1.	V_2.	V_3.			
102·0	0	0	0·738	3,101	2,400	2,289	3,424	118	118	0
102·0	0	0	0·738	3,105	,,	2,293	3,429	118	118	0
102·0	0	0	0·738	3,105	,,	2,293	3,430	119	119	0
102·0	0	0	0·738	3,106	,,	2,295	3,430	118	118	0
101·9	1·06	108	0·759	3,069	,,	2,314	3,542	232	124	46·6
101·9	2·12	216	0·763	3,069	,,	2,342	3,667	316	100	68·4
101·9	2·71	276	0·769	3,070	,,	2,361	3,733	422	146	65·5
101·8	4·20	427	0·775	3,057	,,	2,370	3,808	510	83	83·8
101·5	9·38	952	0·882	3,979	,,	2,627	4,338	1,033	81	92·0
101·0	12·65	1,278	0·969	2,938	,,	2,847	4,694	1,391	113	91·8
100·5	15·62	1,570	1·050	2,585	,,	2,714	4,658	1,657	87	94·8
100·2	19·00	1,904	1·161	2,220	,,	2,578	4,612	1,997	93	95·5
100·0	21·68	2,168	1·255	1,863	,,	2,338	4,438	2,258	90	96·0
99·0	30·10	2,980	1·578	1,470	,,	2,320	4,530	3,189	209	93·5
102·0	0	0	0·756	3,073	,,	2,323	3,444	114	114	0

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Table VIII.—Swinburne "Hedgehog" Transformer.

No. 342. Power = 3,000 watts. Secondary volts = 100.
Frequency used, 82·3 periods per second.
Average final temperature of transformer, 145° F.

Secondary Circuit.			Primary Circuit.						Total Power taken up in Transformer = $W_1 - W_2$.	Efficiency, $\frac{W_2}{W_1}$, in per cents.
Volts.	Amperes.	Watts = W_2.	Amperes.	Resistance = r_1.	Observed Volts.			Watts = W_1.		
					V_1.	V_2.	$\frac{1}{2} V_3$.			
102·1	0	0	0·760	3,060	2,400	2,326	1,716	98	98	0
101·9	0	0	0·759	3,053	,,	2,317	1,711	95	95	0
102·0	0	0	0 759	3,056	,,	2,320	1,716	103	103	0
102·0	0·57	58	0·760	3,060	,,	2,326	1,736	144	86	40·0
101·9	1·26	128	0·761	3,068	,,	2,335	1,762	197	69	65·0
101·9	1·74	177	0·769	3,043	,,	2,373	1,805	269	92	66·0
102·0	2·33	238	0·777	3,078	,,	2,391	1,830	313	75	76·0
101·9	2·91	297	0·784	3,074	,,	2,410	1,862	375	78	79·0
101·6	4·00	406	0·799	3,060	,,	2,445	1,912	472	66	86·0
101·4	5·59	567	0·834	3,085	,,	2,532	2,021	687	120	83·0
101·5	8·25	837	0·857	2,981	,,	2,555	2,119	951	114	88·0
101·4	10·58	1,073	0·915	2,966	,,	2,714	2,242	1,177	104	91·0
101·0	12·96	1,309	0·957	3,000	,,	2,871	2,370	1,411	102	93·0
100·8	15·23	1,537	1·043	2,584	,,	2,696	2,323	1,658	121	93·0
100·5	17·09	1,718	1·098	2,240	,,	2,460	2,230	1,800	82	95·5
100·5	19·29	1,938	1·169	1,899	,,	2,218	2,123	1,938	--	100·0
100·0	21·45	2,145	1·248	1,861	,,	2,323	2,238	2,382	237	90·0
99·8	23·07	2,302	1·304	1,531	,,	1,997	2,073	2,411	109	95·5
99·7	24·74	2,467	1·366	1,519	,,	2,076	2,111	2,558	91	96·5
99·5	25·77	2,564	1·408	1,511	,,	2,121	2,123	2,570	6	99·5
99·5	26·31	2,618	1·423	1,509	,,	2,145	2,152	2,702	84	97·0
99·4	26·83	2,667	1·447	1,506	,,	2,178	2,163	2,724	57	98·0
99·1	27·42	2,718	1·466	1,497	,,	2,196	2,183	2,821	103	96 0
99·0	27·85	2,758	1·485	1,498	,,	2,225	2,199	2,881	123	96·0
99·0	28·43	2,815	1·504	1,496	,,	2,250	2,207	2,898	83	97·0
98·9	28·98	2,867	1·528	1,489	,,	2,276	2,226	2,980	113	96·0
98·8	29·40	2,915	1·543	1,484	,,	2,290	2,238	3,040	121	96·0
98·8	29·93	2,957	1·566	1,481	,,	2,320	2,250	3,075	118	96·0
102·1	0	0	0·759	3,080	,,	2,338	1,716	88	88	0

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In Fig. 5 is shown a curve representing the values of $W_1 - W_2$ for the Ferranti 5-H.P. transformer, taken from the results in Tables V. and VI. The secondary output of the transformer in watts is set off on the horizontal line, and the vertical ordinates of the upper curve are the corresponding values of the "total "lost watts," or total power losses in the transformer at these various loads. The lower curve is the curve of total copper ($C^2 R$) losses, taking both primary and secondary together. The difference between the ordinates of the upper and lower curves represents the power lost in the transformer in iron losses and eddy-current copper losses, if any. It is seen that the upper curve keeps roughly parallel to the lower curve, and that therefore, as far as these observations go, there is no indication that the core losses get less as the output increases; on the other hand, they seem to get slightly greater. It is much more convenient and instructive to plot out in this way a "total loss "curve" for the transformer than to plot an efficiency curve, for if an accompanying $C^2 R$ curve is drawn, it shows us at once what is the magnitude of the total eddy-current and hysteresis losses to be accounted for. The values of $W_1 - W_2$ for the Swinburne "hedgehog" given in Tables VII. and VIII. are found when plotted to lie so irregularly that an exact determination of the total loss curve by these tables is impossible. The power-factor of the "hedgehog," even at full load, is still small, and we were not satisfied with the general nature of the results given at all loads by the three-voltmeter method for this transformer. In the case of the Ferranti 5-H.P. transformer (1885 type), the determinations of the power taken up at no secondary load were fairly consistent, being as follows for seven observations: 516, 516, 504, 540, 538, 509, 516, all taken at a primary pressure of 2,400 volts and a frequency of 82 to 85. The mean of these values is 520 watts. At a frequency of 87 and a primary voltage rather above 2,400, we found for the same quantity the values 563, 565, 568, 552, 566, 561 watts, in six different experiments. The mean of all, corresponding to a frequency of about 83, is probably close to 540 watts, as given in Table I.

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III.—Experiments with the Three-Ammeter Method of Measuring Alternating-Current Power.

10. We next directed our attention to that modification of the voltmeter method suggested by the writer, and which involves current measurements instead of pressure. Theoretically, this method requires three ammeters, A_1, A_2, A_3, arranged in connection with the transformer under test, as shown in Fig. 6. The third ammeter, A_3, measures the current flowing through an inductionless resistance put across between a pair of high-tension mains kept at the potential required for the primary circuit of the transformer to be tested. Two other ammeters, A_2 and A_1, measure the current before and behind the point of take-off of this resistance. In putting this method into practice, we found it necessary in the first place to scheme some simple method of regulating the P.D. at the terminals of the transformer under test. This was accomplished as follows:—A 10-H.P. transformer had its primary, or fine-wire, circuit inserted in series with the primary current of the transformer under test. The secondary, or thick-wire, circuit of the 10-H.P. transformer was closed through a variable resistance consisting of 15 to 20 plates of battery carbon three inches square and a quarter of an inch in thickness, pressed together more or less by a screw clamp. By suitably compressing these plates we were able to regulate with great nicety the secondary current of the 10-H.P. transformer, and hence to adjust the resultant impedance of the primary circuit of the 10-H.P. transformer. The volts at the terminals of the transformer under test could then be kept perfectly constant, irrespective of slight variations due to the dynamo. This adjustment was made by placing one of the calibrated Swinburne voltmeters across the terminals of the transformer under test, and always adjusting the carbon rheostat until the P.D. of these terminals was 2,400 volts. As a non-inductive resistance we employed a series of 50-C.P. incandescent lamps. We then found that, working under these conditions, we could dispense with the third ammeter, A_3, and make one measurement once for all of the current taken by this series of lamps at 2,400 volts. We found that this current remained

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constant over great ranges of time. The experiments consisted, therefore, in measuring the primary current of the transformer under test by two ammeters in series, from the junction point of which was led off the constant current for a series of lamps. The P.D. at the ends of this series of lamps was kept perfectly constant at 2,400 volts, as shown by the voltmeter, V (Fig. 6). We found considerable difficulty in getting two suitable ammeters for the positions A_1 and A_2 which would agree with one another with the requisite accuracy, and finally we reduced the method to the use of a single ammeter, as follows:—An ammeter was put in the primary circuit of the transformer under test, and the P.D. at the terminals of this transformer was kept constant at 2,400 volts. A series of incandescence lamps (generally 24 100-volt 16-C.P. lamps) was joined across the primary mains, and two readings of the ammeter taken—a first when the row of lamps was between the ammeter and transformer, and a second when the ammeter was between the lamps and transformer. An independent determination was made of the current taken by the lamps. These changes of position of the ammeter were made by mercury cup connections, so as to avoid at any time actually breaking the primary circuit. As an ammeter we employed first an Ayrton and Perry twisted strip ammeter, and finally a Siemens dynamometer of low resistance. It will be easily seen that this single-ammeter method possesses all the advantages, whatever they are, of the three-ammeter method, and is much simpler to work.

If A_1 is the reading of the ammeter when it is between the transformer and the lamps, and A_2 the reading when the lamps are between it and the transformer, and A_3 is the value of the current through the lamps, and V the P.D. at the terminals of the series of lamps, it is easy to show that the power, W, taken up in the transformer is given by the expression

$$W = \frac{V}{2A_3}\left\{ A_2^2 - A_1^2 - A_3 \right\}.$$

In order to test the method, we applied it to measure the power taken up in the practically inductionless resistance coil (No. 1 standard). The resistance of this coil when traversed by about a

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quarter of an ampere is 9,590 ohms. The value of the currents were in this case $A_2 = 0{\cdot}88$, $A_1 = 0{\cdot}24$, $A_3 = 0{\cdot}65$, and $V = 2{,}400$.

Hence the power taken up, as calculated from the three-ammeter observations, is 544 watts; whilst the power absorbed, as reckoned from the known value of the resistance and current going through it, is 552 watts. Applying it to measure the power absorbed in two transformers already tested by the voltmeter method—viz., the 5-H.P. Ferranti and the 6,500-watt Westinghouse—when on open secondary circuit, the number obtained by the ammeter method was 580 watts for the Ferranti and 97 watts for the Westinghouse. These values are rather higher than the mean values obtained by the voltmeter method, viz., 540 and 93·5 watts, and, in spite of its greater simplicity, the one-ammeter method did not give very satisfactory results, even on closed magnetic circuit transformers. It was next applied to test the Swinburne 3,000-watt "hedgehog" when on open circuit. It was found here that, as in the case of the three-voltmeter method, still greater difficulty was experienced in getting experimental results which agreed numerically well together.

It is not worth while to give all the details of the observations made on the 3,000-watt "hedgehog" transformer by this one-ammeter method. Suffice it to say that, using sometimes an Ayrton and Perry twisted strip ammeter carefully calibrated, and sometimes a Siemens dynamometer, both adapted for measuring from 0·02 to 4 amperes, we obtained such figures as these for the power in watts taken up at no load in the transformer, viz.: 116, 116, 183, 153, 100, 164, 176, 110, 124, 200, 113, 71, 79, 79, 47, 108, 87, 75, 103, 88, 100, 80, 88, in 23 different determinations. Curious to say, the arithmetic mean of all these values is 111, or almost exactly the same as the mean value obtained from the 30 observations by the three-voltmeter method; the greatest deviations from the mean being about the same percentage. The conclusions arrived at with respect to this ammeter method is, that ordinary commercial ammeters do not enable us to determine the currents with anything like the accuracy necessary when we are operating on open magnetic circuit transformers at no load. It would certainly be necessary to be able to read

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currents of magnitudes from 0·25 to 5 amperes to the third decimal place, and that is not in general a practicable matter by commercial instruments. After many further trials of this method on different transformers, we were obliged to come to the same conclusions with respect to it as those forced upon us in regard to the voltmeter methods—viz., that it is an unsuitable one for transformers of small power-factor, but that with great care and a suitable ammeter it may be made to yield fairly good results for closed magnetic circuit transformers of large power-factor. It may be noted in passing that in applying this ammeter method in any case the most favourable condition for accuracy is when the current through the transformer primary is equal to the current through the non-inductive shunt. Hence the shunt resistance should be decreased as the load on the secondary of the transformer is increased if we desire to obtain a uniform possibility of accuracy in the measurements of the power supplied.

The above statement can be proved by following a similar line of reasoning to that pursued by, with regard to the three-voltmeter method, Prof. Ayrton and Dr. Sumpner, in their paper on "The Measurement of the Power given by any Electric "Current to any Circuit."* For the expressions for the power in terms of the currents or of the potential differences are similar in mathematical form.

IV.—Experiments with the Dynamometer-Wattmeter Method of Measuring Alternating-Current Power.

11. Dissatisfied on the whole with the practical results and difficulties of the voltmeter and ammeter methods, attention was next turned to the employment of the dynamometer wattmeter as an instrument for the measurement of the power given to the primary circuit of transformers. There is no need to recapitulate well-known facts about the use of such an instrument for this purpose. It is well understood that a wattmeter or dynamometer with independent series and shunt coils can be used for this purpose provided that the shunt circuit has a practically zero

* See *Proc. Roy. Soc.*, London, vol. xlix., March, 1891, p. 434.

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time constant, and the only difficulty which presents itself in passing an opinion on the suitability of any particular two-circuit dynamometer for this purpose is that of knowing whether the shunt circuit really *is* practically so far free from sensible inductance or capacity as to make this the case,—and this, as our experiments show, must not be taken for granted without investigation. Two wattmeter dynamometers were provided. The first consisted of a Siemens dynamometer of the usual pattern intended to be used as an ammeter for currents from 0·2 to 4 amperes. The fixed coil consisted of a few dozen or so turns of wire, and the movable coil of four turns. We arranged the connections so that it could be used as a wattmeter; the fixed coil being the series coil, and the movable coil of four turns being put in series with 24 100-volt 50-C.P. incandescent, or also with two rows of 24 16-C.P. lamps arranged in parallel, and this arranged as a shunt circuit. Contrary to the usual practice, we had then a wattmeter in which the shunt circuit carried the larger current. This circuit was traversed by a current of from 1·8 to 2·94 amperes when placed across 2,400-volt mains, according to the group of lamps used.

This dynamometer wattmeter had then a shunt circuit consisting of the 24 series of lamp filaments plus the four turns of wire on its movable coil. It had, therefore, a very small inductance. This wattmeter was calibrated by applying it to measure the known power taken up in our standard resistance. The six coils of our No. 1 resistance, each consisting of about 1,600 ohms, were arranged, two in series and three in parallel, across 2,400-volt mains. The current through these resistances was measured by a Kelvin deciampere balance, and found to be 2·230 amperes; the P.D. between the mains was measured by the Swinburne voltmeter, and found to be 2,400 volts, and kept at that value by a regulating transformer with carbon resistance on the secondary circuit (see Fig. 7); and from these measurements the power taken up in the resistance was known. It was 5,357 watts. The dynamometer wattmeter was then arranged so as to measure the known power being absorbed by this resistance, and the constant of the wattmeter thus determined. A torsion of

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175° of the dynamometer head was found to be required when the resistance was absorbing the above power. This constant was frequently determined. The value was close to 30·6 watts per division of the scale. It will be seen, therefore, that we were employing a wattmeter which, although it absorbed considerable power in its own shunt circuit, had a range of measurement from 12,000 watts down to about 10 or 12 as a minimum visible.

We found it essential to standardise the wattmeter on the largest power-absorbing arrangement possible, so as to get the greatest possible wattmeter scale reading on a known power-absorption in determining the instrumental constant. For if a small power reading was taken, then any error in the scale reading was multiplied up in getting the watts corresponding to a larger scale reading taken from a value of the constant, and sensible errors introduced.

In addition to this instrument, Mr. Swinburne provided two of his own non-inductive wattmeters. In this last instrument the shunt circuit consisted of a small bobbin of a good many (probably a hundred or more) turns of fine wire, having a resistance of about 300 ohms. In series with this externally, and forming part of the wattmeter shunt circuit, were two doubly wound wire resistances in brass boxes; having a total resistance of about 100,000 ohms, assumed to be practically non-inductive. Provided with these wattmeters, we proceeded to make a series of tests on various transformers of the power absorbed when the transformer was doing secondary work of various loads. The results are embodied in the series of tables below. In each case the Siemens dynamometer, called hereafter the "dynamometer wattmeter," was standardised on the inductionless resistance when absorbing about 7 H.P., and then, having obtained the constant, its readings were taken when placed on the primary circuit of the transformer. The column headed "Power taken up by Swinburne Wattmeter," records the observations made with the first wattmeter he sent having an outer case of brass.

In Table IX. are given the figures of observation for the 5-H.P. (1885 type, old form) Ferranti transformer. The transformer had

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gradually increasing loads of lamps put upon its secondary circuit, and whilst the power, W_1, supplied to the primary was measured by the dynamometer wattmeter, that given up by the secondary, W_2, was measured by a Kelvin balance and electrostatic voltmeter placed on the secondary circuit. We measured also the primary current value in amperes corresponding to each definite load on the secondary circuit. We obtained, therefore, for every transformer, the volts, or P.D. across both terminals, the ampere current, and the watt output of the secondary circuit, and at the same time the ampere current and power absorbed in watts by the primary circuit; and hence the difference between W_1 and W_2, or the power lost in the transformer, is also known for each secondary load. Finally, also, the ratio of W_2 to W_1 in percentages can be calculated, and gives us the efficiency. In all these experiments the P.D. on the primary terminals was kept carefully constant at 2,400 volts, and the constant of the primary wattmeter checked at the beginning and end of each series of observations.

A series of tests of this kind was made on each one of the following 11 transformers: A Ferranti 5-H.P. (1885 type), a Ferranti 5-H.P. (after having had the primary and secondary circuits both replaced by others of double the length), two 15-H.P. (1892 type) Ferranti, a 20-H.P. (1892 type) Ferranti, a Westinghouse 6,500-watt (the same transformer used by Dr. Hopkinson in his recent tests), a Mordey-Brush 6,000-watt, a Thomson-Houston 4,500-watt, a Kapp 4,000-watt, and Swinburne 3,000-watt and 6,000-watt "hedgehog" transformers. For each transformer was determined the currents and terminal potential differences corresponding to the various stages of secondary load from no load up to the full nominal output of the transformer, and at the same time the power, W_1, given to the transformer primary circuit, and the power, W_2, taken off from the secondary circuit. The "total lost watts," $W_1 - W_2$, and the efficiency, or ratio, $\frac{W_2}{W_1}$, corresponding to each secondary load, were also calculated. From the known or measured value of the resistances of the copper circuits (taken when the

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transformer was warm) and the measured currents we could calculate the copper losses ($C^2 R$ losses) in both circuits, separately and together, to the nearest integer, omitting fractions of a watt in most cases; and from the primary volts and current we obtained the value of the "apparent watts" given to the transformer at each stage; and from the "apparent" and "true" watts given to the primary circuit we could deduce the power-factor corresponding to any secondary output. The values of the secondary terminal P.D., or volts, gave us also the "total "secondary drop" at each load, and from the resistances and currents could be found the "copper drop," or volts lost by resistance. The observations and calculations for each of the 11 transformers tested are given in full in Tables IX. to XXXI., inclusive, which sufficiently explain themselves.

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Table IX.—Ferranti Transformer (1885 pattern).

No. 747. Power, 5 H.P. Secondary volts, 100.

Frequency used, 81·6 periods per second.

Average final temperature of transformer = 212° F.

Volts on primary circuit (V_1) = 2,400 (kept constant).

Primary circuit resistance = 12·50 ohms at 212° F.

Secondary circuit resistance = 0·0252 ohm at 212° F.

Results plotted in a curve in Figs. 8 and 9, Plates 1 and 2.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		Efficiency, $\frac{W_2}{W_1}$, in per cents.
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	
100·9	0	0	2,400	0·332	549	511	549	511	0
101·0	1·00	101	,,	0·360	665	606	564	505	15·2
101·0	2·94	297	,,	0·432	853	803	556	506	34·8
100·9	4·81	485	,,	0·504	1,035	994	550	509	46·8
100·8	7·28	734	,,	0·598	1,272	1,221	538	487	57·7
100·8	9·16	923	,,	0·673	1,461	1,423	538	500	63·2
100·7	11·18	1,126	,,	0·747	1,691	1,639	565	513	66·6
100·5	13·00	1,307	,,	0·820	1,890	—	583	—	69·1
100·3	14·93	1,498	,,	0·893	2,068	—	570	—	72·4
100·2	17·89	1,793	,,	1·015	2,372	—	579	—	75·6
100·0	20·78	2,078	,,	1·132	2,677	—	599	—	77·6
100·0	23·62	2,362	,,	1·260	2,960	—	598	—	79·9
99·9	26·61	2,658	,,	1·382	3,232	—	574	—	82·2
99·8	29·33	2,927	,,	1·500	3,538	—	611	—	82·8
99·8	31·37	3,131	,,	1·584	3,724	—	593	—	84·0
99·5	33 17	3,300	,,	1·661	3,926	—	626	—	84·1
99·2	34·93	3,466	,,	1·732	4,091	—	625	—	84·8
99·1	36·93	3,661	,,	1·816	4,298	—	637	—	85·2
99·1	38·78	3,844	,,	1·890	4,480	—	636	—	85·9
101	0	0	,,	—	549	506	549	506	0

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Table X.—5-H.P. (1885 pattern) Ferranti Transformer.

Primary volts = 2,400 (kept constant).
Secondary volts at no load = 100·9.

Results plotted in curves in Figs. 10 and 11, Plate 2.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$.
			Primary, $I_1^2 \times 12·5$.	Secondary, $I_2^2 \times 0·0252$.	Total.		Real, W_1.*	Apparent, W_1'.	
0·329	0	0	1·3	0	1	0	528	790	0·68
0·403	2·10	0·087	2·1	0·1	2	0	759	966	0·79
0·474	3·99	0·166	2·8	0·4	3	0·8	839	1,136	0·74
0·554	6·33	0·263	3·8	1·1	4	0·9	1,205	1,329	0·90
0·686	9·80	0·408	5·9	2·4	8	1·0	1,503	1,648	0·91
0·795	12·65	0·527	7·9	4·1	12	0·9	1,786	1,907	0·94
0·888	14·70	0·612	9·8	5·5	15	1·1	1,989	2,130	0·93
1·000	17·44	0·726	12·5	7·7	20	1·2	2,283	2,400	0·95
1·155	21 26	0·886	16·7	11·4	28	1·2	2,658	2,774	0·96
1·236	23·07	0·961	19·2	13·5	33	1·3	2,880	2,966	0·97
1·323	25·38	1·057	21·9	16·3	38	1·6	3,075	3,179	0·97
1·368	26·46	1·102	23·4	17·7	41	1·8	3,226	3,280	0·99
1·430	28·00	1·166	25·6	19·8	45	2·0	3,344	3,433	0·97
1·493	29·53	1·230	28·0	22·0	50	1·9	3,434	3,586	0·96
1·517	30·13	1·255	28·7	22·9	52	1·9	3,630	3,640	0·99
1·559	31·11	1·298	30·4	23·5	54	2·0	3,680	3,738	0·99
1·601	32·06	1·336	32·2	26·0	58	2·0	3,775	3,846	0·98
1·659	33·41	1·393	34·4	28·2	63	1·9	3,953	3,979	0·99
1·695	34·35	1·431	36·0	29·7	66	1·9	4,066	4,063	1·00
1·743	35·50	1·480	38·1	31·8	70	2·0	4,182	4,187	1·00
1·763	36·00	1·500	38·9	32·6	72	2·1	4,218	4,240	1·00
1·796	36·61	1·528	40·5	33·9	74	2·1	4,275	4,304	0·99
1·821	37·09	1·545	41·6	34·8	76	2·2	4,406	4,366	1·00
1·839	37·58	1·567	42·3	35·6	78	2·3	4,414	4,404	1·00
1·861	38·11	1·588	43·4	36·6	80	2·4	4,453	4,461	1·00
1·875	38·57	1·608	44·0	37·5	82	2·4	4,503	4,500	1·00
1·900	39·19	1·633	45·2	38 7	84	2·4	4,580	4,555	1·00

* These primary watts are determined from a different set of observations to those recorded in Table IX., but substantially agree with them.

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Table XI.—Ferranti Transformer (1885 pattern, re-wound).

No. 539. Power, 5 H.P. Secondary volts, 100.
Frequency used, 81·6 periods per second.
Average final temperature of transformer, 150° F.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 29·80 ohms at 150° F.
Secondary circuit resistance = 0·057 ohm at 150° F.

Results plotted in curves in Figs. 8 and 12, Plates 1 and 2.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		Efficiency, $\frac{W_2}{W_1}$, in per cents.
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	
102·7	0	0	2,400	0·11	235	225	235	225	0
102 7	1·02	105	,,	0·14	318	305	213	200	33·0
102·4	2·99	306	,,	0·22	534	501	228	195	57·3
102·1	4 86	496	,,	0·29	732	682	236	186	67·8
102·1	6·08	621	,,	0·34	870	817	249	196	71·5
101·9	9 16	934	,,	0·47	1,182	1,139	248	205	79·0
101·7	11·09	1,125	,,	0 555	1,376	1,334	251	209	81·9
101·5	13·07	1,327	,,	0·638	1,578	1,527	251	200	84·1
101·2	15·03	1,521	,,	0·713	1,771	—	250	—	86·0
101·0	16·97	1,714	,,	0·799	1,960	—	246	—	87·5
100·8	18·89	1,904	,,	0·880	2,176	—	272	—	87·6
100·6	20·97	2,109	,,	0·960	2,380	—	271	—	88·7
100·4	22·56	2,265	,,	1·036	2,531	—	266	—	89·6
100·0	24·60	2,460	,,	1·112	2,746	—	286	—	89·6
99·7	26·46	2,638	,,	1·192	2,946	—	308	—	89·6
99·5	28·35	2,821	,,	1·265	3,152	—	331	—	89·6
99·3	30·40	3,019	,,	1·350	3,308	—	289	—	91·2
99·1	32·06	3,177	,,	1·429	3,529	—	352	—	90·0
99·0	33·88	3,354	,,	1·504	3,703	—	349	—	90·6
98·8	35·78	3,535	,,	1·582	3,904	—	369	—	90·6
98·6	37·58	3,706	,,	1·669	4,088	—	382	—	90·8
98·4	39·65	3,902	,,	1·741	4,301	—	399	—	90 7
102·9	0	0	,,	0·08	229	209	229	209	0

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Table XII.—5-H.P. (1885 *pattern, re-wound*) *Ferranti Transformer.*

Primary volts = 2,400 (kept constant).

Secondary volts at no load = 102·7.

Results plotted in curves in Figs. 13 and 14, Plate 2.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$.
			Primary, $I_1^2 \times 29{\cdot}8$.	Secondary, $I_2^2 \times 0{\cdot}067$.	Total.		Real, W_1.	Apparent W_1'.	
0·11	0	0	0	0	0	0	235	264	0·98
0·14	1 02	0·043	0·6	0·2	1	0	318	336	0·95
0·22	2·99	0·166	1·4	0·5	2	0·3	534	528	1·01
0·29	4·86	0·202	2·5	1·3	4	0·6	732	696	1·05
0·34	6·08	0·254	3·5	2·1	6	0·6	870	816	1·06
0·47	9·16	0·382	6·6	5·9	12	0·8	1,182	1,128	1·05
0·555	11·09	0·496	9·2	7·0	16	1·0	1,376	1,332	1·03
0·638	13·07	0·544	12·2	9·7	22	1·2	1,576	1,531	1·02
0·713	15·03	0·626	15·2	12·9	28	1·5	1,771	1,711	1·03
0·799	16·97	0·707	19·0	16·4	35	1·7	1,960	1,917	1·02
0·880	18·89	0·787	23·1	20·3	43	1·9	2,176	2,112	1·03
0·960	20·97	0·874	27·4	25·0	52	2·1	2,380	2,304	1·03
1·036	22·56	0·940	32·0	28·2	61	2·3	2,531	2,486	1·02
1·112	24·60	1·025	37·0	34·5	72	2·7	2,746	2,669	1·03
1·192	26·46	1·102	42·3	39·5	82	3·0	2,946	2,861	1·03
1·265	28·35	1·181	47·7	45·5	93	3·2	3,152	3,026	1·04
1·350	30·40	1·267	54·2	52·6	107	3·4	3,308	3,240	1·02
1·429	32·06	1·338	61·0	53·9	120	3·6	3,529	3,429	1·03
1·504	33·88	1·412	67·5	65·2	133	3·7	3,703	3,609	1·03
1·582	35·78	1·491	74·5	73·0	148	3·9	3,904	3,797	1·05
1·669	37·58	1·566	83·0	80·5	164	4·1	4,088	4,006	1·02
1·741	39·65	1·652	90·0	89·5	180	4·3	4,301	4,178	1·03

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Table XIII.—Westinghouse Transformer.

Power, 6,500 watts. Secondary volts, 100.

Frequency used, 82·5 periods per second.

Average final temperature of transformer, 96° F.

Volts on primary circuit (V_1) = 2,400 (kept constant).

Primary circuit resistance = 5·95 ohms at 96° F.

Secondary circuit resistance = 0·0108 ohm at 96° F.

Results plotted in curves in Figs. 15 and 16, Plate 2.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		Efficiency, $\frac{W_2}{W_1}$, in per cents.
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	
101·0	0	0	2,400	0·050	95	—	95	—	0
100·9	1·00	101	,,	0·100	205	—	104	—	49·3
100·8	1·98	200	,,	0·140	306	—	106	—	65·4
100·8	2·94	296	,,	0·180	401	—	105	—	73·7
100·7	3·87	390	,,	0·218	493	—	103	—	79·1
100·7	4·79	482	,,	0·250	597	—	115	—	80·7
100·7	8·00	806	,,	0·382	920	—	114	—	87·6
100·4	10·15	1,019	,,	0·472	1,139	—	120	—	89·5
100·3	13·07	1,311	,,	0·580	1,440	—	129	—	91·1
100·1	18·00	1,802	,,	0·800	1,930	—	128	—	93·4
100·1	19·90	1,992	,,	0·880	2,118	—	126	—	94·1
100·0	21·93	2,193	,,	0·960	2,330	—	127	—	94·1
100·0	24·74	2,474	,,	1·080	2,609	—	135	—	94·8
100·0	29·66	2,966	,,	1·285	3,096	—	130	—	95·8
99·8	37·20	3,713	,,	1·610	3,870	—	157	—	96·0
99·5	42·00	4,179	,,	1·810	4,324	—	145	—	96·7
99·3	46·65	4,633	,,	2·002	4,792	—	150	—	96·9
99·2	50·40	5,000	,,	2·160	5,174	—	174	—	96·7
99·0	52·16	5,164	,,	2·240	5,422	—	258	—	95·3
98·9	55·60	5,499	,,	2·383	5,702	—	203	—	96·1
98·8	57·68	5,700	,,	2·478	5,885	—	185	—	96·9
98·9	59·32	5,867	,,	2·550	6,041	—	174	—	97·3
98·7	61·32	6,053	,,	2·633	6,271	—	218	—	96·7
98·8	62·16	6,142	,,	2·672	6,344	—	202	—	96·8
98·7	63·00	6,218	,,	2·700	6,426	—	208	—	96·8
98·7	64·00	6,317	,,	2·750	6,522	—	205	—	96·9
98·6	64·74	6,384	,,	2·775	6,598	—	214	—	96·9

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Table XIV.—Westinghouse Transformer.

Primary volts, 2,400 (kept constant).

Secondary volts at no load = 101·0.

Results plotted in curves in Figs. 17 and 18, Plate 3.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$.
			Primary, $I_1^2 \times 5{\cdot}95$.	Secondary, $I_2^2 \times 0{\cdot}0108$.	Total.		Real, W_1.	Apparent, W_1'.	
0·050	0	0	0	0	0	0	95	120	0·79
0·100	1·00	0·042	0	0	0	0·1	205	240	0·85
0·140	1·98	0·083	0·1	0	0	0·2	306	336	0·91
0·180	2 94	0·122	0·2	0·1	0	0·2	401	432	0·93
0·218	3·87	0·161	0·3	0·2	0	0·3	498	523	0·94
0·250	4·79	0·199	0·4	0·2	1	0·3	597	600	0·99
0·382	8·00	0·333	0·9	0·7	2	0·3	920	917	1·00
0·472	10·15	0·423	1·3	1·1	2	0·6	1,139	1,133	1·00
0·580	13·07	0·545	2·0	1·8	4	0·7	1,440	1,392	1·03
0·800	18·00	0·750	3·8	3·5	7	0·9	1,930	1,920	1·00
0·880	19·90	0·830	4·6	4·3	9	0·9	2,118	2,112	1·00
0·960	21·93	0·914	5·5	5·2	11	1·0	2,330	2,304	1·01
1·080	24·74	1·031	6·9	6·6	14	1·0	2,609	2,592	1·01
1·285	29·66	1·238	9·8	9·5	19	1·0	3,096	3,084	1·00
1·610	37·20	1·550	15·4	14·9	30	1·2	3,870	3,864	1·00
1·810	42·00	1·750	19·5	19·0	39	1·5	4,324	4,344	0·99
2·002	46·65	1·945	23·9	23·5	47	1·7	4,792	4,805	1·00
2·160	50·40	2·100	23·7	27·5	55	1·8	5,174	5,184	1·00
2·240	52·16	2·171	29·8	29·5	59	2·0	5,422	5,376	1·01
2·383	55·60	2·320	33·9	33·5	67	2·1	5,702	5,719	1·00
2·478	57·68	2·404	36·5	36·0	73	2·2	5,885	5,947	0·99
2·550	59·32	2·474	38·7	38·1	77	2·1	6,041	6,120	0 99
2·633	61·32	2·560	41·2	40·6	82	2·3	6,271	6,319	0·99
2·672	62·16	2·594	42·5	41·8	84	2·2	6,344	6,413	0·99
2·700	63·00	2·628	43·3	42·9	86	2·3	6,426	6,480	0·99
2·750	64·00	2·665	45·0	44·2	89	2·3	6,522	6,600	0·99
2·775	64·74	2·700	45 8	45·3	91	2·4	6,598	6,660	1·00

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Table XV.—Ferranti Transformer (1892 *pattern*).

No. 2,194. Power, 15 H.P. Secondary volts, 100.
Frequency used, 81·6 periods per second.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 6·45 ohms.
Secondary circuit resistance = 0·0134 ohm.

Results plotted in curves in Figs. 19 and 20, Plate 3.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	Efficiency, $\frac{W_2}{W_1}$, in per cents.
100·4	0	0	2,400	0·076	149	117	149	117	0
100·3	0·80	80	,,	0·100	222	201	142	121	36·0
100·3	2·68	269	,,	0·170	419	397	150	128	64·2
100·2	4·83	484	,,	0·270	637	617	153	133	75·9
100·1	8·06	807	,,	0·400	951	956	144	149	84·9
100·0	10·35	1,035	,,	0·503	1,189	1,196	154	161	87·0
100·0	12·33	1,233	,,	0·590	1,391	1,408	158	175	88·7
99·9	14·23	1,422	,,	0·670	1,580	1,612	158	190	90·0
100·0	16·19	1,619	,,	0·745	1,762	—	143	—	91·8
99·8	23·50	2,345	,,	1·050	2,506	—	161	—	93·5
99·7	25·22	2,514	,,	1·120	2,648	—	134	—	95·0
99·7	29·12	2,903	,,	1·265	3,064	—	161	—	94·9
99·6	32·98	3,285	,,	1·368	3,461	—	176	—	94·5
99·4	39·60	3,936	,,	1·695	4,120	—	184	—	95·5
99·3	44·18	4,387	,,	1·867	4,564	—	177	—	96·0
99·1	47·32	4,690	,,	1·980	4,878	—	188	—	96·1
98·8	49·96	4,936	,,	2·135	5,265	—	229	—	93·7
98·8	57·68	5,700	,,	2·466	5,947	—	245	—	96·0
98·8	62·36	6,160	,,	2·653	6,378	—	218	—	96·6
98·7	65·24	6,430	,,	2·771	6,680	—	250	—	96·2
98·5	69·62	6,858	,,	2·960	7,085	—	227	—	96·9
98·2	75·16	7,381	,,	3·187	7,618	—	237	—	96·9
98·0	78·80	7,722	,,	3·341	8,040	—	318	—	96·1
98·0	83·42	8,176	,,	3·521	8,501	—	326	—	96·2
98·0	87·90	8,614	,,	3·715	8,932	—	318	—	96·5
97·6	93·15	9,092	,,	—	9,495	—	403	—	95·8
97·6	97·29	9,495	,,	—	9,900	—	405	—	96·0
97·5	102·54	9,998	,,	—	10,415	—	427	—	95·9
97·4	105·48	10,274	,,	—	10,700	—	426	—	96·0
97·2	109·50	10,621	,,	—	11,120	—	477	—	95·5
97·0	113·04	10,965	,,	—	11,496	—	531	—	95·2
97·0	114·63	11,120	,,	—	11,690	—	570	—	95·4
100·1	0	0	,,	—	152	115	152	115	0

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Table XVI.—15-*H.P.* (1892 *pattern*) *Ferranti Transformer.*

Primary volts, 2,400 (kept constant).

Secondary volts at no load = 100·4.

Results plotted in curves in Figs. 21 and 22, Plate 3.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$.
			Primary, $I_1^2 \times 6{\cdot}45$.	Secondary, $I_2^2 \times 0{\cdot}0134$.	Total.		Real, W_1.	Apparent, W_1'.	
0·076	0	0	0	0	0	0	149	188	0·82
0·100	0·60	0·033	0·1	0	0	0·1	222	240	0·92
0.170	2 68	0 112	0·2	0·1	0	0·1	419	408	1·03
0·270	4·83	0·202	0·5	0 3	1	0·2	637	647	0·98
0·400	8·06	0·336	1·0	0·9	2	0·3	951	960	0·99
0·503	10 35	0·431	1·6	1·4	3	0 4	1,189	1,210	0 99
0·390	12·33	0·515	2·2	2·0	4	0·4	1,391	1,416	0·98
0·670	14·23	0·594	2·9	2·7	6	0·5	1,580	1,600	0·99
0·745	16·19	0·674	3·6	3·5	7	0·4	1,762	1,788	0·99
1·050	23·50	0·980	7·1	7·4	15	0·6	2,506	2,520	0·99
1·120	25·22	1·051	8·1	8·5	17	0·7	2,648	2,688	0·99
1·265	29·12	1·213	10·4	11·4	22	0·7	3,064	3,036	1·01
1·368	32·98	1·373	12·1	14·5	27	0 8	3,461	3,283	1·05
1·695	39·60	1·650	18·6	21·0	40	1·0	4,120	4,068	1·01
1·867	44·18	1·841	22·5	26·2	49	1·1	4,564	4,481	1·02
1·980	47·32	1·975	25·3	30·0	55	1·3	4,878	4,752	1·02
2·135	49·96	2 081	29·4	33·4	63	1·6	5,265	5,124	1·03
2·466	57·68	2·404	39 3	44·6	84	1·6	5,947	5,918	1·00
2·653	62·36	2·600	45·5	52·0	98	1·6	6,378	6,367	1·00
2·771	65·24	2·721	49·5	57·1	107	1·7	6,680	6,650	1·00
2·960	69·62	2·901	56·4	65·0	121	1·9	7,085	7,104	1·00
3·187	75·16	3·138	65·6	75·5	141	2·2	7,618	7,649	1·00
3·341	78·80	3·286	72·0	83·2	155	2·4	8,040	8,018	1·00
3·521	83·42	3·478	80·0	93·0	173	2·4	8,501	8,450	1·00
3·715	87·90	3·661	89·1	103·5	193	2·4	8,932	8,916	1·00
3·900	92·34	3·846	97·0	114·0	211	2·6	9,390	9,360	1·00
4·089	97·00	4·040	107·8	126·0	234	2·7	9,980	9,813	1·02
4·276	101·50	4·237	118·1	138 1	256	2·9	10,300	10,502	0·98
4·483	106·32	4·440	130·0	152·0	282	3·1	10,790	10,759	1·00
4·548	107·67	4·485	133·2	155·5	289	3·3	10,978	10,915	1·00
4·648	109·98	4·588	139·2	162·0	301	3·3	11,210	11,155	1·00
4·762	112·89	4·708	146·4	171 0	317	3·4	11,496	11,429	1·00

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Table XVII.—Ferranti Transformer (1892 *pattern, re-wound*).

No. 2,193. Power, 15 H.P. Secondary volts = 100.

Frequency used, 82·5 periods per second.

Volts on primary circuit (V_1) = 2,400 (kept constant).

Primary circuit resistance = 3·77 ohms.

Secondary circuit resistance = 0·0092 ohm.

Results plotted in curves in Figs. 19 and 23, Plate 3.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	Efficiency, $\frac{W_2}{W_1}$, in per cents.
100·0	0	0	2,400	0·103	229	—	229	—	0
99·9	1·20	120	,,	0·150	351	—	231	—	33·2
99·9	2·68	268	,,	0·212	492	—	224	—	54·5
99·9	4·81	481	,,	0·300	715	—	234	—	67·3
99·9	8·06	805	,,	0·412	1,023	—	218	—	78·6
99·9	12·25	1,224	,,	0·591	1,466	—	242	—	83·6
99·8	18·11	1,807	,,	0·833	2,032	—	225	—	89·0
99·8	23·45	2,340	,,	1·053	2,564	—	224	—	91·2
99·7	28·91	2,885	,,	1·292	3,136	—	251	—	92·1
99·6	34·64	3,450	,,	1·529	3,728	—	278	—	92·5
99·5	39·50	3,931	,,	1·723	4,189	—	258	—	94·0
99·3	45·89	4,507	,,	1·967	4,770	—	263	—	94·5
99·1	50·40	4,995	,,	2·181	5,260	—	265	—	95·0
99 0	56·00	5,544	,,	2·408	5,841	—	297	—	95·0
100·0	—	—	,,	—	214	—	214	—	—
98·9	61·58	6,090	,,	2·646	6,391	—	301	—	95·3
98·8	67·06	6,626	,,	2·877	6,957	—	331	—	95·1
98·7	73·76	7,280	,,	3·156	7,645	—	365	—	95 3
98·6	80 80	7,968	,,	3·447	8,349	—	381	—	95·5
98·5	88·10	8,678	,,	3·752	9,103	—	425	—	95·4
98·3	95·00	9,338	,,	4 025	9,791	—	453	—	95·4
98·1	101·82	9,989	,,	4·331	10,473	—	484	—	95·5
98·0	105·99	10,387	,,	4·499	10,901	—	514	—	95·3
97 9	108·33	10,608	,,	4·613	11,162	—	554	—	95·1
97·8	112·08	10,962	,,	4·762	11,559	—	597	—	94·9
97·8	113·85	11,185	,,	4·837	11,742	—	607	—	94·9
100·0	0	0	,,	0	244	—	244	—	0

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Table XVIII.—15-*H.P.* (1892 *pattern, re-wound*) *Ferranti Transformer.*

Primary volts, 2,400 (kept constant).
Secondary volts at no load = 100.0.

Results plotted in curves in Figs. 24 and 25, Plate 3.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$
			Primary, $I_1^2 \times 3\cdot77$.	Secondary, $I_2^2 \times 0\cdot0092$.	Total.		Real, W_1.	Apparent, W_1'.	
0·103	0	0	0	0	0	0·	229	247	0·93
0·150	1·20	0·050	0	0	0	0·1	351	360	0·97
0·212	2·68	0·112	0·2	0	0	0·1	492	508	0·97
0·300	4·81	0·200	0·3	0·2	1	0·1	715	720	0·99
0·412	8·06	0·334	0·6	0·6	1	0·1	1,023	990	1·03
0·591	12·25	0·510	1·3	1·4	3	0·1	1,466	1,418	1·03
0·833	18·11	0·765	2·6	3·0	6	0·2	2,032	1,919	1·06
1·053	23·45	0·977	4·2	5·2	9	0·2	2,564	2,527	1·01
1·292	28·91	1·203	6 3	7·7	14	0.3	3,136	3,101	1·01
1·529	34·64	1·443	8·8	11·0	20	0·4	3,728	3,669	1·01
1·723	39·50	1·647	11·2	15·6	27	0·5	4,189	4,135	1·01
1·967	45·39	1·890	14·6	19·0	34	0·7	4,770	4,721	1·01
2·181	50·40	2·100	18·0	23·3	41	0·9	5,260	5,234	1·00
2·408	56·00	2·334	21·9	28 8	51	1·0	5,841	5,779	1 01
2·646	61·58	2·568	26·4	35·0	61	1·1	6,391	6,350	1·00
2·877	67·06	2·800	31·1	41·3	72	1·2	6,957	6,905	1·01
3·156	73·76	3·072	37·6	50·0	88	1·3	7,645	7,574	1·01
3·447	80·80	3·366	44·7	60·0	105	1·4	8,349	8,273	1·01
3·752	88·10	3·670	53·0	71·3	124	1·5	9,103	9,005	1·01
4·025	95·00	3·959	61·2	82·8	144	1·7	9,791	9,660	1·01
4·331	101·82	4·242	70·8	95·0	166	1·9	10,473	10,394	1·01
4·499	105·99	4·416	76·2	103·0	179	2·0	10,901	10,818	1·01
4·613	108·33	4·514	80·2	108·0	188	2·1	11,162	11,171	1·00
4·762	112·08	4·670	85·4	115·7	201	2·2	11,559	11,429	1·01
4·837	113·85	4·744	88·1	119·0	207	2·2	11,742	11,609	1 01

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Table XIX.—Ferranti Transformer (1892 *pattern*).

No. 2184. Power, 20 H.P. Secondary volts = 100.
Frequency used, 82·7 periods per second.
Average final temperature of transformer, 98 F.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 2·75 ohms at 98° F.
Secondary circuit resistance = 0·0061 ohm at 98° F.

Results plotted in curves in Figs. 26 and 27, Plate 3.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	Efficiency, $\frac{W_2}{W_1}$, in per cents.
100·0	0	0	2,400	0·112	230	195	230	195	0
100·0	1·91	191	,,	0·173	420	373	231	182	45·5
99·9	4·08	408	,,	0·274	646	587	238	179	63·2
99·9	4·82	482	,,	0·304	709	664	227	182	68·0
99·9	6·16	616	,,	0·350	861	795	245	179	71·6
100·0	8·12	812	,,	0·424	1,046	989	234	177	77·7
100·0	10·29	1,029	,,	0·524	1,270	1,208	241	179	81·0
100·0	12·29	1,229	,,	0·602	1,470	1,426	241	197	83·6
100·0	14·28	1,428	,,	0·685	1,682	1,621	254	193	84·9
100·0	19·06	1,906	,,	0·884	2,116	—	210	—	90·2
99·8	24·98	2,493	,,	1·130	2,722	—	229	—	91·5
99·7	30·98	3,089	,,	1·378	3,336	—	247	—	92·6
99·7	34·64	3,454	,,	1·538	3,711	—	257	—	93·1
99·7	39·80	3,968	,,	1·740	4,216	—	248	—	94·1
99·6	45·44	4,526	,,	1·975	4,758	—	232	—	95·2
99·5	51·22	5,097	,,	2·220	5,372	—	275	—	94·8
99·3	57·68	5,728	,,	2·500	6,047	—	319	—	94·8
99·2	65·48	6,496	,,	—	6,780	—	284	—	95·7
99·1	72·88	7,223	,,	3·124	7,525	—	302	—	96·2
99·0	79·50	7,871	,,	3·400	8,215	—	344	—	95·8
98·9	86·82	8,587	,,	3·690	8,933	—	346	—	96·2
98·8	93·20	9,208	,,	3·965	9,543	—	335	—	96·5
98·8	100·48	9,928	,,	4·271	10,307	—	379	—	96·3
98·7	108·31	10,691	,,	4·614	11,000	—	309	—	97·1
98·5	114·48	11,277	,,	4·866	11,602	—	325	—	97·2
98·3	121·92	11,985	,,	5·184	12,373	—	388	—	97·0
98·3	127·56	12,589	,,	5·440	12,959	—	420	—	97·0
98·1	134·97	13,241	,,	5·754	13,676	—	435	—	97·0
98·0	141·99	13,915	,,	6·040	14,375	—	460	—	96·9
97·9	146·10	14,308	,,	6·222	14,828	—	525	—	96·5
97·9	148·56	14,544	,,	6·312	15,052	—	508	—	96·6
97·9	151·44	14,828	,,	6·500	15,322	—	504	—	96·9
100·0	0	0	,,	0	228	195	228	195	0

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Table XX.—20-H.P. (1892 pattern) Ferranti Transformer.

Primary volts, 2,400 (kept constant).
Secondary volts at no load = 100·0.

Results plotted in curves in Figs. 28 and 29, Plate 4.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$.
			Primary, $I_1^2 \times 2{\cdot}75$.	Secondary, $I_2^2 \times 0{\cdot}0061$.	Total.		Real, W_1.	Apparent, W_1'.	
0·112	0	0	0	0	0	—	230	269	0·86
0·173	1·91	0·079	0	0	0	—	422	416	1·00
0·274	4·08	0·170	0	0	0	0·1	646	657	0·98
0·304	4·82	0·201	0·2	0	0·2	0·1	709	729	0·97
0·350	6·16	0·256	0·3	0	0·3	0·1	861	840	1·03
0·424	8·12	0·338	0·5	0·4	1	—	1,046	1,017	1·03
0·524	10·29	0·429	0·7	0·6	1	—	1,270	1,257	1·01
0·602	12·29	0·511	1·0	0·9	2	—	1,470	1,445	1·02
0·685	14·28	0·595	1·3	1·2	3	—	1,682	1,643	1·02
0·884	19·06	0·794	2·1	2·2	4	—	2,116	2,121	1·00
1·130	24·98	1·040	3·5	3·8	7	0·2	2,722	2,712	1·00
1·378	30·98	1·290	5·2	5·8	11	0·3	3,336	3,307	1 01
1·538	34·61	1·443	6·5	7·3	14	0·3	3,711	3,691	1·00
1·740	39·80	1·659	8·3	9·5	18	0·3	4,216	4,176	1 01
1·975	45·44	1·895	10·7	12·5	23	0·4	4,758	4,740	1·00
2·220	51·22	2·140	13·6	15 8	29	0·5	5,372	5,328	1·01
2 500	57·68	2·401	17·2	20·0	37	0·7	6,047	6,000	1·01
3·124	72·88	3·038	26·9	32·4	59	0·9	7,525	7,498	1·00
3·400	79·50	3·310	31·8	37·8	70	1·0	8,215	8,160	1·00
3·690	86·82	3·620	37·5	45·8	83	1·1	8,933	8,856	1·01
3·965	93·20	3·881	43·3	50·4	94	1·2	9,543	9,516	1·00
4·271	100·48	4·195	50·0	61·7	112	1·2	10,307	10,250	1·00
4·614	108·31	4·513	58·4	70·7	129	1 3	11,000	11,074	0·99
4·866	114·48	4·770	65·0	80·0	145	1·5	11,602	11,678	0·99
5·184	121·92	5·061	74·0	89·0	163	1·7	12,373	12,434	0·99
5·440	127·56	5·315	81·3	99·5	181	1·7	12,959	13,056	0·99
5·754	134·97	5·624	91·2	109 4	201	1·9	13,676	13,810	0·99
6·040	141·99	5·916	100·0	123·0	223	2·0	14,375	14,496	0·99
6·222	146·10	6·087	106·5	128·2	235	2·1	14,823	14,933	0·99
6·312	148·56	6·190	110·0	135·0	245	2·1	15,052	15,149	0 99
6·500	151·44	6·310	116·0	138·0	254	2·1	15,322	15,400	0·98

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Table XXI.—Mordey Transformer.

No. 1868. Power, 6,000 watts. Secondary volts, 50 and 100.
Frequency used, 82·9 periods per second.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 7·73 ohms.
Secondary circuit resistance = 0·0163 ohm.

Results plotted in curves in Figs. 30 and 31, Plate 4.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		Efficiency, $\frac{W_2}{W_1}$, in per cents.
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	
101·1	0	0	2,400	0·076	148	105	148	105	0
101·1	1·00	101	,,	0·100	246	217	145	116	41·0
101·1	2·97	300	,,	0·173	461	417	161	117	65·1
101·1	4·83	488	,,	0·269	646	600	158	112	75·6
101·1	6·60	667	,,	0·357	836	800	169	133	79·8
101·1	9·00	910	,,	0·447	1,071	1,047	161	137	84·8
101·0	12·04	1,216	,,	0·562	1,378	1,368	162	152	88·3
101·0	15·07	1,522	,,	0·692	1,690	1,668	168	146	90·1
101·0	16·91	1,708	,,	0·769	1,904	—	196	—	89·7
100·9	19·90	2,008	,,	0·900	2,177	—	169	—	92·5
100·8	22·93	2,311	,,	1·019	2,521	—	210	—	91·6
100·7	25·41	2,559	,,	1·127	2,736	—	177	—	93·5
100·6	28·35	2,852	,,	1·253	3,044	—	192	—	93·7
100·5	31·18	3,134	,,	1·368	3,366	—	232	—	93·1
100·4	33·94	3,408	,,	1·490	3,622	—	214	—	94·1
100·2	36·82	3,689	,,	1·615	3,889	—	200	—	94·9
100·0	39·75	3,975	,,	1·735	4,203	—	228	—	94·6
100·0	42·76	4,276	,,	1·862	4,516	—	240	—	94·5
100·0	45·56	4,556	,,	1·980	4,775	—	219	—	95·3
99·9	48·37	4,832	,,	2·105	5,089	—	257	—	95·0
99·7	50·64	5,049	,,	2·195	5,316	—	267	—	94·9
99·6	53·36	5,315	,,	2·300	5,596	—	281	—	95·0
99·5	56·14	5,586	,,	2·431	5,895	—	309	—	94·8
99·4	57·96	5,762	,,	2·510	6,061	—	299	—	95·1
99·3	60·00	5,958	,,	2·592	6,266	—	308	—	95·1
99·2	61·58	6,109	,,	2·668	6,420	—	311	—	95·4
101·3	0	0	,,	0·076	150	108	150	108	0

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Table XXII.—Mordey Transformer.

Primary volts, 2,400 (kept constant).

Secondary volts at no load = 101·1.

Results plotted in curves in Figs. 32 and 33, Plate 4.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$.
			Primary, $I_1^2 \times 7{\cdot}73$.	Secondary, $I_2^2 \times 0{\cdot}0163$.	Total.		Real, W_1.	Apparent, W_1'.	
0·076	0	0	0	0	0	0	148	182	0·81
0·100	1·00	0·042	0	0	0	0	246	240	1·02
0·193	2·97	0·124	0·3	0·1	0·4	0	461	463	1·00
0·269	4·83	0·202	0·5	0·3	1	0	646	655	0·99
0·357	6·60	0·275	1·0	0·7	2	0	836	854	0·98
0·447	9·00	0·375	1·5	1·3	3	0	1,071	1,070	1·00
0·562	12·04	0·501	2·4	2·4	5	0·1	1,378	1,348	1·02
0·692	15·07	0·627	3·7	3·7	7	0·1	1,690	1,660	1·02
0·769	16·91	0·705	4·6	4·7	9	0·1	1,904	1,845	1·03
0·900	19·90	0·830	6·3	6·5	13	0·2	2,177	2,160	1·01
1·019	22·93	0·955	8·0	8·6	17	0·3	2,521	2,445	1·03
1·127	25·41	1·060	9·8	10·6	20	0·4	2,736	2,705	1·01
1·253	28·35	1·181	12·2	13·1	25	0·5	3,044	3,007	1·01
1·368	31·18	1·300	14·5	15·8	30	0·6	3,366	3,283	1·02
1·490	33·94	1·414	17·2	18·8	36	0·7	3,622	3,576	1·01
1·615	36·82	1·536	20·2	22·1	42	0·9	3,859	3,876	1·00
1·735	39·75	1·658	23·3	25·7	49	1·1	4,203	4,164	1·01
1·863	42·76	1·782	27·0	29·8	57	1·1	4,516	4,471	1·01
1·980	45·66	1·900	30·3	34·0	64	1·1	4,775	4,752	1·00
2·105	48·37	2·018	34·5	38·1	73	1·2	5,089	5,052	1·00
2·195	50·64	2·110	37·2	41·8	79	1·4	5,316	5,268	1·01
2·300	53·36	2·221	41·0	46·5	88	1·5	5,596	5,520	1·01
2·431	56·14	2·340	45·7	51·5	97	1·6	5,895	5,834	1·01
2·510	57·96	2·415	48·8	54·8	104	1·7	6,061	6,024	1·00
2·592	60·00	2·500	50·4	58·8	109	1·8	6,266	6,221	1·00
2·668	61·58	2·566	55·0	62·0	117	1·9	6,420	6,403	1·00

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Table XXIII.—Thomson-Houston Transformer.

No. 46,740. Power, 4,500 watts. Secondary volts, 50 and 100.
Frequency used, 82·1 periods per second.
Average final temperature of transformer, 102° F.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 19·68 ohms at 102° F.
Secondary circuit resistance = 0·019 ohm at 102° F.

Results plotted in curves in Figs. 34 and 35, Plate 4.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		Efficiency, $\frac{W_2}{W_1}$, in per cents.
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	
99·8	0	0	2,400	0·083	116	82	116	82	0
99·8	0·98	98	,,	0·119	216	185	118	87	45·4
99·8	1·96	196	,,	0·150	320	282	124	86	61·2
99·8	2·90	289	,,	0·185	403	380	114	91	71·7
99·7	4·73	472	,,	0·250	597	562	125	90	79·1
99·6	6·93	690	,,	0·340	818	779	128	89	84·4
99·4	8·89	884	,,	0·420	1,005	974	121	90	88·0
99·2	10·95	1,087	,,	0·500	1,206	1,183	119	96	90·1
99·0	12·80	1,275	,,	0·583	1,410	1,383	135	108	90·5
98·7	15·10	1,490	,,	0·671	1,623	1,611	133	121	91·8
98·5	18·00	1,773	,,	0·800	1,905	—	132	—	93·0
98·5	20·45	2,014	,,	0·900	2,160	—	146	—	93·2
98·3	23·26	2,287	,,	1·016	2,447	—	160	—	93·5
98·3	25·50	2,507	,,	1·112	2,668	—	161	—	94·0
98·1	27·06	2,655	,,	1·173	2,820	—	165	—	94·2
97·9	29·87	2,925	,,	1·286	3,090	—	165	—	94·7
97·7	32·50	3,175	,,	1·394	3,376	—	201	—	94·2
97·5	35·33	3,445	,,	1·506	3,630	—	185	—	94·9
97·3	38·00	3,698	,,	1·620	3,921	—	223	—	94·3
97·1	39·85	3,870	,,	1·698	4,070	—	200	—	95·1
96·8	41·62	4,035	,,	1·773	4,242	—	207	—	95·2
96·7	43·41	4,198	,,	1·860	4,418	—	220	—	95·0
96·6	45·08	4,355	,,	1·928	4,620	—	265	—	94·3
96·5	46·00	4,439	,,	1·960	4,695	—	256	—	94·5
99·8	0	0	,,	0	98	84	98	84	0

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Table XXIV.—Thomson-Houston Transformer.

Primary volts, 2,400 (kept constant).

Secondary volts at no load = 99·8.

Results plotted in curves in Figs. 36 and 37, Plate 4.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$.
			Primary, $I_1^2 \times 19{\cdot}68$.	Secondary, $I_2^2 \times 0{\cdot}0119$.	Total.		Real, W_1.	Apparent, W_1'.	
0·083	0	0	0	0	0	0	116	199	0·58
0·119	0·98	0·041	0·2	0	0	0	216	285	0·76
0·150	1·96	0·081	0·4	0	0	0	320	360	0·89
0·186	2·90	0·120	0·6	0·2	1	0	403	447	0·90
0·250	4·73	0·197	1·3	0·4	2	0·1	597	600	1·00
0·340	6·93	0·288	2·3	0·9	3	0·2	818	816	1·00
0·420	8·89	0·370	3·5	1·5	5	0·4	1,005	1,006	1·00
0·500	10·95	0·456	5·2	2·2	7	0·6	1,206	1,199	1·00
0·583	12·88	0·536	7·0	3·1	10	0·8	1,410	1,400	1·00
0·671	15·10	0·629	9·3	4·3	14	1·1	1,623	1,608	1·01
0·800	18·00	0·750	12·8	6·1	19	1·3	1,905	1,920	0·99
0·900	20·45	0·852	16·3	7·9	24	1·3	2,160	2,160	1·00
1·016	23·36	0·970	20·1	10·2	30	1·5	2,447	2,438	1·00
1·112	25·50	1·061	23·5	12·3	36	1·5	2,667	2,669	1·00
1·173	27·06	1·129	26·0	13·8	40	1·7	2,820	2,815	1·00
1·286	29·37	1·245	31·0	16·8	48	1·9	3,090	3,086	1·00
1·394	32·50	1·352	36·7	20·0	57	2·1	3,376	3,345	1·01
1·506	35·33	1·471	43·1	23·6	67	2·3	3,630	3,614	1·00
1·620	38·00	1·582	51·7	27·0	79	2·5	3,921	3,888	1·01
1·698	39·85	1·661	57·0	30·0	87	2·7	4,070	4,075	1·00
1·778	41·62	1·735	62·0	32·7	95	3·0	4,242	4,255	1·00
1·860	43·41	1·809	68·1	35·7	104	3·1	4,418	4,464	0·99
1·928	45·08	1·880	73·7	38·5	112	3·2	4,620	4,627	1·00
1·960	46·00	1·916	76·0	40·0	116	3·3	4,695	4,704	1·00

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Table XXV.—Kapp Transformer.

No. 570. Power, 4,000 watts. Secondary volts, 100.
Frequency used, 82·1 periods per second.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 11·38 ohms.
Secondary circuit resistance = 0·024 ohm.

Results plotted in curves in Figs. 38 and 39, Plate 5.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		Efficiency, $\frac{W_2}{W_1}$, in per cents.
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	
99·0	0	0	2,400	0·145	152	112	152	112	0
99·0	1·00	99	,,	0·160	265	212	166	113	37·4
98·9	1·95	193	,,	0·200	338	301	145	108	57·1
98·9	2·89	286	,,	0·229	445	393	159	107	64·3
98·8	3·98	393	,,	0·270	548	494	155	101	71·7
98·8	4·87	481	,,	0·304	640	588	159	107	75·2
98·8	7·17	708	,,	0·374	868	801	160	93	81·6
98·8	8·85	875	,,	0·467	1,029	982	154	107	85·0
98·0	10·81	1,060	,,	0·519	1,225	1,176	165	116	86·5
98·7	12·73	1,257	,,	0·618	1,426	1,387	169	130	88·1
98·6	14·63	1,443	,,	0·675	1,609	1,550	166	107	89·7
98·3	16·49	1,621	,,	0·743	1,800	—	179	—	90·1
98·2	18·52	1,819	,,	0·831	1,988	—	169	—	91·5
98·1	20·42	2,003	,,	0·908	2,182	—	179	—	91·8
98·0	22·25	2,180	,,	0·981	2,358	—	178	—	92·5
98·0	24·10	2,362	,,	1·052	2,538	—	176	—	93·2
97·9	25·95	2,541	,,	1·136	2,745	—	204	—	92·6
97·8	27·86	2,725	,,	1·212	2,894	—	169	—	94·3
97·7	29·80	2,912	,,	1·288	3,100	—	188	—	94·0
97·7	31·62	3,090	,,	1·360	3,289	—	199	—	94·0
97·5	33·41	3,258	,,	1·442	3,481	—	223	—	93·5
97·4	35·27	3,436	,,	1·520	3,658	—	222	—	93·9
97·3	37·09	3,609	,,	1·590	3,821	—	212	—	94·5
97·2	39·14	3,805	,,	1·680	4,035	—	230	—	94·4
97·1	40·89	3,971	,,	1·750	4,207	—	236	—	94·4

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Table XXVI.—Kapp Transformer.

Primary volts, 2,400 (kept constant).
Secondary volts at no load = 99·0.

Results plotted in curves in Figs. 40 and 41, Plate 5.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$.
			Primary, $I_1^2 \times 11{\cdot}38$.	Secondary, $I_2^2 \times 0{\cdot}024$.	Total.		Real, W_1.	Apparent, W_1'.	
0·145	0	0	0	0	0	0	152	348	0·61
0·160	1·00	0·042	0·2	0	0	0	265	384	0·66
0·200	1·95	0·081	0·3	0	0	0·1	338	480	0·70
0·229	2·89	0·120	0·5	0·2	1	0·1	445	549	0·81
0·270	3·98	0·165	0·8	0·4	1	0·2	548	648	0·85
0·304	4·87	0·203	1·0	0·6	2	0·2	640	729	0·88
0·374	7·17	0·299	1·6	1·3	3	0·2	868	897	0·97
0·467	8·85	0·369	2·5	1·9	4	0·2	1,029	1,121	0·92
0·519	10·81	0·450	3·1	2·8	6	1·0	1,225	1,245	0·98
0·618	12·73	0·531	4·3	3·9	8	0·3	1,426	1,483	0·96
0·675	14·63	0·610	5·2	5·1	10	0·4	1,609	1,620	0·99
0·743	16·49	0·687	6·6	6·5	13	0·7	1,800	1,783	1·01
0·831	18·52	0·771	7·9	8·2	16	0·8	1,988	1,994	1·00
0·908	20·42	0·852	9·4	10·0	19	0·9	2,182	2,179	1·00
0·981	22·25	0·927	11·0	11·9	23	1·0	2,358	2,354	1·00
1·052	24·10	1·001	12·6	13·9	27	1·0	2,538	2,525	1·00
1·136	25·95	1·081	14·6	16·3	31	1·1	2,745	2,726	1·01
1·212	27·86	1·161	16·7	18·6	35	1·2	2,894	2,909	0·99
1·288	29·80	1·241	19·0	21·3	40	1·3	3,100	3,091	1·00
1·360	31·62	1·320	21·0	24·0	45	1·3	3,289	3,264	1·01
1·442	33·41	1·392	23·6	26·7	50	1·5	3,481	3,461	1·00
1·520	35·27	1·469	26·3	30·8	57	1·6	3,658	3,648	1·00
1·590	37·09	1·547	28·6	33·1	62	1·7	3,821	3,816	1·00
1·680	39·14	1·631	32·0	36·8	69	1·8	4,035	4,032	1·00
1·750	40·89	1·703	34·5	42·0	77	1·9	4,207	4,200	1·00

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Table XXVII.—Swinburne "Hedgehog" Transformer.

No. 342. Power, 3,000 watts. Secondary volts, 100.
Frequency used, 81·1 periods per second.
Average final temperature of transformer, 145° F.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 24·00 ohms at 145° F.
Secondary circuit resistance = 0·051 ohm at 145° F.

Results plotted in curves in Figs. 42 and 43, Plate 5.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		Efficiency, $\frac{W_2}{W_1}$, in per cents.
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	
101·8	0	0	2,400	0·756	121	55	121	55	0
101·7	1·00	102	,,	0·761	213	157	111	55	47·9
101·5	2·97	301	,,	0·786	414	357	113	56	72·7
101·3	4·84	490	,,	0·811	608	546	118	56	80·6
101·3	6·00	607	,,	0·829	730	664	123	57	83·1
101·2	8·00	810	,,	0 862	943	869	133	59	85·9
101·0	10·20	1,030	,,	0·915	1,161	1,092	131	62	88·6
100·9	12·00	1,211	,,	0·960	1,361	1,275	150	64	89·0
100·6	14·00	1,408	,,	1·013	1,551	1,477	143	69	90·8
100·3	15·87	1,592	,,	1·066	1,750	—	158	—	91·1
100·0	17·89	1,789	,,	1·133	1,951	—	162	—	91·7
100·0	19 80	1,980	,,	1·197	2,129	—	149	—	93·0
99·9	21·82	2,180	,,	1·260	2,344	—	164	—	93·0
99·7	23·66	2,359	,,	1·321	2,525	—	166	—	93·3
99·5	25·46	2,534	,,	1·397	2,732	—	198	—	92·8
99·3	26·46	2,628	,,	1·420	2,823	—	195	—	93·2
99·1	27·42	2,718	,,	1·465	2,914	—	196	—	93·4
99 0	28·28	2,800	,,	1·500	2,988	—	188	—	93·7
98·9	29·26	2,896	,,	1·532	3,103	—	207	—	93·3
99·0	30 20	2,990	,,	1·566	3,185	—	195	—	94·0
101·9	0	0	,,	0	121	55	121	55	0

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Table XXVIII.—Swinburne "Hedgehog" Transformer.

No. 342. Power, 3,000 watts. Secondary volts, 100.
Frequency used, 82·3 periods per second.
Average final temperature of transformer, 145° F.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 24·00 ohms at 145° F.
Secondary circuit resistance = 0·051 ohm at 145° F.

Results plotted in curves in Figs. 42 and 43, Plate 5.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		
					Watts.				
Volts.	Amperes.	Watts, W_2.	Volts.	Amperes.	By Dynamometer Wattmeter, W_1.	By Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By Swinburne Wattmeter.	Efficiency, $\frac{W_2}{W_1}$, in per cents.
101·9	0	0	2,400	0·756	121	55	121	55	0
101·8	1·02	104	,,	0·761	228	159	124	55	45·6
101·7	2·98	303	,,	0·786	420	359	117	56	72·1
101·5	4·84	491	,,	0·811	621	547	130	56	79·1
101·3	6·00	608	,,	0·829	730	665	122	57	83·3
101·1	8·00	809	,,	0·862	943	868	134	59	85·8
100·9	10·00	1,009	,,	0·911	1,152	1,070	143	61	87·5
100·7	12·00	1,208	,,	0·960	1,353	1,272	145	64	89·3
100·6	14·0	1,408	,,	1·013	1,538	1.477	130	69	91·6
100·4	15·88	1,594	,,	1·066	1,746	—	152	—	91·4
100·2	17·89	1,792	,,	1·133	1,932	—	140	—	92·8
100·0	19·80	1,980	,,	1·197	2,129	—	149	—	93·0
99·8	21·63	2,159	,,	1·260	2,320	—	161	—	93·0
99·5	23·58	2,341	,,	1·321	2,510	—	169	—	93·4
99·2	25·46	2,526	,,	1·395	2,706	—	180	—	93·3
99·0	26·38	2,612	,,	1·426	2,829	—	217	—	92·4
98·9	27·28	2,698	,,	1·460	2,890	—	192	—	93·3
98·9	28·21	2,790	,,	1·498	2,993	—	203	—	93·2
98·7	29·19	2,881	,,	1·529	3,042	—	161	—	94·7
98·6	30·33	2,991	,,	1·567	3,163	—	172	—	94·5
101·9	0	0	,,	0	121	55	121	55	0

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Table XXIX.—Swinburne "Hedgehog" Transformer.

Primary volts, 2,400 (kept constant).
Secondary volts at no load = 102·00.

Results plotted in curves in Figs. 44 and 45.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$
			Primary, $I_1^2 \times 24{\cdot}00$.	Secondary, $I_2^2 \times 0{\cdot}061$.	Total.		Real, W_1.	Apparent, W_1'.	
0·756	0	0	13·7	0	14	0	121	1,816	0·07
0·761	1·02	0·042	14·0	0·1	14	0·1	228	1,829	0·13
0·786	2·98	0·124	14·9	0·5	15	0·2	420	1,886	0·22
0·811	4·84	0·202	15·8	1·2	17	0·4	621	1,948	0·32
0·829	6·00	0·250	16·5	1·8	18	0·6	730	1,988	0·37
0·862	8·00	0·333	17·8	3·3	21	0·8	943	2,067	0·46
0·911	10·00	0·417	19·9	5·1	25	1·0	1,152	2,188	0·53
0·960	12·00	0·500	22·6	7·3	30	1·2	1,353	2,301	0·59
1·013	14·00	0·584	24·7	10·0	35	1·3	1,538	2,432	0·63
1·066	15·88	0·663	27·2	12·9	40	1·5	1,746	2,559	0·68
1·133	17·89	0·746	30·9	16·3	47	1·7	1,932	2,720	0·71
1·197	19·80	0·825	34·5	20·0	55	1·9	2,129	2,873	0·74
1·260	21·63	0·902	38·2	24·0	62	2·1	2,320	3,022	0·77
1·321	23·58	0·983	42·0	28·3	70	2·4	2,510	3,172	0·79
1·395	25·46	1·060	46·6	33·1	80	2·7	2,706	3,350	0·81
1·426	26·38	1·098	48·9	35·4	84	2·9	2,829	3,422	0·83
1·460	27·28	1·136	51·2	38·0	89	3·0	2,890	3,501	0·83
1·498	28·21	1·175	54·0	40·8	95	3·0	2,993	3,596	0·83
1·529	29·19	1·215	56·1	43·5	100	3·2	3,042	3,666	0·83
1·567	30·33	1·262	59·0	46·9	106	3·3	3,163	3,761	0·84

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Table XXX.—Swinburne "Hedgehog" Transformer.

No. 601. Power, 6,000 watts. Secondary volts, 100.
Frequency used, 83 periods per second.
Volts on primary circuit (V_1) = 2,400 (kept constant).
Primary circuit resistance = 7·93 ohms (measured hot).
Secondary circuit resistance = 0·01512 ohm (measured hot).

Results plotted in curves in Figs. 47 and 48, Plate 6.

Secondary Circuit.			Primary Circuit.				Total Power taken up in Transformer.		
					Watts.				
Volts.	Amperes, I_2.	Watts, W_2.	Volts.	Amperes, I_1.	By Dynamometer Wattmeter, W_1.	By New Swinburne Wattmeter.	By Dynamometer Wattmeter, $W_1 - W_2$.	By New Swinburne Wattmeter.	Efficiency, $\frac{W_2}{W_1}$, in per cents.
102·0	0	0	2,400	1·194	151	142*	151	142	0
102·0	1·02	104	,,	1·199	272	243	168	139	38·2
102·0	3·06	312	,,	1·214	469	451	157	139	66·5
102·0	5·19	529	,,	1·217	696	659	167	130	76·0
101·9	8·00	815	..	1·281	984	961	169	146	82·9
101·8	12·08	1,229	,,	1·342	1,401	1,356	172	127	87·7
101·7	16·03	1,630	,,	1·442	1,804	1,740	174	110	90·3
101·5	20·00	2,030	,,	1·549	2,182	2,200	152	170	93·0
101·4	23·87	2,420	,,	1·661	2,583	2,580	163	160	93·7
101·2	26·08	2,639	,,	1·721	2,784	2,806	145	167	94·7
101·0	29·73	3,003	,,	1·855	3,178	3,198	175	195	94·7
100·8	34·35	3,462	,,	2·030	3,632	—	170	—	95·4
100·5	39·29	3,948	,,	2·218	4,101	—	153	—	96·3
100·1	43·86	4,390	,,	2·392	4,571	—	161	—	96·0
99·9	48·58	4,853	,,	2·577	5,083	—	180	—	96·5
99·7	51·38	5,123	,,	2·692	5,297	—	174	—	96·9
99·5	53·96	5,370	,,	2·800	5,584	—	214	—	96·1
99·2	57·68	5,723	,,	2·953	5,947	—	224	—	96·4
99·0	60·26	5,966	,,	3·059	6,225	—	259	—	95·9
102·1	0	0	,,	—	160	142	160	142	0

* The observations in this column, headed "By New Swinburne Wattmeter," were taken with the second wattmeter. See Section 17.

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Table XXXI.—Swinburne "Hedgehog" Transformer.

Primary volts, 2,400 (kept constant).
Secondary volts at no load = 102·0.

Results plotted in curves in Figs. 49 and 50, Plate 6.

Primary Current, I_1.	Secondary Current, I_2.	$\frac{I_2}{24}$.	Copper Losses in Watts.			Total Secondary Drop in Volts.	Primary Watts.		Power-Factor, $\frac{W_1}{W_1'}$
			Primary, $I_1^2 \times 7{\cdot}93$.	Secondary, $I_2^2 \times 0{\cdot}01612$.	Total.		Real, W_1.	Apparent, W_1'.	
1·194	0	0	11·3	0	11	0	151	2,866	0·053
1·199	1·02	0·045	11·4	0	11	0	272	2,878	0·095
1·214	3·06	0·127	11·6	0·1	12	0	469	2,914	0·161
1·217	5·19	0·216	11·7	0·4	12	0	696	2,921	0·238
1·281	8·00	0·333	13·0	1·0	14	0·1	984	3,074	0·320
1·342	12·08	0·503	14·3	2·2	17	0·2	1,401	3,221	0·435
1·442	16·03	0·670	16·6	3·9	21	0·3	1,804	3,461	0·521
1·549	20·00	0·833	19·0	6·1	25	0·5	2,182	3,718	0·588
1·661	23·87	0·995	21·9	8·6	30	0·6	2,583	3,986	0·65
1·721	26·08	1·088	23·5	10·3	34	0·8	2,784	4,130	0·67
1·855	29·73	1·239	27·3	13·3	41	1·0	3,178	4,452	0·71
2·030	34·35	1·431	32·8	17·8	51	1·2	3,682	4,872	0·75
2·218	39·29	1·639	39·0	23·3	62	1·5	4,101	5,323	0·77
2·392	43·86	1·827	45·4	29·0	74	1·9	4,571	5,741	0·80
2·577	48·58	2·026	52·7	35·7	88	2·1	5,033	6,185	0·81
2·692	51·38	2·141	57·2	39·8	97	2·3	5,297	6,461	0·82
2·800	53·96	2·248	62·2	44·0	106	2·5	5,584	6,720	0·83
2·953	57·68	2·403	69·1	50·2	119	2·8	5,947	7,087	0·84
3·059	60·26	2·515	74·0	55·0	129	3·0	6,225	7,242	0·86

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Table XXXII.

Efficiencies of Transformers Calculated from Curves of Total Losses. From Observations by Dynamometer Wattmeter.

Size and Description of Transformer.	Fractions of Full Secondary Load.												
	0	0·025	0·05	0·1	0·2	0·3	0·4	0·5	0·6	0·7	0·8	0·9	1·0
	Percentage Efficiencies.												
6 500-watt Westinghouse	0	61·8	75·9	85·7	91·9	94·0	95·1	96·0	96·3	96·6	96·8	96·9	96·9
6,000-watt Swinburne	0	48·4	65·2	79·0	88·2	91·8	93·7	94·8	95 5	95·9	96·1	96·1	96·1
3,000-watt ,, ...	0	39·3	56·2	71·7	82·9	87·6	89·8	91·2	92·2.	92·8	93·2	93·4	93·5
4,500-watt Thomson-Houston	0	49·1	65·4	78·8	87·8	91·1	92·9	93·8	94·2	94·6	95·0	95·0	94·7
20-H.P. 1892 pattern Ferranti	0	62·0	76·6	86·5	92·6	95·0	95·6	96·1	96·5	96·8	96·8	96·6	96·6
15-H.P. , ,, ...	0	65·5	79·0	88·1	93·4	95·0	95·7	96·0	96·2	96·3	96·1	95·8	95·5
15-H.P. ,, ,, ,, (re-wound) ...	0	55·0	70·8	83·0	90·5	93·1	94·5	95·1	95·4	95·5	95·4	95·2	94·7
6,000-watt Mordey	0	52·1	67·6	80·1	88·5	91·1	92·7	93·7	94·2	94·7	94·9	95·1	95·4
4,000-watt Kapp	0	39·5	56·5	72·3	83·8	88·0	90·4	91·9	92·6	93·3	93·8	94·0	94·2
5-H.P. 1885 pattern Ferranti (re-wound) ..	0	28·8	44·5	61·4	75·6	81·9	85·5	87·7	89·0	89·8	90·2	90·5	90·8
5-H.P. ,, ,, ,, ...	0	14·6	25·3	40·3	57·2	66·5	72·5	76·4	79·1	81·5	83·1	84·5	85·5

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12. The results of these numerous observations are set out graphically, and thus represented to the eye, in a series of diagrams, shown in Figs. 8 to 45 inclusive. For each transformer four diagrams have been prepared.

The first shows the "total lost watts" curve, or $W_1 - W_2$ plotted as a curve in terms of the corresponding secondary output W_2; and on the same diagram is given a curve showing the total copper losses ($C^2 R$) in the transformer in both circuits together. Hence the difference of the ordinates of these two curves gives the value of the losses other than those due to ohmic resistance in the transformer. The second diagram shows the value of the two currents, primary and secondary, plotted in terms of the secondary output. Since all the closed magnetic circuit transformers tested had a transformation ratio and windings in the ratio of 24 to 1, the primary current and 1-24th part of the secondary current are plotted together on the same scale. The third diagram gives the efficiency curve. The efficiencies in per cents., corresponding to definite fractions of the full secondary output, are calculated from the "total loss" curve and then graphically represented in diagrams, which are all drawn to the same scale. Hence they are all comparable, and we can see at a glance the percentage efficiency corresponding to loads of one-tenth, two-tenths, &c., of the full secondary load. The fourth curve is the "secondary drop" curve, and gives the "total observed drop," the "drop" due to secondary and to primary resistance, and the surplus or "leakage drop," due to leakage of induction, all plotted in terms of the secondary output. The efficiency values are tabulated in Table XXXII. for the different fractions of full load for all the transformers.

13. It is desirable to make one or two remarks about each transformer, and draw some general conclusions.

Ferranti Transformer, 1885 *type*, 5 *H.P.* (Tables IX. and X. Figs. 8, 9, 10, 11.)

It is obvious that this transformer is, in comparison with modern ones of the same size, very wasteful of energy in transformation. But we must bear in mind that in 1885 little or none

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of our present knowledge about iron core energy losses existed, and it is not to be wondered at that its design is imperfect, according to modern views. The immense core loss of 549 watts (540 by three-voltmeter method) makes it possess a low efficiency at all loads and gives it a flat efficiency curve. Its small copper losses, however, make it remarkably good in regulation, it having only 2·4 per cent. of total secondary drop. It is hardly necessary to say that Mr. Ferranti's present type of transformer is a vast improvement on these, and holds its own with the best now made.

Ferranti Transformer, 1885 (*re-wound*). (Tables XI. and XII. Figs. 8, 12, 13, and 14.)

The effect of doubling the length of both the primary and secondary circuit is shown in the curves of this transformer. The total internal losses are halved, but the copper losses and "secondary drop" about doubled.

Ferranti Transformers, 1892 *type*, 15 *and* 20 *H.P.* (Tables XV., XVI., XVII., XVIII., XIX., XX. Figs. 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29.)

These new transformers of Mr. Ferranti's are exceedingly good transformers. The total loss on open circuit of the 20-H.P. is only 1·3 per cent. of the full secondary output. The maximum efficiency is 97·1 per cent., and the efficiency at one-tenth of full load is 85 per cent., whilst the total secondary drop is only 2·1 per cent.

Two sets of curves are given for the transformer of 15-H.P. size. In two transformers tested there was a difference in the winding which resulted in the total internal loss being greater in one than in the other. In the transformer having the greatest total loss the copper losses (C^2R) are least, and the "secondary "drop" diminished, therefore, about 1 per cent. The efficiency curve of all these transformers is very square-shouldered, owing to the high efficiency (about 85 to 87 per cent.) at one-tenth of the full secondary load.

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Westinghouse Transformer, 6,500-*watt*. (Tables XIII. and XIV. Figs. 15, 16, 17, and 18.)

This transformer was one of the pair tested by Dr. J. Hopkinson by his elegant differential method. His results are recorded in the *Electrician* of June 24 and July 1, 1892. Employing a method depending upon the graphic description of the instantaneous curves of current and electro-motive force, Dr. Hopkinson deduced the power lost in the transformer at no load, quarter, half, and full load. Hence the corresponding efficiencies. The same quantities were given me independently as determined by the manufacturers from tests made at Pittsburgh. These are here collected with my own results for comparison. Dr. Hopkinson's results are placed in the column marked H, those of the manufacturers in column P, and my own in column F; and it will be seen that we all substantially agree in the values of the quantities measured.

Quantities Measured.	P.	H.	F.
Watts lost at no load	91	114	98
Watts lost at half load...	—	149	141
Watts lost at full load	—	236	209
Secondary drop at full load	2 %	2·2 %	2·4 %
Efficiency at half load	96·4 %	96 %	96 %
Efficiency at full load	97 %	96·9 %	96·9 %
Total copper losses (C^2R) at full load, in watts	104	—	93

It is clear, therefore, that there is no serious disagreement between us, and that this transformer is a very excellent instrument. Its maximum efficiency is just upon 97 per cent., and its efficiency at one-tenth of full load is nearly 85 per cent.

The loss in power at no load is by my figures 1·46 per cent. (by Dr. Hopkinson's 1·75 per cent.) of the full secondary output. The core of this transformer is made of a high quality of pure steel, the plates being slightly oxidised to insulate them for eddy-currents.

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Mordey Transformer, 6,000-*watt*. (Tables XXI. and XXII. Figs. 30, 31, 32, and 33.)

Two transformers of this size were tested. In one the three-voltmeter method showed the power loss at no load to be 125 watts; in the other the dynamometer wattmeter gave 148 watts. If we take the mean of these results, we get 136 watts as the open-circuit loss. This is 2·27 per cent. of 6,000 watts.

We found the "total secondary drop" between no load and full load to be 1·9 per cent. of the voltage at no load. In the official publications of the Brush Company the manufacturer's figures, for this sized transformer, for the open-circuit loss as a percentage of full load is given as 1·82 per cent., and the percentage total drop is 2·25 per cent., but these figures refer to the 6-kilowatt transformer worked at a frequency of 100 ∿ and transforming from 2,000 to 100 volts. As the transformer tested by us had a transformation ratio of 24 to 1 and worked at 83 ∿, it is a question how far the official figures may be applied to it; but, at any rate, our determinations agree with those of the manufacturer very fairly in assigning a value of not far from 2 per cent. both to the open-circuit loss and total secondary drop.

According to our observations this transformer takes rank as one of the best for regulation, having regard to its size and its open-circuit loss. The maximum total copper loss (C^2R loss) is 117 watts, and the maximum total loss at full load is 311 watts. The maximum efficiency is close to 96 per cent., and the efficiency at one-tenth of full load is 80 per cent. These figures are perhaps not quite comparable with those of the 20 to 1 6,000-watt transformer worked at 100 ∿, but they are, at any rate, not very different from the values of the similar quantities given by the manufacturers for this last transformer.

We shall deal more at length presently with the important question of the constancy of the iron losses at all loads, which the observations on this transformer bring out most clearly.

Thomson-Houston 4,500-*Watt Transformer*. (Tables XXIII. and XXIV. Figs. 34, 35, 36, and 37.)

There is nothing which here specially calls for remark. The

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transformer is a very good example of a high efficiency closed iron circuit transformer of small size. At one-tenth of full load the efficiency is 79·1 per cent. The open-circuit loss is only 1·6 per cent. of the total full secondary output.

Kapp 4,000-*Watt Transformer.* (Tables XXV. and XXVI. Figs. 38, 39, 40, and 41.)

This is a very interesting transformer, having an exceedingly good regulation for such a small size. Its total secondary drop is under 2 per cent. of its secondary voltage at no load, and its open-circuit loss is 2·8 per cent. of full output. It is curious in the rather slow rise of its power-factor as the secondary load is increased. More about this will be said presently. The good regulation of the transformer and small "leakage drop" are no doubt due to the effective "sandwiching" of the primary and secondary coils.

Swinburne 3,000-*Watt* "*Hedgehog*" *Transformer.* (Tables XXVII., XXVIII., and XXIX. Figs. 42, 43, 44, and 45.)

Lastly, but by no means least, there are several remarks to make about the "hedgehog" transformer of Mr. Swinburne. As a problem in transformer testing this open-circuit transformer of his is a hard nut to crack. From first to last we have made many hundreds of observations on it. The tables XXVII., XXVIII., and XXIX. give the results of the two best sets of observations with the dynamometer wattmeter on this transformer. The mean of a number of observations with the dynamometer wattmeter on the power taken up at no load by this transformer gave a value very close to 114 watts. We have already seen that the mean of the three-voltmeter and three-ammeter methods indicated probable values of the power loss close to this number. The dynamometer observations gave values of 114, 108, 108, 114, 114, 121, 121, in seven series of observations at slightly different frequencies. Hence the mean of all means of over 62 observations by three different methods gives the value of 112 watts for the power absorbed at no load in this 3,000-watt "hedgehog" transformer.

The core of this transformer is composed of soft iron wires

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18 inches in length, and each 0·0225 of an inch in diameter. The total volume of iron wire is about 856 cubic centimetres, and weighs 5,982 grammes. The induction in the centre of the core is about 10,000 C.G.S. units. It is clear that with such subdivision of the iron true eddy-current loss in the iron must be small, and my estimate of it is that it does not amount to more than 10 or 15 watts at the frequency used. Also, the true hysteresis loss cannot probably be much greater than 20 to 30 watts. In general one watt per 30 cubic centimetres of core is about the value of the true total core loss in most usual small-sized transformers. How comes it to pass, then, that all the measurements above described point to a loss of about 112 watts at no load? The answer to this question is, I take it, that there may be considerable eddy-current loss in the copper of the secondary circuit. In this transformer the secondary circuit is first wound on the core, and then the primary circuit over it. If we consider for a moment, we shall see that the lines of induction passing up the core return back through the space outside the core, and in so doing they must cut through the copper of the circuits. Hence, at each reversal of induction, unless the copper circuits are exceedingly well stranded, there are eddy-currents set up in the copper mass; and this takes place equally well at full and at no secondary load. The only way to prevent this waste is to make the secondary circuit of stranded wire, each strand slightly insulated and very small. It is perfectly clear that this loss must take place to some extent, and that it is a special defect of the open magnetic circuit transformer. We may, from one point of view, regard the function of the iron core of a transformer to be the preservation of the lines of induction in the proper place, and making them thread through the aperture of the secondary circuit without intersecting or cutting through the mass of the copper in that circuit.

In any case, it seems to me possible that in the "hedgehog" tested by us there may be a large eddy-current loss in the copper, and that this may explain the abnormally large loss on open circuit, which cannot well be accounted for in the iron core alone, unless phenomenally bad iron has been

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employed. I venture to suggest, therefore, that Mr. Swinburne should direct his attention to getting rid of this copper eddy loss, because, as it stands, this 3,000-watt "hedgehog" must be kept down in efficiency from this cause. If we compare the efficiency curve of this transformer with that of a closed magnetic circuit transformer of nearest size, we see that the open magnetic circuit so far gives no advantage in efficiency at the low loads. This "hedgehog" has an efficiency of about 73 per cent. at one-tenth load, and 94 or 95 per cent. at full load. A transformer with a closed magnetic circuit and of not much greater output can be built to do just as well, or better, than this, as can be seen by an inspection of the efficiency curves of the Kapp or Thomson-Houston transformers. If, however, the eddy-current loss in the copper can be entirely destroyed, and the large magnetising current provided by non-wasteful condensers, the open-circuit transformers may perhaps be made more efficient at low loads than closed-circuit transformers of equal output. To attempt to predetermine the efficiency of this type of transformer as now made by calculations carried out on the assumption that the only internal loss is the eddy and hysteresis loss in the iron, leads to considerable overstatement of the efficiency.

If subsequent experiments should confirm this view that at present there is considerable avoidable loss in the copper circuits of the "hedgehog," it is clear we are not yet at the end of our inquiries; and until it has been freed from this defect, it does not show inherent superiority over its closed-circuit rival. When it has arrived at the ideal condition, and enjoys the constant presence of a condenser imparting to it its necessary magnetising current, which in the 3,000-watt "hedgehog" is nearly 50 per cent. of the full-load primary current, we shall be in a position to give it a final verdict, and say if the result could not be just as well obtained by adding the necessary small amount of iron to complete the magnetic circuit.

Swinburne 6,000-*Watt* "*Hedgehog*" *Transformer.* (Tables XXX. and XXXI. Figs. 46, 47, 48, and 49.)

Subsequently to the date when the above paragraphs were in

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print, Mr. Swinburne submitted for test a 6000-watt open magnetic circuit transformer, the detailed observations on which are given in Tables XXX. and XXXI. We have here, then, a transformer which is directly comparable as regards output with the closed magnetic circuit transformers of Mordey or the Westinghouse Company. An inspection of the total loss curve for this 6000-watt "hedgehog" (Fig. 46) shows that the core losses, after subtracting the $C^2 R$ losses, are not quite constant, but slightly diminish as the load increases. The loss at no load in transformer is 156 to 160 watts, and the efficiency at one-tenth of full secondary load is about 77 per cent. A comparison of the efficiency curve of this transformer with that of a closed magnetic circuit transformer of equal output, such as the Mordey or Westinghouse, reveals the fact that the "hedgehog" has rather less efficiency at the low loads, but practically the same efficiency—about 96·9 per cent.—at its maximum. Hence, as far as this test goes, it shows that the open magnetic circuit transformer in itself is not superior to the equal-sized closed magnetic circuit transformer. The difference between the magnitude of the respective magnetising currents is, however, enormous. The "hedgehog" (6000-watt) has a magnetising current of 1·194, or nearly 1·2 amperes, at no load, or some 40 per cent. of its full-load primary current; whereas the similar-sized closed magnetic circuit transformer has a magnetising current of about 0·07, or some 3 per cent. of the full-load primary current. It has been frequently pointed out that although this large magnetising current in the case of the "hedgehog" is a "wattless" current, or current of small power-factor, yet it has many inconveniences, and is objectionable. The inventor accordingly suggested the association of a condenser with the transformer to supply the necessary current. Granted that the union of an appropriate condenser with the "hedgehog" transformer may in a given case make a transforming device in every way practical and perfect, yet the above experiments show that as far as it has been carried at present this condenser-transformer combination is no more efficient, or, rather, not quite so efficient a device as the much more simple closed magnetic circuit transformer.

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V.—General Remarks on the above Experiments.

14. On examining all those diagrams in which are delineated the curves of total loss of power in the transformer and the curve of total copper resistance loss ($C^2 R$), we see that in every case the upper curve runs *sensibly parallel* to the lower curve; in some cases most exactly parallel. In the curves for closed magnetic circuit transformers of good design the difference between the total $C^2 R$ loss and the total loss of all kinds is the iron core loss in the transformer. Hence we see that all the experiments lead to the conclusion that *the iron core losses are constant at all loads.* In no case have we found any substantial evidence that the iron core losses are evanescent at full secondary load. This last view has been held by Mr. Mordey and others. Seeing that our methods, when applied to the 6,000-watt Mordey transformer, give practically the same results at no load as Mr. Mordey obtains, it is an interesting matter to inquire how it comes to pass that the calorimetric method employed by him seemed to indicate that the whole loss in the transformer at full load was accounted for by the frictional losses in the copper. At present the writer can offer no explanation of this discrepancy. The above-described observations were all made and plotted out before the appearance of the paper of Professor Ayrton and Dr. Sumpner, and shown to many people, but it is very satisfactory to find that their undoubtedly excellent method has conducted them to precisely the same conclusion. Hence we seem to have very good reason to believe that the total internal loss curve of a transformer can be drawn from observations of the $C^2 R$ losses and the total core loss at no load.

When we come to consider it, it is very difficult to see on what valid supposition we could conclude that hysteresis loss should be less at full than at no load. The only difference in the magnetic conditions are that in one case the iron is magnetised by a small current acting in one direction, and in the other by the difference between two currents flowing in opposite directions, but governed by the condition that the total ampere-turns remain the same. In a special experiment, I have taken the hysteresis diagram for a soft iron wire employing a mag-

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netising force of 1,000 ampere-turns, in one case produced by 2·5 amperes and 400 turns, and in the other case by 20 amperes and 50 turns. The hysteresis areas for one complete cycle were found to be practically the same—the latter, if anything, the larger of the two. Prof. Ewing's direct evidence on this point will be remembered.

15. These measurements have brought out, in addition, another very interesting fact. In the foregoing tables for each transformer will be seen a column of figures headed *Power-Factor*. We have already defined this term as the ratio of the "true watts" to the "apparent watts," or product of the effective amperes and effective volts. This ratio is given for each measurement of the power. We see at once that for closed magnetic circuit transformers of good type, such as the Mordey, Westinghouse, Ferranti, &c., the power-factor at no secondary load begins at a value of about 0·8 or thereabouts. For such types of transformers, however, a very little loading up of the secondary circuit—not more than one-tenth of full load—brings the power-factor up to unity. In Fig. 46, Plate 5, are given three curves showing the progress of increase of the power-factor as the load on the secondary circuit is progressively increased. The upper curve represents the growth of power-factor (P.F.) for the 6,500-watt Westinghouse transformer. Beginning at 0·8, it rises up to unity at about one-tenth of full load. Hence at and after this very little load the apparent watts are the same as the true watts, and the real power taken up in the transformer is quite accurately given by the product of the primary terminal pressure and the primary current, effective, or $\sqrt{\text{mean}^2}$, values being understood. For an open magnetic circuit transformer like the "hedgehog" the case is quite different. The P.F. begins at a value of 0·08 or 0·06, and it never rises up above 0·8. Hence at no stage of the load is the real power taken up by the transformer anywhere nearly equal to the "apparent watts." A transformer like the 4,000-watt Kapp appears to occupy an intermediate position, and although it has a medium power-factor to start with, its P.F. rises up to unity at about half load. The importance of this fact in alternate-current station working is very great. It shows us, if we have a station wholly supplied with transformers of the

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type of Mordey, Westinghouse, Thomson-Houston, Ferranti, &c., that the apparent watts supplied to the transformers is the real watts at any hour when all the transformers are more than one-tenth loaded. If, however, the station is engineered with "hedgehogs," or with that museum of transformers of all sorts and makes which some central station engineers are fond of gathering together—under the idea, perhaps, that they can "hedge" for economy and make up for the vices of one kind by the virtues of another—then in hardly any state of the load will it be safe to assume the identity of the true and apparent measures of outgoing power. We must, of course, here assume that concentric primary cables are not in use, otherwise an error of another kind may he introduced. If we take the case of a station provided wholly with closed magnetic circuit transformers, and consider it as having an average kind of load diagram, say for the whole year,—then if the measure of the "apparent units" sent out from the station is taken, I do not think that in general we shall be far out in our estimate of the true units sent out if we deduct from the apparent units about 12 to 15 per cent. and call the remainder the measure of the true units of energy sent out in the whole time.

16. On looking at the curves in which are delineated the values of the primary and secondary currents for each transformer, we note that in these the value of the primary current and 1-24th part of the secondary current is plotted in terms of the secondary output. These curves are very nearly straight lines. In the case of the closed magnetic circuit transformers of large power-factor the difference between these current-values is constant.

Since the windings of these transformers are in the ratio of 24 to 1, it is obvious that the ordinates of the current-curves may be taken to represent the primary and secondary ampere-turns. If we look at the tables and examine the difference between the values of the primary current and 1-24th part of the secondary current, we shall find this difference is sensibly equal to the primary current at no load, or to the "magnetising current," so called. This is only another way of stating the fact that the resultant ampere-turns are constant at all loads and equal to the ampere-

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turns at no load. The above is true only for the high power-factor transformers. For the open magnetic circuit transformer it is not the case. In this last the difference between the primary and secondary ampere-turns diminishes as the load increases. This is probably because there is a continual change of the relative phase of the primary and secondary currents in the open magnetic circuit transformer as the load is increased; whereas in the closed circuit, after the first beginning of load, the primary and secondary currents come nearly into opposition with each other as regards phase, and preserve that difference at all greater loads.

There are also, as mentioned, for each transformer a set of curves showing the total secondary drop in volts between no load and any load. Consider the case at full secondary load. The P.D. between the secondary terminals is then less than that between them when the transformer is unloaded; primary terminal P.D. being kept always constant. The difference between these secondary P.D.'s is the "total secondary drop." Part of this drop is due to the volts lost by resistance in the secondary circuit, part by resistance in the primary, and the outstanding portion is the "leakage drop," or lost volts due to magnetic leakage. In all of the "drop" curves a line is drawn in to indicate the "drop" due to secondary resistance, and in some a dotted line is added also to indicate the drop due to the primary resistance. The relation of these parts to the whole drop in volts may be examined as follows for a closed magnetic circuit transformer:—

Let v_1 be the instantaneous value of the potential difference of the primary terminals, and v_2 the same for the secondary terminals, of the transformer. Let i_1 and i_2 be the instantaneous values of the currents, and let R and S be the primary and secondary circuit resistance, and N_1 and N_2 the primary and secondary turns. Then let b_1 be the induction density in the centre of the primary coil, and b_2 that in the secondary, and b the density of the leakage; or, if A is the constant cross section of the magnetic circuit, let $A\,b_1$ be the lines threading through the primary coil, $A\,b_2$ those through the secondary coil, and $A\,b$ the leakage of lines. Then,

$$A\,b_1 = A\,b_2 + A\,b \quad \ldots \quad \ldots \quad (1)$$

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By fundamental equations,

$$\left.\begin{aligned} v_1 &= \mathrm{R}\, i_1 + \mathrm{A}\, \mathrm{N}_1 \frac{d b_1}{d t} \\ \text{and} \qquad o &= \mathrm{S}\, i_2 + v_2 + \mathrm{A}\, \mathrm{N}_2 \frac{d b_2}{d t} \end{aligned}\right\} \quad \ldots \quad \ldots \quad (2)$$

Hence, writing a for $\frac{\mathrm{N}_1}{\mathrm{N}_2}$, we have, by elimination of b_1 and b_2 between (1) and (2),

$$\frac{v_1}{a} + v_2 + \mathrm{S}\, i_2 - \mathrm{R}\, \frac{i_1}{a} = \mathrm{A}\, \mathrm{N}_2 \frac{d b}{d t} \quad \ldots \quad (3)$$

Let $v_1 = \mathrm{V}_1 \sin p t$: then $v_2 = -\mathrm{V}_2 \sin p t$, because the primary and secondary potentials are in opposite phases. Also, we can write $i_1 = \mathrm{I}_1 \sin p t$, and $i_2 = -\mathrm{I}_2 \sin p t$, at all stages of the secondary load very nearly, because the primary and secondary currents are in opposite phases, and the primary current is in synchronism with the primary P.D. at all stages of the load above one-tenth. Also, we can write $b = -\mathrm{B} \sin p t$, because the "leakage" is determined by and is in close synchronism with the secondary current. Hence we arrive, by substitution in (3), at the equation,

$$\left(\frac{\mathrm{V}_1}{a} - \mathrm{V}_2 - \mathrm{S}\, \mathrm{I}_2 - \frac{\mathrm{R}}{a} \mathrm{I}_1\right) \sin p t = -\mathrm{A}\, \mathrm{N}_2\, p\, \mathrm{B} \cos p t \quad (4)$$

The quantity $\mathrm{A}\, \mathrm{N}_2\, p\, \mathrm{B} \cos p t$ is the instantaneous value of the volts lost by leakage. When the secondary circuit is open, the secondary current, I_2, is zero; the P.D. at the secondary terminals has a value whose maximum we may denote by ${}_0\mathrm{V}_2$; and the primary current, then called the "magnetising" current, has a value the maximum of which may be called ${}_0\mathrm{I}_1$. If the secondary circuit is fully loaded up, then the P.D. at the secondary terminals drops to a value whose maximum may be represented by ${}_f\mathrm{V}_2$, and the primary and secondary currents have maximum values respectively I_1 and I_2. Substituting these values for the two cases of no load and full load on the secondary circuit in the equation (4) successively, and then subtracting the two resulting equations from each other, and afterwards taking the $\sqrt{\text{mean}^2}$ value of both sides,* and putting dashes over the

* This symbol, $\sqrt{\text{mean}^2}$, is a convenient abbreviation for the long phrase, "taking the square root of the mean of the squares of all the instantaneous values of the periodic quantity at equal short intervals of time throughout the period." It has been called the *effective* value.

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letters to indicate that they now stand for the values of the currents and potentials as read on alternate-current ammeters and voltmeters, we get the equation,

$$({}_0V_2' - {}_fV_2') - \left(S\,I_2' + \frac{1}{a}R\,(I_1' - {}_0I_1')\right) = A\,N_2\,p\,B.$$

Since ${}_0V_2'$ is the value of the secondary P.D. at no load, and ${}_fV_2'$ is that at, say, full load, the first term in the bracket is the "total drop." The second term in the bracket is the sum of the copper drop due to the secondary circuit and that due to the primary circuit, and the term on the right-hand side of the equation is the drop due to leakage. Hence, if we have a closed magnetic circuit transformer with windings, say, in the ratio of 24 to 1, we have the following rule for obtaining the true magnetic leakage drop, or volts lost by leakage at full load :—Add together the product of secondary resistance and secondary current and 1-24th part of the same product for the primary circuit, after deducting from the primary current at full load the "magnetising" current. Subtract this sum from the total observed drop, and the remainder is the secondary volts lost by magnetic leakage.

Examining in this way all our transformers, we see that in some, such as the Westinghouse, half the observed drop is due to magnetic leakage; whilst in some, such as the Kapp transformer, magnetic leakage is very small. This is no doubt due to the way in which the primary and secondary coils are interplaced in the Kapp transformer.

From what has been said above, it is clear that we can, from certain figures of observation on a transformer, predetermine all these curves when that transformer is of the closed magnetic circuit type. For if we are given—

(*a*) The primary P.D. of terminals;

(*b*) The primary current at no load, commonly called the "magnetising current;"

(*c*) The primary and secondary circuit resistances;

(*d*) The P.D. of secondary terminals, and the secondary current corresponding to various secondary loads up to full load;—

we can determine the efficiency curve and everything else.

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For the product of (*a*) and (*b*) gives us the apparent watts at no load, and if we take 0·8 of this we get the core loss at all loads. Then from the ratio of the primary to secondary volts at no load, and the observed secondary currents and magnetising current, we can calculate the primary currents, and from the currents and resistances get the total C^2R loss at certain secondary loads. Hence we can draw a total C^2R loss curve and a total loss curve parallel to it, and from this last determine the efficiency at every stage of the load.

VI.—Experiments with Mr. Swinburne's Wattmeters.

17. As mentioned, Mr. Swinburne sent with his transformer a brass-cased wattmeter stated to be adapted for measuring alternating-current power. In most of the tables giving the power absorbed by various transformers will be found a column headed "Total Power taken up in Trans-"former by Swinburne Wattmeter," and in the "total loss" curves these numbers are plotted as a dotted line, marked "Losses by Swinburne Wattmeter." In various cases we made measurements with this Swinburne wattmeter, as far as it would read, and our own dynamometer wattmeter, either simultaneously or immediately after one another. The Swinburne wattmeter was carefully calibrated on the same non-inductive resistance as the dynamometer wattmeter, and its constant determined. An examination of the values of the power taken up by various transformers at various loads shows that the power values given by the first sent Swinburne wattmeter are in nearly all cases lower, and often much lower, than those given by the dynamometer wattmeter. In the case of the Westinghouse transformer the two instruments gave practically the same value for the loss at no load; and for that transformer, and for the 15-H.P. Ferranti transformer, the differences in the two readings are not great at any load. It seemed that the Swinburne wattmeter gave readings, within reasonable limits of experimental errors, quite as likely to be right as not. The case is quite different, however, when it is applied to measure the power-absorption in a transformer of small or moderate

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power-factor. In the case of the Thomson-Houston transformer the Swinburne wattmeter gave power values only about two-thirds as great as those given by the dynamometer; in the case of the Kapp transformer rather less. Most remarkable of all, in the case of Mr. Swinburne's own transformer, his first sent wattmeter gave power values on the average only about half of those obtained by the dynamometer wattmeter. Again and again we repeated these experiments, calibrating the wattmeters in various ways, and using every precaution. The mean of all our observations with the Swinburne wattmeter gave 74 watts as the power taken up in the 3,000-watt "hedgehog" at no load, and the most frequent value obtained was 55 watts. We have seen that the mean of all other methods gave 114, and the most trustworthy observations by the dynamometer gave 121 watts. Here, then, was a riddle to be explained. It was only susceptible of two solutions. Either the dynamometer, three-voltmeter, and three-ammeter results for the "hedgehog" were greatly in excess of the true value of the power absorbed, or else Mr. Swinburne had achieved the marvellous feat of inventing a wattmeter which, whilst reading approximately correctly the power taken up in transformers of good closed magnetic circuit type, measured too little by nearly 50 per cent. when applied in the same way to test his own.

18. It was felt to be necessary to examine this matter with the greatest care. Experiments were therefore made with three small "hedgehogs" which had been made for lecture purposes. These transformed from 100 to 33 volts. These three little transformers were arranged with their primary circuits (100 volts) in parallel, and with their secondary circuits open. The power taken up in the three together at 100 volts was measured by the three-voltmeter method, the dynamometer wattmeter, and with the Swinburne wattmeter. These experiments were carried out as follows:—In series with the transformers was placed a couple of incandescence lamps as a non-inductive resistance, and three Kelvin multicellular electrostatic voltmeters were employed in a three-voltmeter method to measure the power absorbed in the transformers. The resistance of the lamps was obtained by a

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measurement of the current passing through the lamps, using either a Kelvin ampere balance or a dynamometer placed behind the voltmeters. With these arrangements, and using a very steady pressure, and such ratios of the potential differences as would give the best results, we obtained 11 measurements of the power absorbed at no load by these three little "hedgehogs." These results are tabulated in Table XXXIII. Then, next, both the Swinburne wattmeter and the dynamometer wattmeter were carefully standardised on an incandescence lamp working at 100 volts; measuring first the true power taken up in the lamp by volts and current, and then using this value to obtain the constant of the wattmeters. The standardised wattmeters were then set to measure the power taken up in the three small transformers. The results are given in Table XXXIV.

Table XXXIII.

Experiments on Power absorbed by Three Small "Hedgehog" Transformers at No Load, taken by Three-Voltmeter Method at 100 volts.

Current through Transformers, in Amperes.	Readings of Electrostatic Voltmeters (Kelvin).			Resistance in Series with Transformers, in Ohms.	Power in Watts absorbed by the Three Transformers.
	V_1.	V_2.	V_3.		
2·02	100	93·6	143·0	46·3	18·2
2·02	100	93·9	143·5	46·5	19 1
2·02	100	94·0	143·0	46 5	17·1
2·02	100	93·9	143·4	46·5	18·9
2·02	100	93·9	143·0	46·5	17·6
2·02	100	94 3	144·0	46·7	19·4
2·07	100	95·2	144·7	45·9	20·4
2·06	100	95·4	144·6	46·2	19 5
2·096	100	95·5	145·0	46·1	20·7
2·092	100	96 7	145·8	46·2	20·5
				Mean power in watts,	19·26

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Table XXXIV.

Experiments on the Power absorbed by Three Small "Hedgehog" Transformers at No Load, taken by Dynamometer and Swinburne Wattmeter at 100 volts.

(Wattmeters both standardised on an incandescence lamp taking 42 watts.)

Volts on Terminals of Transformers.	Scale Readings of		Power absorbed in Watts by the Three Transformers.	
	Dynamometer Wattmeter.	Swinburne Wattmeter.	By Dynamometer Wattmeter.	By Swinburne Wattmeter.
100	15·0	28·5	19·1	12·6
100	15·1	28·5	19·2	12 6
100	15·1	28 8	19·2	12·7
			Mean 19·16	Mean 12·61

Collected Results for the Mean Value of Power absorbed by the Three Small "Hedgehogs" arranged in Parallel.

By three-voltmeter method	19·26 watts.
By dynamometer wattmeter	19·16 ,,
By Swinburne wattmeter	12·61 ,,

We see that the mean value of the power taken up as measured by the three-voltmeter method agrees quite well with the value obtained by the dynamometer wattmeter, but that the value obtained by the Swinburne wattmeter is nearly 34 per cent. lower than the other two.

These experiments, which were very carefully made, convinced me that the cause of the discrepancy must be looked for in the Swinburne wattmeter.

Mr. Swinburne sent for use with his transformer a condenser suitable for putting upon the primary circuit in parallel with the primary circuit of the "hedgehog." We therefore applied the two wattmeters to measure the power taken up (1) in the condenser alone, (2) in the transformer alone, and (3) in the condenser and transformer on open secondary circuit when the

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condenser was joined in parallel with the primary of the transformer. The primary pressure was 2,400 volts in all cases. When the condenser was across the circuit alone, the current flowing into it was 0·458 ampere, as measured by a Kelvin balance. Since the frequency was 83, this shows the capacity of the condenser was about 0·36 of a microfarad. The magnetising current, or open-circuit current, of the 3,000-watt "hedgehog" was 0·761 ampere; and when "hedgehog" and condenser were joined in parallel the current flowing into them jointly was 0·283 ampere. The Swinburne wattmeter and dynamometer wattmeter being both standardised, 12 measurements were taken with each instrument—four of the power absorbed in the condenser, four of that absorbed in the transformer with open secondary, and four of the condenser and transformer joined in parallel. The results are given in Table XXXV.

Table XXXV.

Experiments on the Power absorbed in Condenser and "Hedgehog" Transformer separately and together, taken by Two Wattmeters at a primary pressure of 2,400 volts.

(*a.*) *Power absorbed in Condenser alone.*

By Swinburne Wattmeter.		By Dynamometer Wattmeter.
44·8 watts.	...	18·0 watts.
44·0 „	...	21·0 „
50·0 „	...	30·0 „
47·0 „	...	27·0 „
Mean ... 46·4 „	Mean ...	24·0 „

(*b.*) *Power absorbed by "Hedgehog" at No Load.*

By Swinburne Wattmeter.		By Dynamometer Wattmeter
41·5 watts.	...	120·0 watts.
44·0 „	...	120·0 „
36·0 „	...	108·0 „
33·0 „	...	112·0 „
Mean .... 39·0 „	Mean ...	115·0 „

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(c.) *Power absorbed by Condenser and same Transformer in Parallel.*

By Swinburne Wattmeter.		By Dynamometer Wattmeter.
96·3 watts.	...	144·0 watts.
95·0 „	...	144·0 „
100·0 „	...	150·0 „
95·0 „	...	140·0 „
Mean ... 96·5 „	Mean ...	144·0 „

It is clear, therefore, that whilst relatively to the dynamometer wattmeter Mr. Swinburne's first wattmeter reads too low on his transformer, it reads too much on his condenser, and he has been overrating the power taken up in the condenser to about the same extent as he has been underrating that taken up in the transformer.

On thinking over these results, it became evident that they could be accounted for on the supposition that the current through the shunt circuit of the Swinburne wattmeter was *advanced* in phase as compared with that of the primary impressed potential difference between the ends of that circuit. The correctness of the wattmeter as a power-measuring instrument depends upon the current in the shunt circuit being exactly in step, or consonance, with the periodic potential difference at the ends of that circuit. When the wattmeter is applied to the open circuit-transformer, the current through the transformer, which is also the current in the series coil of the wattmeter, lags behind the P.D. at the primary terminals of the transformer. Hence, if the current through the wattmeter shunt circuit (which should represent that P.D. in phase and magnitude) is advanced in phase, the two currents (series and shunt) in the wattmeter will have a greater difference of phase impressed upon them, and hence the mean product will be diminished. Again, if the wattmeter is applied to a condenser, the current through the condenser is normally in advance in phase of the P.D. at its terminals. Hence, by the same reasoning, any cause tending to advance the phase of the current through the shunt circuit of the wattmeter will bring the shunt and series currents more into step and increase the mean product. There-

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fore the wattmeter reading will be too little on a transformer and too great on a condenser.

It is obvious that this advancement of phase of the current in the shunt circuit of the wattmeter could be produced by *capacity* in that circuit.

The resistance of the shunt circuit of the Swinburne wattmeter is about 100,300 ohms; of that, the movable coil has 300 ohms resistance, and the 100,000 ohms is the resistance of the doubly wound fine wire in two brass boxes, which are joined in series with the bobbin. Experiments were then made to see if we could, by artificially increasing the capacity of either of these parts of the shunt circuit, exaggerate the defect, and, if so, whether the readings of the wattmeter on the "hedgehog" were more altered than readings on a non-inductive resistance.

Exp. 1.—A small Leyden jar of 1 pint size, about 0·001 or 0·0005 of a microfarad in capacity, had its inside and outside coatings connected to the ends of the external resistance (in brass boxes) of the shunt circuit. So arranged, the Swinburne wattmeter was applied to measure the power taken up in the 3,000-watt "hedgehog" on open circuit and in our non-inductive resistance. With the jar removed, the deflection of the wattmeter was 5 divisions, corresponding to about 80 watts taken up in the transformer. With the jar added, the deflection fell to 0·3, or 5 watts. No such change was produced by adding the jar when the power being measured was being taken up in the non-inductive resistance. It is evident, therefore, that capacity added to the shunt circuit still further reduces the wattmeter readings.

Exp. 2.—In order to find out, if possible, whether the prejudicial capacity existed in the external resistance in the brass boxes or in the internal resistance in the movable bobbin of the shunt circuit, the brass boxes were removed and their place taken by a resistance of 34,000 ohms, made up of the filaments of high-resistance incandescence lamps. It seemed clear that a row of lamps could have no sensible capacity. The readings of the wattmeter were then compared when the shunt circuit external resistance consisted of 34,000 ohms made up of lamp filaments, and 100,000 ohms made up of doubly coiled wire in brass boxes, being

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taken both on the 3,000-watt "hedgehog" and on a 15-H.P. Ferranti transformer. No greater difference was found in the power measurements in the two cases than might be due to errors of reading. For the "hedgehog," the power reading with the external shunt resistance formed of lamps was 79 watts, and 81 watts when using the coiled wire resistances in the brass boxes. For the other transformer values of 125 and 130 watts were found. Since we have good reason to believe that the true value of the power given to the 3,000-watt "hedgehog" is 114–120 watts, and that to the 15-H.P. Ferranti is 150 watts, when on open secondary circuit, we see that the substitution of lamps for the brass box resistances did not appreciably increase the abnormally low reading of this wattmeter, and therefore the cause of this low reading could not be capacity in this external resistance in the brass boxes. Hence it appeared to be more probable that the capacity giving rise to this diminished power reading might be due to the small electrostatic capacity of the tightly coiled wire bobbin of very many turns which forms the suspended circuit of the wattmeter, and that this capacity advances the phase of the current in the shunt circuit, so that instead of being exactly in step with the P.D. on the terminals of the transformer it is slightly in advance of it.

Subsequently to the date of the experiments above recorded, Mr. Swinburne sent a second wattmeter of similar character, but differing from the first sent in that it had an ebonite outer case and the metal frame carrying the fixed or series coil was insulated at the top and bottom by ebonite distance pieces. This second wattmeter was carefully compared with the first one and with our own dynamometer wattmeter. The three wattmeters were standardised, and their constants determined by noting the instrumental readings when the instruments were set to read the known power taken up in the standard inductionless platinoid resistance arranged to absorb 600 watts at a P.D. of 2,400 between its terminals. The so-calibrated wattmeters were then set to read the power taken up in the 3,000-watt "hedgehog" on open circuit, the Swinburne condenser, the old Ferranti 1,850-watt

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transformer, the Kapp and Thomson-Houston transformers. The results are given in the table below.

Table XXXVI.

Comparison of Wattmeter Readings on various Circuits.

Reading of Dynamometer Wattmeter, in Watts.	Readings of Swinburne Wattmeters, in Watts.			Object Tested.
	Second Pattern.	First Pattern.		
		Case on.	Case off.	
600	600	600	600	Standard resistance.
114	106	57	68	3,000-watt "hedgehog."
15	20	46	—	Condenser.
114	99	83	—	Thomson-Houston transformer.
156	154	112	—	Kapp transformer.
303	282	—	—	Ferranti transformer.

It is clear from the above observations that the second wattmeter with ebonite case agrees much more nearly with the dynamometer wattmeter than does the first having a brass case. The differences between the second wattmeter and the dynamometer are quite within the limits of errors of observation, seeing that the instrumental reading for 15 watts is only about 0·5 of a scale division on all instruments. It is evident, therefore, that the second wattmeter sent by Mr. Swinburne is a very different instrument from the first one. The writer is unable to say whether it differs in any way from the first one except in having an ebonite instead of metal case and in having the metal supports of the series coil insulated, but it is obvious that the displacement of phase of the shunt current no longer takes place. According to the view above stated, this displacement is the consequence of a capacity effect. It is difficult to see how eddy-currents set up in any metal near the movable coil could create the observed defects in accuracy; because, although these might weaken the resultant

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field in which the movable bobbin hangs and so make the wattmeter readings too low, we have to account also for the fact that the first wattmeter reads too much on a condenser. It is evident, however, that the second wattmeter Mr. Swinburne sent me is a fairly accurate instrument, whereas the first one is quite unreliable on circuits of small power-factor.

Before we christen a wattmeter a "non-inductive wattmeter," it should be carefully tested on inductive circuits of small power-factor and large capacity or inductance.

VII.—General Remarks on the Use of Dynamometer Wattmeters to Measure Alternating-Current Power.

The foregoing experiments impressed upon us the necessity for caution in concluding that any wattmeter of the dynamometer pattern, and however made, would give correct readings of the alternating-current power taken up in an inductive circuit. If we consider a simple harmonic current, I, in, and potential difference, V, at the ends of such an inductive circuit, then the mean value of power, W, taken up in the circuit is $\frac{1}{2}$ I V cos ϕ, where I and V are the maximum values during the period, and ϕ is the phase difference of I and V. If ϕ varies, then the rate at which the measurements of the power W is varied is expressed by $d\,W / d\,\phi = -\frac{1}{2}$ I V sin ϕ, and the ratio of $d\,W / W$ is therefore equal to $-\tan\phi\, d\,\phi$. Hence a small change in ϕ produces a greater effect on the value of the error in measuring the resulting power in proportion as ϕ increases from 0° to 90°.

In the case of an open-circuit transformer there is a very considerable difference of phase between the primary current at no secondary load and the difference of potential between the primary terminals.

Accordingly, if a wattmeter of the dynamometer type is applied to measure the power being taken up in such a transformer at no load, there is a correspondingly great difference between the phases of the currents in the shunt and series coils of the wattmeter. If, then, either by reason of capacity or inductance in the shunt circuit, the phase of the shunt current is displaced by a

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small amount, the percentage change so introduced into the mean product of the currents—*i.e.*, into the wattmeter reading—is much larger than if the instrument was being used on a transformer of large power-factor.

Hence in the employment of such a wattmeter it is desirable that the movable part of the shunt circuit should have very few turns, and that all metal framework near it be avoided. Generally speaking, it will then be difficult to bestow upon the shunt coil sufficient magnetic moment unless the current in it is comparatively large. This means that considerable power will be dissipated in the wattmeter itself. It is better, however, to employ a wattmeter which uses up a few horse-power and obtain accurate results with it, than reduce its shunt current to a small value and introduce sources of error. It is as necessary to avoid capacity as to annul self-induction in the shunt circuit. As to the external non-inductive resistance in series with the shunt coil, we have, as seen, always preferred to employ a series of incandescence lamps. There is no difficulty in doing this when the means exist for keeping the P.D. between the primary mains constant. Large circuits of incandescence lamps in series are, however, rather unwieldy and expensive. I have been experimenting lately with other methods to obtain the necessary non-inductive resistance for the external shunt circuit of the wattmeter cheaply and in a compact form. The conclusion arrived at, however, is that, with proper precautions, a suitable dynamometer wattmeter is a very useful instrument to employ for measuring in a practical, workshop-like way the power being taken up in inductive circuits, and that it is much more easy to manage than any of the many other complex methods which look so well on paper and are so difficult to carry out satisfactorily in real life. In any case, a practically non-inductive metal wire resistance is necessary wherewith to standardise the wattmeter *in situ* and before use; and if the movable coil has very few turns, and no metal near it, and the external shunt circuit is of proper kind, excellent results in measuring alternating-current power may be obtained with it.

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VIII.—Experiments on the Magnetic Behaviour of Transformers at the Moment of Connecting them to a Circuit.

More than fourteen months ago I carried out a series of experiments and observations on the behaviour of transformers at the moment of switching them on to a "live" circuit, and which are, I venture to think, of some practical importance. They arose out of facts which came to my notice regarding instances of sudden failure of transformers at the instant of closing the primary switch. Transformers have been known to be pierced or to break down on switching on after having worked well for a long time. To investigate the behaviour of transformers at the moment of closing the primary switch and connecting them to a high-pressure "live" circuit, the following experiments were tried:—

Exp. 1.—The secondary circuit of a 4-inch spark induction coil (kindly lent by Mr. Apps) was closed by a vacuum tube of a very sensitive kind. It had long overlapping platinum electrodes. The ordinary primary coil with iron core was removed, and its place filled by a bobbin of 12,000 turns of insulated copper wire wound on a wooden core. This primary coil could be slid into or out of the secondary coil. The primary coil was placed in series with the primary circuit of a 40-H.P. transformer, the "magnetising current" of which was 0·137 ampere. When the transformer had its secondary circuit open, this magnetising current in the primary flowing through the primary coil of the induction bobbin just sufficed to keep up a glow, visible in the dark, in the vacuum tube. The primary coil of the induction bobbin was then withdrawn from the secondary until the glow in the tube just disappeared. Under these circumstances the vacuum tube was an exceedingly delicate detector of any sudden increase in the primary current. If the primary circuit of the transformer was then suddenly and frequently opened and closed by a switch, it was found that at intervals, on some occasions of closing the primary switch, the vacuum tube flashed up brilliantly, thus showing that a rush of current far exceeding the normal magnetising current took place into the transformer on some occasions when the primary switch was closed.

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Exp. 2.—Another way of detecting this rush of current is to insert in the primary circuit of the transformer an incandescent lamp, which is brought to proper incandescence by the normal magnetising current of the transformer. Thus, in one of our experiments, a 10-candle incandescent lamp, taking 0·33 ampere, was placed in the primary circuit of a 5-H.P. transformer, the magnetising current of which was 0·35 ampere. When the secondary circuit of the transformer was left open and the primary switch suddenly closed, on most occasions the lamp came quietly to normal incandescence; but on some occasions, on closing the primary switch, the lamp flashed up brilliantly, so much so as to blacken the glass, and in some cases even to break the filament. This experiment showed in a striking manner the sudden rush of current into the transformer on closing the primary switch.

Exp. 3.—In another experiment, a Kelvin centiampere balance was inserted in the primary circuit of a 40-H.P. transformer, of which the normal magnetising current was 0·137 ampere. The balance was adjusted so as to be in equilibrium when this current was passing, and the primary current was then switched on frequently. On some occasions the balance was thrown up violently, even when the weight was moved into positions corresponding to 5 or 10 amperes. In one case, the weight being at the position corresponding to 10 amperes on the balance, the balance was thrown up violently at the third switch-on. On other occasions, the weight being at the position corresponding to 10 amperes, the balance was thrown up at the 5th, 11th, and 13th time of switching on, whilst on the other occasions it was not moved. In one instance the beam of the balance was thrown up so violently that the weight, having a mass of about 66 grammes, was completely thrown off the scale pan; thus indicating that on that occasion an immense rush of current passed in the transformer at the moment of closing the primary switch, but which current sank again almost instantly to the steady value of 0·137 of an ampere.

Exp. 4.—The safest and by far the most convenient method of showing this rush of current was found to be the employment of a simple form of dynamometer placed in the primary circuit.

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Two flat coils were constructed of No. 20 covered copper wire, each coil having 23 turns coiled up as a sailor coils a rope, the external diameter of the coil being 5½ inches. These flat coils were suspended by silk ribbons on a vertical plane, facing one another. The coils had straws cemented to them, which hung down like the leaves of a gold-leaf electroscrope. By means of flexible connections, the primary current of the transformer could be passing through both coils in series in such a direction that the coils tended to repel one another when the current was passing. The weight of the coils was such that when the normal magnetising current of the transformer was passing the repulsion was hardly visible. On switching on and off the primary current a great many times, on certain occasions the coils were flowing widely apart, so that the straws, which were about 8 inches long, diverged from one another like the leaves of a gold-leaf electroscope when an excited glass rod is brought near them; the divergence being such as to cause the tips of the straws to separate about 6 inches. This arrangement could be conveniently inserted in the primary circuit of any transformer, and adjusted so as to show the sudden rush of current on some occasions when the primary circuit was closed.

Exp. 5.—On those occasions when the sudden rush into the transformer took place, a violent thump, or knock, was heard in the transformer. The suggestion was made by Mr. Partridge to employ a wooden rod pressed against the transformer in order to listen to the internal sounds. Accordingly, a transformer stethoscope was constructed, consisting of a rod of deal with a piece of ebonite on one end and a circular disc of pine on the other. By pressing the ebonite against the iron core of the transformer and putting the ear against the wooden disc, it was found possible to study these internal noises in the transformer. On listening to the character of the sound made on switching on a transformer, it was found that on some occasions there was hardly any noise; on others, a slight knock, or thump, or loud tick; and on others, a still more violent noise; and the violent noise always occurred when the other appliances showed a sudden rush of current taking place into the transformer. It is clear, therefore, from these experiments that

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something peculiar happens on some occasions on closing the primary switch of a transformer having an open secondary circuit, at the moment when the primary current is connected to the "live" circuit. On considering the matter carefully, it became evident that the conditions under which the transformer is disconnected from the circuit must determine what happens when that transformer is re-connected to the circuit. If the current is flowing into the primary circuit of a transformer, and the switch is opened, as long as the arc continues at the switch, so long does the alternating current produce in the iron core an alternating magnetism; but, at some instant, the arc breaks suddenly, and the primary current is then interrupted. The transformer, therefore, is left magnetised in some particular phase—either strongly magnetised one way or the other, or, perhaps, not magnetised at all, depending on the phase of the current when the current finally ceases. It is clear that on switching on the current again the switch must be closed in some particular phase of the pressure, and that, therefore, the in-coming current finds the iron of the transformer already in a magnetic condition. It may be that the magnetism of the iron is in such a direction that the current is able to produce instantly the necessary counter electro-motive force to keep down the current to its normal mean value; but it may be that the magnetism of the core is in such a direction that the first change which takes place in the magnetic condition of the core is one which, so far from introducing a back electro-motive force into the primary circuit, actually assists the current. In other words, the self-induction of the transformer is non-existent for the first moment, and until the current has got the core into proper magnetic condition, in proper relative step with itself, there is not sufficient impedance in the primary coil to prevent the impressed electro-motive force making just whatever current it can, so far as the true, or "ohmic," resistance of the primary circuit will allow. Under these conditions, therefore, it is evident that there must be a great rush of current into the transformer, until the impedance is built up by the magnetism of the core falling into proper relative step with the current.

Exp. 6.—It was considered advisable to find out whether this

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sudden rush of current into the transformer was accompanied by an increased potential difference between the primary terminals of the transformer; accordingly, the induction coil and vacuum tube employed in Experiment 1 were arranged so that the primary circuit of the induction coil was in series with a non-inductive resistance placed across the primary mains, in parallel with the primary circuit of the transformer. It was expected that if a rise of pressure took place on switching on the transformer, the vacuum tube would indicate it by flashing up; but the experiment failed to indicate any rise of pressure, even when the primary coil of the induction coil was adjusted as carefully as possible to show any increase in pressure between the primary mains.

Exp. 7.—The following method was then tried:—Two brass balls, about 1 centimetre in diameter, carried by horizontal screws, having 24 threads to the inch, passed through fixed nuts on an insulating stand. One of these screws had a large divided ebonite head, or arm, to serve both as a handle and an index. This discharger was placed in series with a primary fuse across the primary mains, in parallel with the transformer. The balls were then screwed up until just in physical contact, and afterwards separated by such a distance that the alternating pressure employed between the primary mains—namely, 2,400 volts—just could spark across the distance. This sparking distance was found to be about three-quarters of a millimetre. Having determined this sparking distance, the balls were further separated by such a small amount as just to prevent the normal pressure from sparking across. This arrangement being kept in parallel with the primary circuit of the transformer, the primary current was then switched on and off a good many times, and on some occasions the spark jumped across the spark balls at their increased distance, thus showing an increase of pressure. The greatest distance between the balls across which the spark was found capable of jumping was 0·84 of a millimetre. Other experiments were made to determine what alternating pressure would jump across this increased distance. It was found to be something not exceeding 4,000 volts.

The above experiments were varied in many ways, but they

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all went to prove that at the instant of switching the transformer with an open secondary circuit on to the primary mains, a sudden rush of current may take place into the transformer, and a sudden rise of pressure at the terminals of the transformer.

Exp. 8.—It was deemed advisable to ascertain whether the rush of current into the transformer was a continuous rush—that is to say, a rush all in one direction, lasting over a certain fraction of a second—or whether it was an alternating rush—that is to say, a great increase of current, but at the same time maintaining the normal alternations. In order to settle this point, the flat coils before described in Experiment 4 were arranged so that the primary current of the transformer flowed through one coil, whilst the other coil was traversed by a continuous current produced by a secondary battery. If the rush of current into the transformer is a current always in the same direction, lasting a sensible fraction of a second, it was thought probable that a repulsion would take place between the two coils, one traversed by the steady current, and the other traversed by a current into the transformer; but no such repulsion was found under any circumstances.

It seems probable, therefore, that this rush of current is an alternating rush, and that it is simply due to the fact that until the magnetisation of the core has settled down into the proper condition, the impressed alternating electro-motive force produces whatever current it can through the primary coil, being only limited in so doing by the true resistance of the primary coil.

Exp. 9.—It was considered important to ascertain how this rush into the transformer could be prevented. It is obvious that it can be entirely destroyed by inserting a sufficient non-inductive resistance into the primary circuit, which is gradually removed in place of suddenly closing the switch.

A better method, however, was suggested by Mr. Reginald Brougham—that is, to wind on the transformer a small tertiary circuit. This tertiary circuit is made to take current from another small transformer, which is kept in permanent connection with the primary main, and by means of which the core of the transformer to be connected is magnetised in the proper manner

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and in the proper direction for producing instantly the necessary opposing electro-motive force in the primary circuit when the primary switch of that transformer is closed. When this is done, there is never any "bang," or sudden current-rush, on closing the primary switch of the transformer under test. As a matter of fact, it has been found that when a bank of transformers is being fed from one primary circuit, and feeding their secondary currents into one secondary circuit, then, provided that one of the transformers of the lot is kept connected both to the primary and secondary sides, it is amply sufficient to proceed in the following manner in connecting in other transformers, in order to obtain perfect immunity from any harmful rush of current:—The transformer to be connected to the circuit is connected first on the secondary side, and although this process does produce a rush of secondary current back into the transformer, yet, nevertheless, the rises of pressure that take place never seem to be very serious. The transformer core being thus magnetised in proper relative step with the primary pressure, the primary circuit can be closed, and under no conditions then will there be any sudden rush of current into the transformer.

Exp. 10.—Many experiments were tried to find out what was the minimum resistance necessary to prevent this rush in the various sizes of transformers. It is obvious that a switch which closes the primary circuit, first by introducing resistance, and then gradually removing it, will sufficiently prevent any obnoxious rush of current into the transformer, because the impressed electro-motive force is held back, as it were, by the additional resistance from producing excessive current until the magnetism of the core has had time to fall into proper step.

Exp. 11.—Many effects due to similar causes can be produced in open magnetic circuit transformers. In switching off an open-circuit transformer, such as the "hedgehog," when the secondary of the transformer is on open circuit, a very strong arcing is sometimes observed at the switch; and the shorter the magnetic circuit of the transformer, the worse does this effect become.

On one occasion I had brought to my notice an experimental transformer of an open-circuit type, the iron core of which was

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remarkably short in comparison with its width. This transformer had a remarkable property—that, after having been connected to the primary circuit, it could not be disconnected as long as the secondary circuit of the transformer was open, without making an immense arc at the switch.

The rough explanation of this fact appears to be as follows:—In the open magnetic circuit transformer, especially if the iron part of the magnetic circuit is very short, the average value of the rate of change of induction or of the back electro-motive force decreases much more rapidly as the mean valne of the current decreases than is the case in closed magnetic circuit transformers. Hence, when in the case of the first-named transformer the primary switch is opened, the immediate reduction of the primary current-strength removes the greater part of the back electro-motive force, or opposing counter electro-motive force of induction, and greatly increases, therefore, the resultant electro-motive force in that circuit. An arc, therefore, continues to follow the switch points as they open, for a considerable distance. In the case of the closed magnetic circuit transformer this is not the case. The initial reduction of the current-strength as the primary switch opens is not accompanied by so great a reduction in the mean rate of change of the induction, and, therefore, of the back electro-motive force. The resultant electro-motive force in the circuit does not, therefore, rise much, and the switch points are soon beyond the distance at which an arc can be maintained.

Such an arcing can be subdued in the case of a very short iron magnetic circuit transformer by closing the secondary circuit. The mutual induction of the circuit then assists in creating a greater back electro-motive force in the primary circuit, and so keeps the resultant electro-motive force small as the switch points open.

There are several effects of this kind observable in the process of connecting or disconnecting large condensers to or from high-pressure alternating circuits. There is, in fact, always a risk of breaking down large condensers at the moment of putting them on or off alternating-current circuits, and this appears to be connected with a peculiar behaviour of an alternating-current arc

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between metallic surfaces, in virtue of which it acts like a resistance of large self-induction.

Conclusion.

There are very many derivative matters which, if space permitted, might be discussed as consequences of facts observed in the course of the above-described researches. It is evident, for instance, that the last word has by no means been said on the subject of design of transformers. I have every hope that before long transformer manufacturers may be able to produce transformers of 25 H.P. and upwards having close upon 90 per cent. efficiency at one-tenth of full load. When that is done, it will be interesting to repeat some of those calculations which have already been made as to the relative cost of delivering a certain number of thousand units on an average load diagram, say for distances of 1,000, 2,000, 3,000 yards, &c., by low-pressure feeders and by high-pressure plus transformer feeders. Calculations of this kind can be made with some degree of confidence now that it seems clear that the iron losses in transformers are constant at all loads, and that there is a definite relation between this loss and the open-circuit, or magnetising, current. We can hardly regard the great controversy of open *versus* closed magnetic circuits as entirely and definitely settled, but there is no reason to believe that the majority of those who have designed transformers with closed magnetic circuits have been supremely unwise in their faith or in their works.

In the matter of transformer testing, the experience gained in obtaining the facts brought before you has established in the writer's mind at least the feeling that a properly constructed dynamometer wattmeter is perhaps the best workshop instrument to use for this purpose, but that its personal character must be strictly examined before trusting too implicitly to the results obtained by it.

In conclusion, it will only be right to make mention of those who have assisted me in the somewhat arduous work of taking the observations here recorded. The principal share of this has been borne by my assistant, Mr. Shields, who has

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worked with great perseverance and care for many months taking the actual observations, and this in many cases requi some painstaking work. He was assisted at intervals by Mes J. and D. Morris, students in University College, Lond especially in those observations in which the careful simultane reading of several instruments was required. The observati on the sudden rush of current into transformers were made for by Mr. Wordingham.

The facts and measurements thus obtained have in all ca been sifted and repeated with the greatest possible care, with desire to arrive at conclusions which should be worthy of c fidence, and useful to those engaged in transformer manufact and design.

The PRESIDENT: In consequence of the lateness of the ho the discussion on this important paper will be postponed u the 1st of December.

A ballot for new members took place, at which the follow candidates were elected:—

Associates.

Epstein, L. | Guy, George Heli.
Moir, William.

Student.

Warden-Stevens, Fredric Joseph.

The meeting then adjourned.

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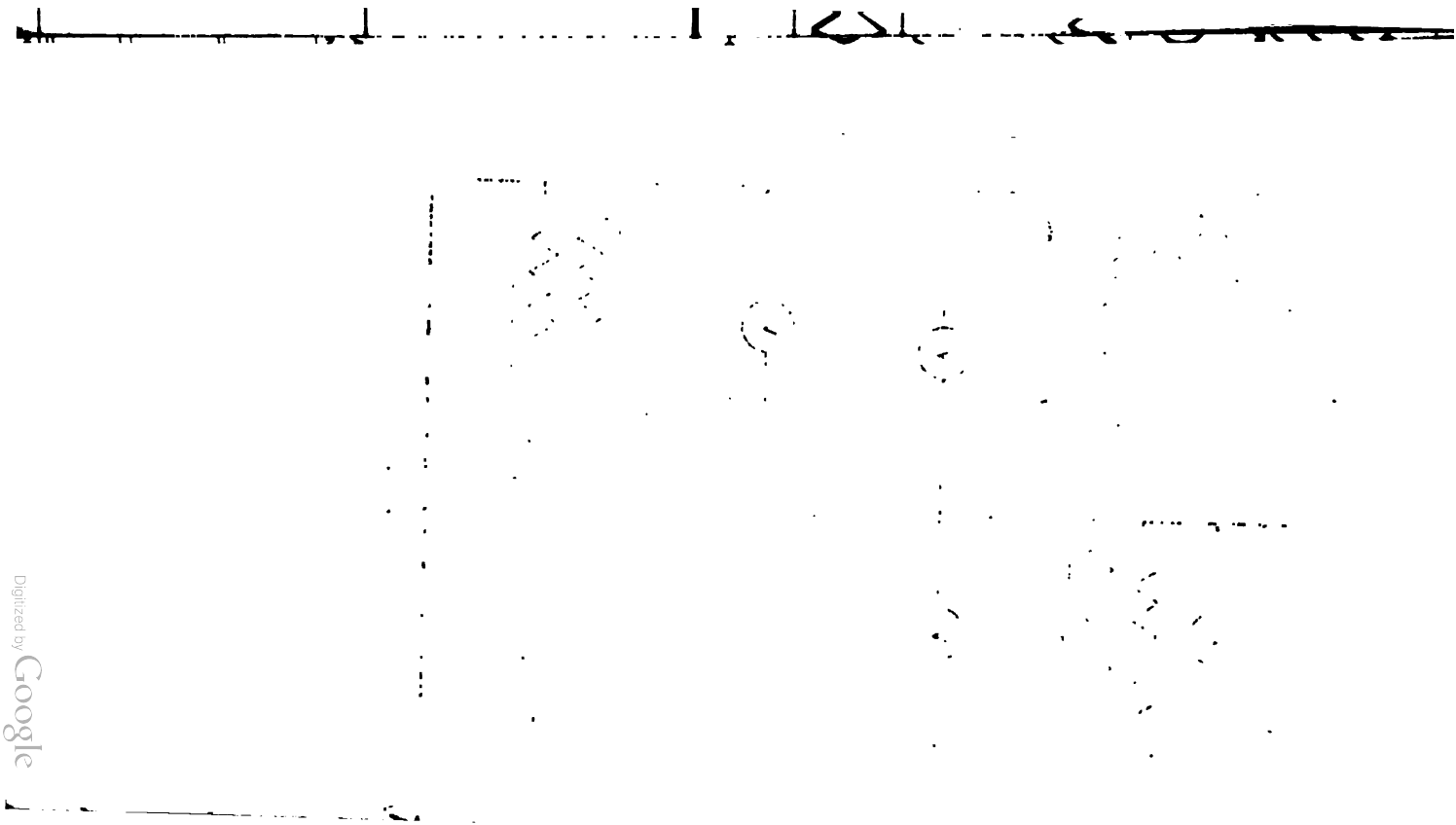

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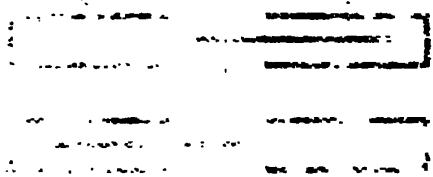

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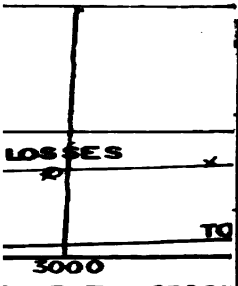
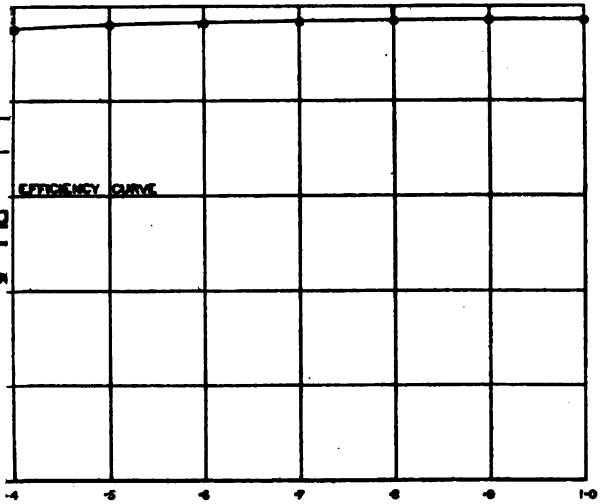
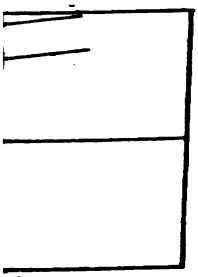


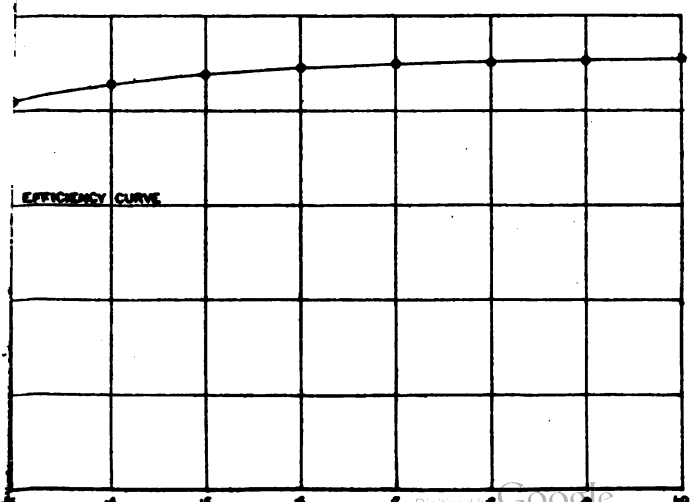
WESTINGHOUSE TRANSFORMER.



OF FULL SECONDARY LOAD. FIG 18.



185 PATTERN) FERRANTI TRANSFORMER REWOUND.



ACTIONS OF FULL SECONDARY LOAD. FIG 12.

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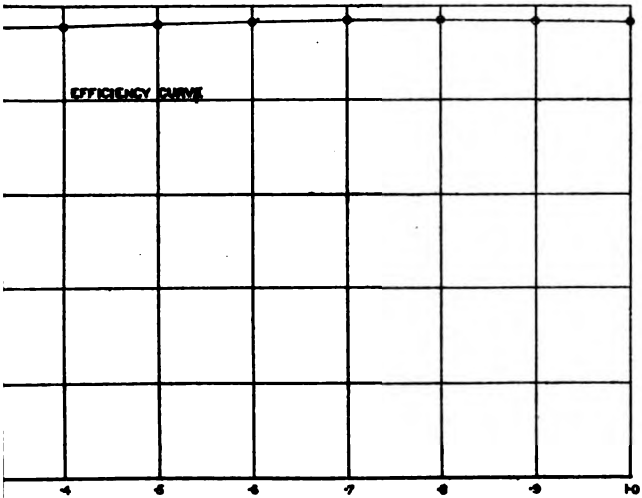
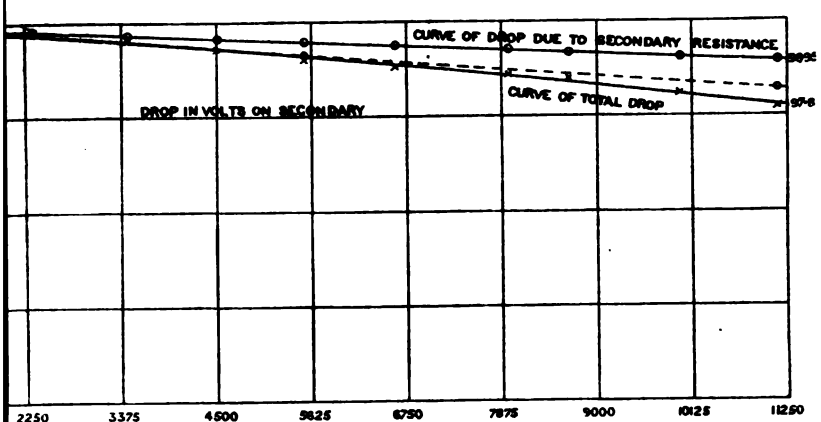
: PATTERN) FERRANTI TRANSFORMER



FULL SECONDARY LOAD FIG. 27.

15 HP (1892 PATTERN) FERRANTI TRANSFORMER REWOUND



OUTPUT IN SECONDARY WATTS FIG. 25.

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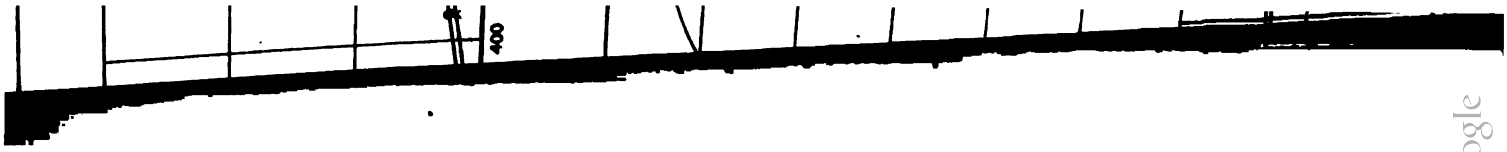

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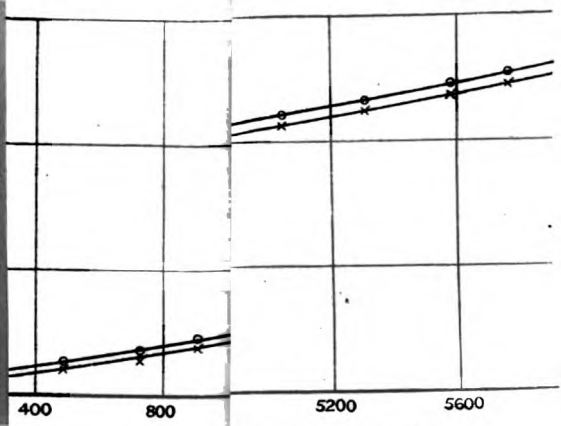
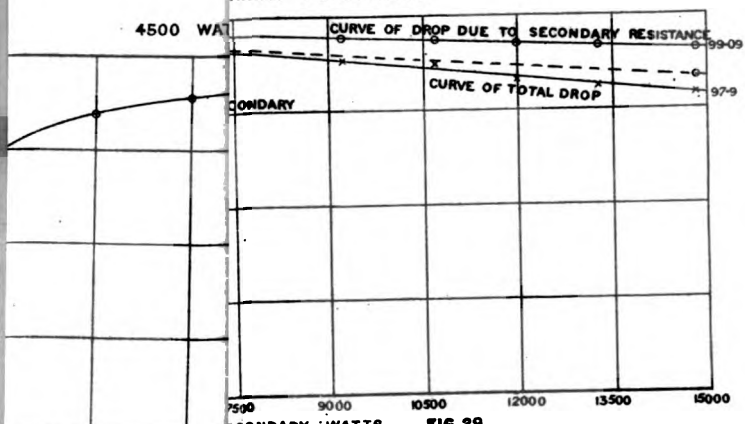


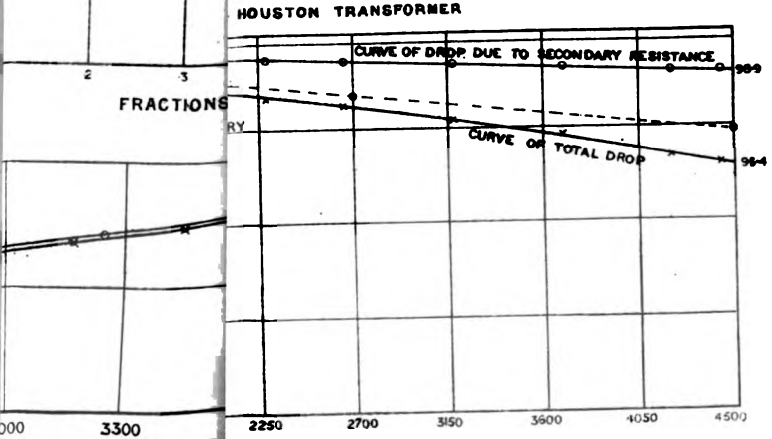
RANTI TRANSFORMER

4500 WAT



ECONDARY WATTS. FIG 29.

HOUSTON TRANSFORMER



CONDARY WATTS. FIG.37.

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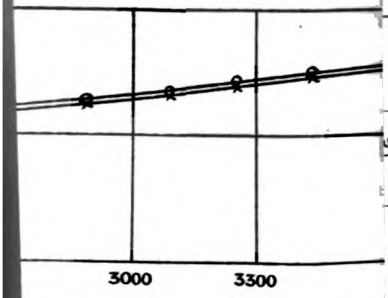

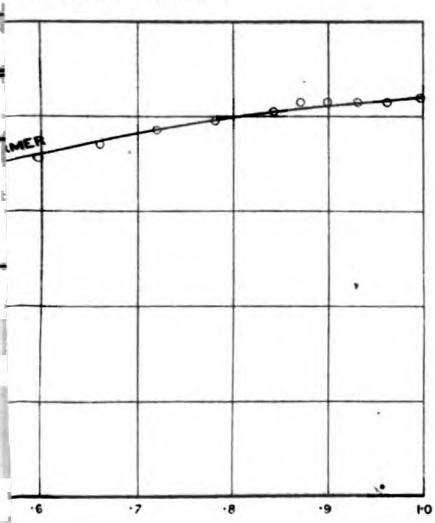

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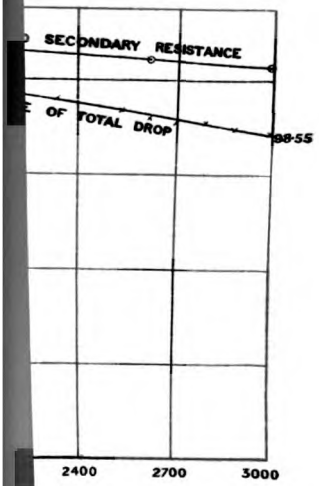

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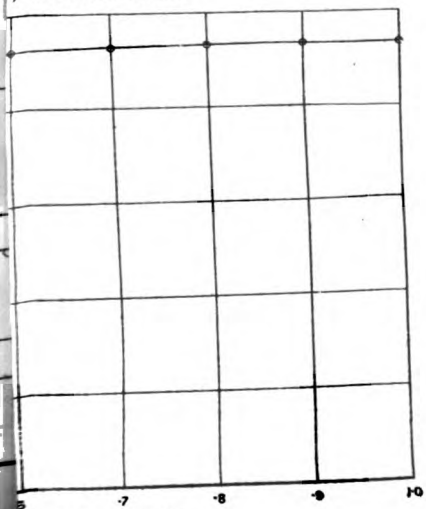

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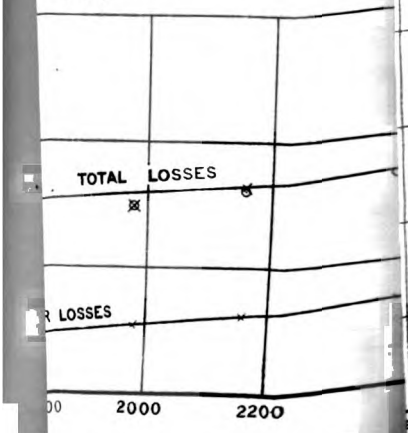

IG 42.

LOAD. FIG 43.

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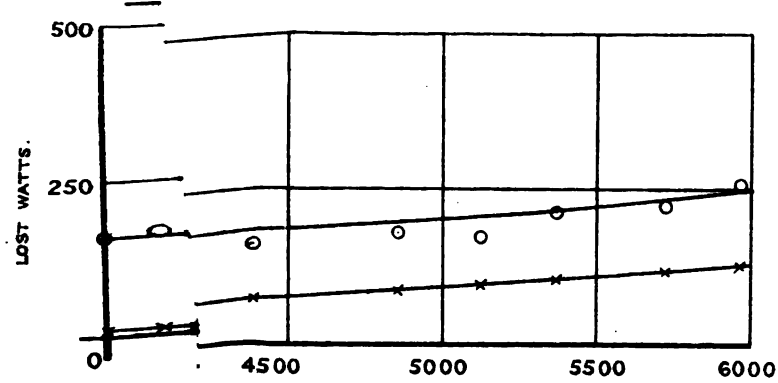


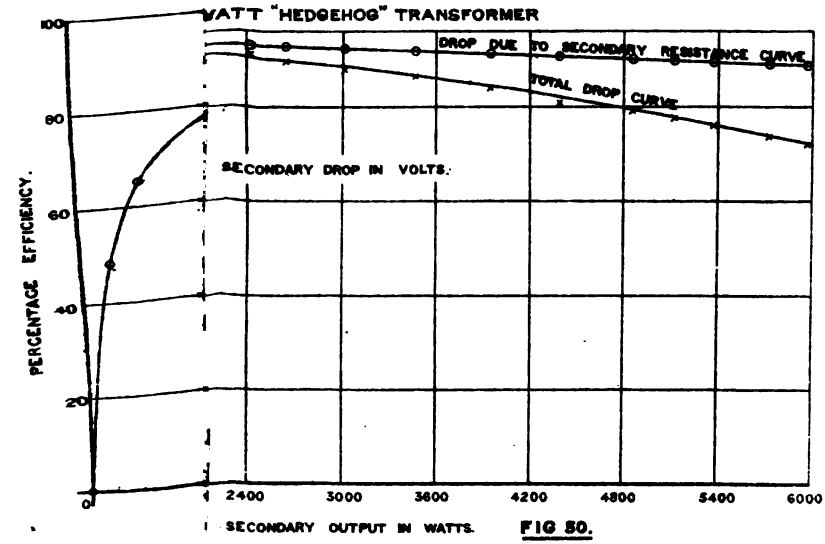
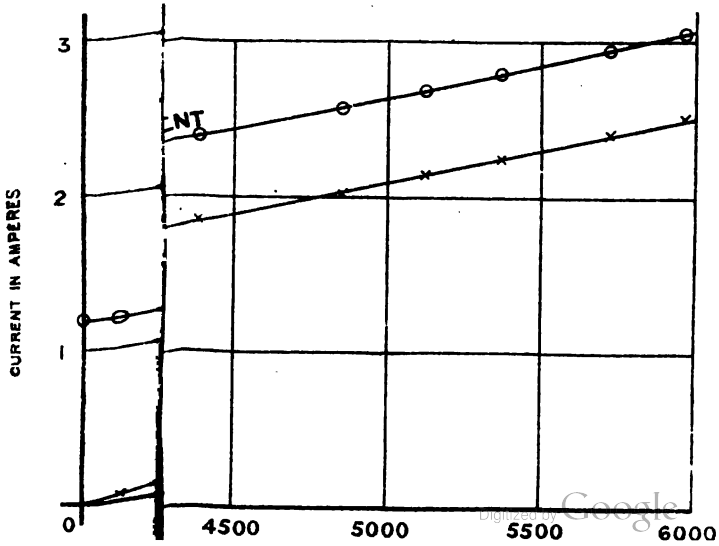


FIG 50.



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LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals from JUNE to DECEMBER, 1892.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

G. RICHARD—Details of Dynamo Construction (S. I.).—*Lum. El.*, vols. 44-46.

E. SCHULTZ—Comparison between Two-Pole and Multipole Machines.—*E. T. Z.* vol. 13, No. 34. p, 455; *Lum. El.*, vol. 45, No. 37, p. 529.

GUIDO GRASSI—Reluctance in the Stray Field.—*Lum. El.*, vol. 45, No. 32, p. 295.

O. PATIN—Dynamos and Transformer (I.).—*Lum. El.*, vol. 46, No. 47, p. 373.

H. BEHN-ESCHENBURG—The Drop of Volts in Alternators and Transformers.—*E. T. Z.*, vol. 13, No. 49, p. 651.

C. IMHOF—The Dimensions of Transformers.—*E. T. Z.*, vol. 13, No. 34, p. 456; *Lum. El.*, vol. 45, No. 34, p. 388, No. 39, p. 625.

F. LUCAS—Transformer (Direct to Alternating).—*Lum. El.*, vol. 46, No. 45, p. 274.

R. PICOU—Efficiency of Transformers and Transformer Systems.—*Bull. Soc. Int.*, vol. 9, No. 92, p. 389.

— DERI—Motor.—*Lum. El.*, vol. 46, No. 48, p. 428.

G. PELLISSIER—Statistics of European Central Stations (Output, Cost, &c.).—*Lum. El.*, vol, 45, No. 37, p. 509.

Installation Rules of Chambre Syndicale des Industries Electriques.—*Lum. El.*, vol. 45, No. 30, p. 172.

F. UPPENBORN—The Cologne Central Station (I.).—*E. T. Z.*, vol. 13, No. 28, p. 351.

F. GERALDY—Installations at Boulogne.—*Lum. El.*, vol. 46, No. 42, p. 101.

— GRAWINKEL—The Dangers of Bare Conductor Distribution.—*E. T. Z.*, vol. 13, No. 47, p. 634.

R. HIECKE—Testing for Earth on a Three or more Wire Network.—*E. T. Z.*, vol. 13, No. 40, p. 535.

ANON.—The Tivoli (Rome) Water-Power Installation.—*E. T. Z.*, vol. 13, No. 37, p. 500

J. P. ANNEY—The Use of Water Power.—*Lum. El.*, vol. 46, No. 42, p. 120.

F. UPPENBORN—The Lauffen-Frankfort Transmission (I.).—*E. T. Z.*, vol. 13, No. 29, p. 379.

F. GERALDY—Electrically Driven Tools—New Workshops of the Cie. de Nord, Paris (I.).—*Lum. El.*, vol. 45, No. 39, p. 601.

E. HARTMANN—Electric Power in Machine Shops.—*E. T. Z.*, vol. 13, No. 49, p. 657.

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G. RICHARD—Recent Electric Tram and Railway Work (S. I.).—*Lum. El.*, vols. 45, 46. (For complete list of matter of previous articles up to date see vol. 45, No. 29, p. 107.)

J. KRÄMER—Calculation of Maximum Speed for Electric Railways.—*E. T. Z.*, vol. 13, No. 43, p. 581.

ANON.—Electric Installations on the Railway across the Andes (I.).—*Lum. El.*, vol. 45, No. 32, p. 256.

SHEPARDSON and BURCH—Tests on Tramway Motors.—*Lum. El.*, vol. 46, No. 47, p. 383.

G. RICHARD—Recent Arc Lamps (S. I.).—*Lum. El.*, vols. 45 and 46.

C. FELDMANN—Lead Fuses.—*E. T. Z.*, vol. 13, No. 32, p. 423.

C. RAAB—Meter (Direct).—*E. T. Z.*, vol. 13, No. 49, p. 657.

G. RICHARD—Electric Welding (various Systems) (I.).—*Lum. El.*, vol. 46, No. 42, p. 114.

BURTON and ANGELL—Cooking Apparatus (Alternating) (I.).—*Lum. El.*, vol. 45, No. 32, p. 285.

C. FELDMANN—Light given and Power used in Glow Lamps.—*E. T. Z.*, vol. 13, No. 50, p. 667.

ACCUMULATORS.

H. MÜLLER—The Use of Accumulators for Central Stations.—*E. T. Z.*, vol. 13, No. 29, p 378.

J. F. WEYDE—Specific Gravity of "Formed" Peroxide.—*E. T. Z.*, vol. 13, No. 49, p. 657.

F. STREINTZ—Theory of the Accumulator.—*W. A.*, vol. 46, No. 7, p. 449.

M. CANTOR—Chemistry of the Accumulator.—*W. A.*, vol. 47, No. 11, p. 424.

MAGNETISM.

A. ABT—Permanent Magnetism of Steel and Nickel.—*Beibl.*, vol. 16, No. 10, p. 687.

P. CURIE—Magnetic Properties of Bodies at different Temperatures.—*C. R.*, vol. 115, No. 20, p. 805.

A. BANTI—The Magnetisation of Nickel under various Conditions of Mechanical Strain.—*Beibl.*, vol. 16, No. 8, p. 557; *Rend. Lincei*, 2, 287, 1890.

MAX CORSEPIUS—The Magnetic Circuit.—*E. T. Z.*, vol. 13, No. 31, p. 414.

C. P. STEINMETZ—The Magnetic Circuit.—*E. T. Z.*, vol. 13, No. 28, p. 365.

G. ADLER—The Work of Magnetisation.—*W. A.*, vol. 46, No. 7, p. 503.

E. VAN AUBEL—Influence of Magnetism on Length of a Bar of Bismuth.—*Jour. de Phys.*, October, 1892, p. 424.

MAX CORSEPIUS—Hysteresis Loss in the Armature.—*E. T. Z.*, vol. 13, No. 33, p. 443; *Lum. El.*, vol. 45, No. 38, p. 577.

— SCHMOLLER—Industrial Testing of Iron for Hysteresis.—*E. T. Z.*, vol. 13, No. 30, p. 407.

C. P. STEINMETZ—Magnetic Hysteresis.—*E. T. Z.*, vol. 13, No. 39, p. 519.

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C. DECHARME—Movements of a Magnet Floating on Mercury carrying a Current. —*C. R.*, vol. 115, No. 18, p. 651; *Lum. El.*, vol. 46, No. 5, p. 501 (I.).

W. WIEN—The Direction of Lines of Force in a Magnetic Field.—*W. A.*, vol. 47, No. 10, p. 327.

ANON,—Descartes on the Magnetic Circuit: Lines of Force, A.D. 1656.—*Lum. El.*, vol. 45, No. 27, p. 45.

TELEGRAPH AND TELEPHONE.

E. BUELS—Bronze Telegraph Wires.—*Jour. Tel.*, vol. 16, No. 9, p. 229.

E. DISSIMOZ—The Adjustment of the Break on the "Hughes."—*Ann. Tel.*, vol. 18, p. 217.

H. BEHN-ESCHENBURG—Tests on a G.P. Cable.—*E. T. Z.*, vol. 13, No. 30, p. 399.

GUIACOMINI and SACCO—Printing Telegraph (I.).—*Lum. El.*, vol. 45, No. 37, p. 539.

G PELLISSIER—The French Telegraph Cable Industry (S. I.).—*Lum. El.*, vol. 46, No. 45, p. 251.

— MERCADIER—The Bi-Telephone (I.).—*Lum. El.*, vol. 46, No. 49, p. 451.

ANON.—Disturbing Effect of Berlin Network on Telephones.—*E. T. Z.*, vol. 13, No. 47, p. 629.

F. TROUTON—Theory of Permanent Magnet Core in Telephone.—*Phil. Mag.*, vol. 34, No. 208, p. 276.

J. GILTAY—Remarks on above.—*Phil. Mag.*, vol. 34, No. 210, p. 460.

— BERTON—Multiple Board (I.).—*Lum. El.*, vol. 45, No. 31, p. 233.

ANON.—German Telephone Development from 1887 to 1890.—*Ann. Tel.*, vol. 18, No. 3, p. 233.

INSTRUMENTS AND MEASUREMENTS.

— DUCRETET—Laboratory Electrical Furnace (I.).—*Lum. El.*, vol. 46, No. 50, p. 512.

G. RICHARD—Aluminium (*continued*—S. I.).—*Lum. El.*, vol. 46, No. 48, p. 301.

V. DVOŘÁK—Automatic Contact-Breakers.—*Beibl.*, vol. 16, No. 7, p. 440.

EDSER and STANSFIELD—Instrument for Measuring Magnetic Fields (I.).—*Phil. Mag.*, vol. 34, No. 207, p. 186.

— SIEMENS—Apparatus for Measuring Magnetic Field.—*Lum. El.*, vol. 46, No. 45, p. 278.

— DE PLACE—The Schiseophone (Testing Masses of Metal for Flaws, &c., with the Microphone).—*C. R.*, vol. 115, No. 17, p. 582.

— VERNEUIL—Automatic Mercury Pump.—*Lum. El.*, vol. 45, No. 37, p. 528.

C. BARUS—Measurement of High Temperatures—*Phil. Mag.*, vol. 34, No. 206, p. 1.

E. GLENGAIRE—Quick-Reading Thermopile Thermometer.—*Lum. El.*, vol. 46, No. 47, p. 372.

GRIFFITHS and CLARKE—Low Temperatures by Platinum Thermometers.—*Phil. Mag.*, vol. 34, No. 211, p. 515.

HOLBORN and WIEN—Measurement of High Temperatures.—*W. A.*, vol. 47, No. 9, p. 107.

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G. Adler—Determination of Permeability by the (Chemical) Balance.—*Wien Ber.*, 100, p. 897, 1891; *Beibl.*, vol. 16, No. 8, p. 553.

— Bergmann—Further Investigations with the Induction Balance.—*Beibl.*, vol. 16, No. 7, p. 441.

A. Winkelmann—Zero Methods with the Telephone.—*W. A.*, vol. 46, No. 8, p. 666.

— Cardani—Measurement of S.I.C.—*Lum. El.*, vol. 46, No. 48, p. 342.

O. Troje—Determination of Inductance by the Electro-Dynamometer.—*W. A.*, vol. 47, No. 11, p. 501.

C. E. Guillaume—Temperature Coefficient of Mercury.—*C. R.*, vol. 115, No. 11, p. 414.

J. Dewar—Resistances of Metals, &c., at the Boiling Point of Oxygen.—*Phil. Mag.*, vol 34, No. 209, p. 326.

A. Hess—Insulators (Insulation Resistance does *not* vary with E.M.F.).—*Lum. El.*, vol. 46, No. 48, p. 401, *et. seq.*

E. Branly—Resistance of Non-Conductors.—*Jour. de Phys.*, Nov. 1892, p. 459.

H. Abraham—New Determination of "v" ($2{\cdot}992 \times 10^{10}$).—*Jour. de Phys.*, Sept., 1892, p. 361.

— Miculescu—Table of various Determinations of J (Mech. Equiv. of Heat).—*Lum. El.*, vol. 45, No. 29, p. 145.

E. Rosa—S.I.C. of Electrolytes.—*Phil. Mag.*, vol. 34, No 209, p 344.

W. Stschegtiaeff—On the same.—*Phil. Mag.*, vol. 34, No. 209, p. 388.

STATIC AND ATMOSPHERIC ELECTRICITY.

H. Abraham—The Output of an Influence Machine.—*Jour. de Phys.*, October, 1892, p. 409.

G. Adler—Capacity of Condensers.—*W. A.*, vol. 46, No. 7, p. 500.

J. Lefèvre—Attraction of Two Plates separated by a Dielectric.—*Jour. de Phys.*, June, 1892, p. 243.

R. Arno—Rotating Electric Field and a Motor Working by Electrostatic Hysteresis (I.).—*Lum. El.*, vol. 46, No. 50, p. 537.

G. Vicentini—Luminous Effects—Conductors in Rarefied Air.—*Lum. El.*, vol. 45, No. 37, p. 539.

Elster and Geitel.—Atmospheric Electricity.—*Beibl.*, vol. 16, No. 10, p. 697.

P. Lenard—Electricity in the Neighbourhood of Waterfalls.—*W. A.*, vol. 46, No. 8, p. 584.

G. Herholz—Earth Plates of Lightning Conductors.—*E. T. Z.*, vol. 13, No. 32, p. 431.

L. Palmieri—Atmospheric Electricity.—*Lum. El.*, vol. 46, No. 40, p. 22.

— Ricco—Sun Spots and Magnetic Disturbances.—*C. R.*, vol. 115, No. 17, p. 595.

J. Lisnar—The Determination of the Disturbing Force in Magnetic Storms.—*Sitzb. Wien. Acad.*, 101, ii., p. 87, 1892; *Beibl.*, vol 16, No. 10, p. 699.

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THEORY.

H. Behn-Eschenburg—Pressure in Ferranti Cables.—*E. T. Z.*, vol. 13, No. 45, p. 605.

D. Korda—Theory of a Condenser interposed in the Secondary of a Transformer.—*C. R.*, vol. 115, No. 7, p. 331, No. 11, p. 411.

A. Blondel and Others—Long Series of Mathematical Articles on Coupling and Synchronising Alternators.—*Lum. El.*, vols. 45 and 46.

P. Boucherot—The Effect of Moment of Inertia on the Synchronising of Alternators.—*Lum. El.*, vol. 46, No. 50, p. 512.

J. Heubach—Work and Lag in the Alternating-Current Arc.—*E. T. Z.*, vol. 13, No. 34, p. 460.

H. A. Rowland—Notes on the Theory of the Transformer.—*Phil. May.*, vol. 34, No. 206, p. 54.

E. Cohn—The Coexistence of S.I.C. and Conductivity.—*C. R.*, vol. 115, No. 14, p. 472, No. 20, p. 802.

E. Bouty—On the same.—*C. R.*, vol. 115, No. 16, p. 554, No 20, p. 804; *Jour. de Phys.*, Nov., 1892, p. 445.

Bedel and Crehore—Effects of Self-Induction and Distributed Static Capacity in a Conductor.—*Phil. Mag.*, vol. 34, No. 211, p. 528.

E. Branly—The Conductivity of a Gas between a Cold Metal Plate and an Incandescent Body.—*C. R.*, vol. 114, No. 26, p. 1531.

A. P. Chattock—An Electrolytic Theory of Dielectrics.—*Phil. Mag.*, vol. 34, No 211, p. 461.

H. Dubois—Mathematical Theory of Paramagnetism.—*W. A.*, vol. 46, No. 7, p. 485; *Phil. Mag.*, vol. 34, No. 208, p. 307.

F. Osmond—Relation between Resistance of Iron and Steel and their Composition and Temper.—*Lum. El.*, vol. 46, No. 41, p. 93.

V. Bjerknes—Resonance. The Absorbing Power of Metals for the Energy of Electric Waves.—*W. A.*, vol. 47, No. 9, p. 69.; *Lum. El.*, vol. 46, No. 45, p. 285.

R. Blondlot—The Rate of Propagation of Electro-magnetic Waves in Insulating Mediums, and on Maxwell's Relation.—*C. R.*, vol. 115, No. 4, p. 225.

R. Colson—Demonstration by the Telephone of the Existence of Interference of Waves in a Closed Circuit.—*C. R.*, vol. 115, No. 20, p. 800.

B. Galitzine—Radiant Energy.—*W. A.*, vol. 47, No. 11, p. 479.

D. A. Goldhammer—Studies in the Electro-magnetic Theory of Light.—*W. A*, vol. 47, No. 10, p. 265; *Jour. de Phys.*, Aug., 1892, p. 345.

P. Janet—Electrical Oscillations.—*C. R.*, vol. 115, No. 21, p. 875.

A. Perot—Measurement of S.I.C. by Electro-magnetic Oscillations.—*C. R.*, vol. 114, No. 26, p. 1528; vol. 115, No. 1, p. 38, No. 3, p. 165.

C. Raveau—Maxwell's Electro-magnetic Theory of Light (S.).—*Lum. El.*, vol. 46, No. 40, p. 7, and No. 42.

W. Robb—Oscillations in Charging a Condenser.—*Phil. Mag.*, vol. 34, No. 210, p. 389.

Sarasin and De la Rive—The Production of the Hertz Oscillation Spark in a Liquid Dielectric instead of Air.—*C. R.*, vol. 115, No. 12, p. 439.

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A. Toepler—Production and Observation of Oscillations.—*W. A.*, vol. 46, Nos. 7 and 8, pp. 464, 624.

L. Zehnder—Making the Hertz Spark Effect apparent to an Audience.—*W. A.*, vol. 47, No. 9, p. 77.

VARIOUS.

A. d'Arsonval—Physiological Effects of a Sinusoidal Alternating Current.—*C. R.*, vol. 114, No. 26, p. 1534.

V. Frey—Resistance of Human Body.—*Phil. Mag.*, vol. 34, No. 207, p. 218.

— Korthals—Effect of Alternating Currents on the Human Body.—*E. T. Z.*, vol. 13, No. 32, p. 429; *Lum. El.*, vol. 45, No. 36, p. 486.

Dr. Larat—Therapeutic Effects of Alternating Current.—*Bull. Soc. Int.*, July, 1892, p. 314.

L. Brasse—Gutta-Percha.—*Lum. El.*, vol. 46, No. 41, p. 51, *et seq.*

— Cardani—Heating of Bare Wires.—*Lum. El.*, vol. 46, No. 45, p. 283.

P. Drude—Magneto-optical Phenomena.—*W. A.*, vol. 46, No. 7, p. 353.

H. de Graffigné—Electric Chronography (S. I.).—*Lum. El.*, vols. 45 and 46.

R. Lohnstein—Passage of Weak Currents through Electrolytes.—*W. A.*, vol. 47, No. 10, p. 299; *Lum. El.* vol. 46, Nos. 45, 46, p. 288.

A. Oberbeck—Behaviour of Allotropic Silver with Current.—*W. A.*, vol. 47, No. 11, p. 353; *Lum. El.*, vol. 45, No. 33, p. 343.

Anon.—Cementing Glass to Metal.—*Beibl.*, vol. 16, No. 7, p. 458.

G. Richard—Applications of Electricity (S. I.).—*Lum. El.*, vols. 45, 46, &c.

W. v. Turin—The Influence of Electrical and Magnetic Forces on Strength of Solutions.—*Beibl.*, vol. 16, No. 8, p. 562.

J. Violle—Radiation of Incandescent Bodies.—*Jour. de Phys.*, July, 1892, p. 298.

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JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXI. 1892. No. 102.

The Two Hundred and Forty-fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 1st, 1892—Prof. W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on the 24th November were read and approved.

The name of one new candidate for election into the Institution was announced and ordered to be suspended.

Donations to the Library were announced as having been received since the last meeting from Messrs. Macmillan; Mr. W. Ellis, Member; Mr. W. J. Hancock, member; and also from Mr. Preece, an oil portrait of Lord Kelvin; to whom the thanks of the meeting were duly accorded.

Mr. Fairfax and Mr. Mitchell were appointed scrutineers of the ballot.

The PRESIDENT: We will now discuss the paper by Professor Fleming, read at the last meeting, on "Experimental Researches "on Alternate-Current Transformers." Perhaps Mr. Swinburne would like to open the discussion.

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Mr. Swinburne.

Mr. SWINBURNE: Before discussing the main subject of Dr. Fleming's paper, I will make a few remarks about the electrostatic voltmeters supplied by my firm. Dr. Fleming's paper would, quite accidentally, give the impression that we sent him instruments which would not work properly. We sent him four instruments, three of which were to be used as voltmeters to test the three-voltmeter method, and one as a direct-reading electrostatic wattmeter. They were ordered for half the pressures for which they have been actually employed: hence the sparking. We had previously considered the question of using mica in our central station electrostatic voltmeters, but, though an advantage in the case of alternate currents, it prevents the instruments from being calibrated direct from standard cells, as charges are apt to accumulate when direct pressures are used. Dr. Fleming also finds fault with the damping arrangement. I adopted the electro-magnet for damping because oil is messy and troublesome. The electro-magnet arrangement works well enough, but Dr. Fleming does not appear to have had a suitable direct current at command. The electro-magnet should have been connected across the terminals of one or two storage cells. This instrument was really designed as a direct-reading wattmeter. There is a strong prejudice against the electro-magnetic wattmeter, and many people prefer the three-voltmeter method. This form of wattmeter is really three voltmeters in one instrument, and thus eliminates errors due to the use of three instruments. For many experimental purposes it is extremely convenient. It is very sensitive, and is a direct-reading instrument.

Coming now to the more important parts of Dr. Fleming's paper: I have for five years strongly advocated the use of the dynamometer wattmeter, but so far the idea of using a wattmeter for alternate-current work has been treated with ridicule. People always said there was an error due to the self-induction of the fine-wire circuit. It was in vain to say that the fine-wire circuit could be made with such a low time constant that the error is unappreciable. People insisted upon making calculations with R and L, and obstinately refused to put approximately true values of R and L into their equations to see how much the probable

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error really was. There seems to be a strong prejudice against using a simple instrument that reads direct. Though Professor Ayrton and Dr. Sumpner were the first to think of the three-voltmeter method, I was the first to publish it, and in doing so I pointed out that it was only a kind of scientific curiosity, and that it was not of any practical value in comparison with the wattmeter method. In spite of this, its complication, and the obvious difficulty in getting good readings, it seemed to become popular at once. Dr. Fleming's measurements, taken with the greatest care, now show that the non-inductive wattmeter is much the best instrument to use. Mr. Swinburne.

Of course it may be said that I have weakened my case by sending a wattmeter which did not read correctly. Dr. Fleming puts this inaccuracy down to capacity in the fine-wire circuit. I tried in vain to persuade him to calculate how much error the capacity of the resistance boxes could give. I worked it out myself, however, and found there was no error due to capacity. Even now Dr. Fleming seems to think the error was due to capacity; he locates the capacity in the fine-wire coil itself. Capacity there would make the instrument read too high, however, so that view is not tenable. The real cause is very simple. There are slight Foucault currents in the cases, and especially in the framework, of the instruments. With the primary current lagging about a quarter of a period behind the pressure, the Foucault currents are almost half a period out of step with the currents in the fine-wire circuit. The result is that they reduce the reading. This error, which is a small percentage of the apparent power of the circuit under examination, is enough to vitiate readings taken on a "hedgehog" transformer. It does not come in in cases where the current and pressure are nearly in step. The first wattmeters we made, had no metal case and insulated standards, and they were carefully checked to make sure that they were right. The metal case was a later addition, as glass was not suitable for workshop use, and I did not think of the Foucault current being more important in a wattmeter than in other alternate-current instruments. As a matter of fact, very few of our wattmeters have had metal cases.

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Mr. Swinburne.

The general results of Dr. Fleming's paper are three, namely: that a wattmeter is the best instrument for measuring power; that the efficiency of a transformer can be found by taking the no-load loss and adding the loss by $C^2 R'$ to get the full-load loss; that the "hedgehog" transformer sent him is not as good as I have said it is. The first two are very welcome to me, as I have advocated them both for the last five years. The last is not so pleasing. As to the "hedgehog" transformer sent, I must say at once that I believe Dr. Fleming's results to be quite correct. They have been obtained with great care and considerable labour, and he has been quite free from any partiality. I can only say that he shows conclusively that the transformer sent him is not nearly so good as it ought to be. What is wrong with it is another matter. Dr. Fleming points out fairly enough that there is not really room for a loss of 100 odd watts in a small iron core, and he has come to the conclusion that the loss is due to Foucault currents in the copper. It has also puzzled me a great deal. It cannot be loss in the copper. When Lord Kelvin read his paper on the subject at the Leeds meeting of the British Association, I made a table of the losses of power in copper in my own transformers, and I do not think there is any chance of an error there. This particular transformer has a secondary of cable, and the loss is just as great if the cable is removed altogether. Besides, loss by Foucault currents is enormously greater at full load. It is only within the last day or two that I have found what I believe to be the cause of the trouble. In winding the primary and secondary wire considerable tension is employed. This gives rise to a pressure of no less than 30 tons on the iron core. This may force the fine wires into such close contact that they are no longer insulated from one another, so that the core acts more or less like a solid cylinder of iron. Professor Ayrton and Dr. Sumpner had made tests of another transformer made at the same time, and they got the same results as Dr. Fleming. As the results seemed so different from those claimed by me, they preferred to consider their readings incorrect rather than run the chance of damaging my firm's business by publishing tests without being absolutely certain that they are right. I must thank them for their kindness in this matter.

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Mr. Swinb

Dr. Fleming's paper, unfortunately, does not really settle the question as to the relative superiority of closed and open circuit transformers, as the inefficiency of the open-circuit transformer sent him is evidently due to some error in construction, and is not inherent in the type of transformers.

I would protest against the too general use of the "power-"factor." I believe I was the first to suggest this, but it was only as a very rough guide for central station engineers—a guide that might be used with advantage, until people came to realise that a wattmeter could be used. As a means of taking the efficiency of the transformer it is too rough. Many transformers have a joint in the iron circuit, and this would make a very large error. In addition to this, the power-factor depends on the induction, and this on the frequency.

Professor Fleming refers to one very important point which is generally overlooked in discussions before societies, and that is the price. The price of a transformer is just as important as the efficiency. As Dr. Fleming points out, anyone can make efficient transformers if he is not limited as to price. Convenience and ease of handling are also of the greatest importance.

Dr. Fleming's method of finding drop by waste field seems to be wrong. I have already published a method of finding the drop from the drawings of the transformer, which is, I believe, correct, and which agrees with the results found in practice. The back electro-motive force producing the drop is out of step with the pressure, so that it does not show much on non-inductive circuits. If a transformer with 1 per cent. inductive drop is run on a choking coil, it will be found to have a drop of about 14 per cent.; and on a condenser its ratio of transformer will apparently be altered so that it gives 14 per cent. too high pressure. This may give serious trouble when alternate motors come into use, and difficulties are no doubt felt even now with the multiple-current motors employed abroad.

I have never observed that transformers broke down on switching on, and Dr. Fleming's explanation will not hold water. If you switch off a closed-circuit transformer you may certainly leave it strongly magnetised, and on switching on its residual

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Mr. Swinburne.

gnetism might be in the wrong direction. This would not allow an enormous primary current to rush through, but would merely make the iron less permeable, so that the exciting current would at first be greater. This would produce no high electromotive force, though. When the transformer is in circuit its electro-motive force must at every instant be the same as that of the mains, and this determines the rate of change of induction in the core. It is much more likely that the transformer should break down on switching off. It may be left with the iron magnetised, as the hysteresis allows you to open the switch without a spark. The most minute disturbance will make the iron lose its magnetism suddenly, and if the primary and secondary circuits are open, a well-laminated core may produce many thousands of volts for an instant and break down the insulation. This cannot occur in an open-circuit transformer, as the iron demagnetises and keeps the arc going at the switch until it has died away.

Condensers are apt to break down on opening the circuit. This appears to be due to quite a different cause. On breaking, an arc is formed, and this seems to behave like an inductive circuit—*i.e.*, the current lags behind the pressure. At a certain length of arc electrical resonance is produced, and this breaks down the condenser. This is suggested as an explanation. The effect gave us a great deal of trouble for many months, as a condenser broken down on turning off did not give any outward sign of injury until switched on again; and as the pressure cannot then be above the normal, we thought the condensers were bad, and spent many months and a great deal of money over them.

The effect of the open and closed circuit transformers on the working at switches is of course due to the one being able to keep its induction by hysteresis, and the other not.

Finally, I must congratulate Dr. Fleming on bringing us such a valuable and useful paper, and I think the thanks of the Society are due to him for the immense amount of labour and trouble involved in its preparation.

Professor Forbes.

Prof. GEORGE FORBES: I have very few remarks to make on the subject. This is, of course, a paper of the very utmost value

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from many points of view. The results which appear in the paper are so voluminous and so complete, that really it leave it very little more to be said on the subject. A great many people who had not made tests on transformers before had a sort of belief that all transformers had about the same efficiency. Those, however, who had made actual tests were perfectly well aware of the enormous difference that there was in the efficiency of transformers. I commenced a series of tests of a Westinghouse transformer at Pittsburg in the year 1888, and I mentioned some of the results before this Institution in that year, and I was extremely surprised at the remarkably high efficiency of those transformers even at that date with a small output. Well, the calorimetric methods which I introduced there of testing them have been extended, and an enormous number of laboratory experiments made on different types of transformers with the object of improving them in their efficiency at different loads, and I have had these results transmitted to me at various times, and I know that they were very accurate, and I was aware of the very high efficiency of these transformers. Now we have from Dr. Fleming's experiments a confirmation that these transformers, and also the Mordey and some others, are of extremely high efficiency indeed; while others which have been very largely used in this country have been proved by these experiments, as was known to many of us before, to be extremely inefficient indeed. One does not feel inclined to criticise a paper of this value in any way whatever adversely, and in the results I am sure there is but little that we can find to criticise; and I am sure that with regard to anything that Mr. Swinburne has said which would seem to imply a want of accuracy, Dr. Fleming will be able to reply to it very fully himself when the time comes. There is one little remark about the paper which I would like to make. I would suggest that in other papers which may be read before this Institution, attention should be paid to this fact. Of course it is of the utmost importance when a paper is read before us and we have such a large number of diagrams, that the reference numbers to the different figures should be bold and distinct, so that we can see them thoroughly

Mr. Swinb

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Professor Forbes.

well. There is another point in the diagrams which is a matter of considerable importance, and that is, that the diagrams themselves should be visible. Now, when a paper has taken such an enormous amount of trouble and labour to perfect it, I think it is very much indeed to be regretted that these diagrams were not drawn on a sufficiently large scale that some of the audience at least may be able to see the curves. The point which everybody has been aiming at in the design of transformers in the past has been to make the efficiency at low loads as high as possible; and so long as the present system of using transformers with separate wires in each house was employed, that was certainly the most important thing, because a transformer during the greater part of the 24 hours is not working at its full load or anything like it, and for the greater part of the time it has no load at all, and if the losses under these conditions are great, of course the total efficiency of the system becomes very low indeed. Great attention has justly been paid to making transformers with these properties, but it may turn out—it seems probable that there is a consensus of opinion at present—that in the immediate future, when the houses which are taking electric light are much more closely grouped together than they have been in the past, that then we shall require secondary mains going in front of the houses,—that we shall have sub-stations with much larger transformers, capable of regulation either automatically or by hand. If that is the case, then the efficiency of the transformer at low load is not of so very paramount importance. I wish to draw your attention to another case where the efficiency at low loads is not of so very much importance, and that is in the utilisation of water power for transmission to distances; especially when we are using transformers of considerable size this becomes an important matter. The difference in design of a transformer to give you a very high efficiency at full load, or a good efficiency at nearly every load, and the difference in construction, consist in the relative proportions of copper and iron. If we wish to get these excellent results at low load, we must put in a larger proportion of copper; whereas if we are content to have a

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high efficiency at the very large loads, then we can put in a much smaller quantity of copper and a certain larger quantity of iron. That is, of course, a cause of enormous reduction in the cost of transforming. Now in the case of a transmission of power the cost of the transformer is very great indeed. Consider the cost of a transmission of power from Niagara Falls to Buffalo, where we are using units of 5,000 H.P. There, if we were to use alternating currents, we should be using a step-up transformer at Niagara Falls and a step-down transformer at Buffalo. The cost of each one of these transformers, according to the estimates put in up to the present time, is considerably in excess of the 5,000-H.P. dynamo which produces the current, and consequently the two transformers are nearly three times the cost of the dynamo which is generating the current. Now, of course, this is a very serious item in the cost of the H.P. per annum at which it can be delivered at Buffalo, and it requires very little consideration to see that the value of our power when a small amount of power is being used is much less; that is to say, at low loads we can do perfectly well with a far lower efficiency than we can at high loads. In the case of the utilisation of water power, we want the highest efficiency we can get, when all the power is being delivered at Buffalo, and we do not care much about the efficiency of transformers at low loads. Consequently we are able to produce transformers at an enormously reduced cost; I think it will come out something between one-half and one-third of the cost of transforming, if they were made in the way they have been made for lighting purposes—that is to say, to get as good an efficiency as we can over the whole range. I have just introduced these remarks because I want manufacturers to see the importance there is in certain cases in giving us the very highest efficiency they can at full load in as cheap a transformer as we can get, without paying attention so much to efficiency at low loads. At the same time, one of the great values of this paper is in showing how very high efficiencies can be obtained at low loads. The data which are given to us by Dr. Fleming in this most valuable paper are sufficient for us to calculate out with very great accuracy the

Professor Forbes.

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Professor Forbes. total efficiency of an alternating-current system in any electric light station, and until this paper was read by Dr. Fleming our materials for forming such an estimate were very small indeed; and I am sure everyone here will join with me in thanking Dr. Fleming most heartily for this paper, involving so many laborious experiments, which he has given us.

Mr. Kapp. Mr. Gisbert Kapp: Mr. President and gentlemen,—The very valuable paper of Dr. Fleming is nominally on transformers, but really on the measurement of transformers; and in this direction I believe the paper is quite a unique document. I do not think there has ever been such a complete series of measurements of all types of transformers undertaken with so much care, and—if I may be allowed to use a slightly vulgar expression—without any "cooking" whatever. This is a fact which detracts somewhat from the symmetry and beauty of the figures, but it gives you immense confidence in their accuracy. I see, for instance, in one or two cases Dr. Fleming states a power-factor of 100·1. Now that is quite impossible, and some less conscientious investigators would have struck off the ·1 and given 100. I think we may look at this paper as an absolutely reliable record of the present condition of transformers. Dr. Fleming has been at great trouble to investigate the accuracy of the three-voltmeter method, and prefers the wattmeter. At Frankfort all measurements were made by wattmeters, and these instruments have been standardised in various ways, and the measurements were as accurate as possible. But at the same time I should like to put in a word for the voltmeter method, the accuracy of which Dr. Fleming seems to doubt. I have made a great many experiments with the three-voltmeter method, using the same instrument for the three readings, by switching over into mercury cups, and I found at first that the method seemed to be very inaccurate, but that was simply due to errors of reading. Now I take about 12 readings in order to determine three quantities—that is to say, I read every quantity four times—and in this way I get readings which are perfectly connsistent with each other, and which, if repeated, give always the same result. This makes me think that the three-voltmeter method is perfectly accurate, provided you

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take enough time over it. Since the paper was read and Dr. Fleming pointed out a possible explanation of the large Foucault loss in the "hedgehog" transformer, I have made a test on a transformer wound with very stout secondary copper, 512 mils per 120, and then exactly the same core wound with a thin wire, and, curiously enough, the transformer which had the stout wire—I am speaking from memory—gave about 120 watts total loss, including hysteresis, and that with very thin copper wire gave 130; so I do not believe the loss is in the copper. The most important part of the paper, to my mind, is the table on slip 26, in which Dr. Fleming has grouped together the efficiencies of the various transformers tested at various loads. Those of us who have to do with central station work, and have to decide whether they should use alternating current or continuous current in any particular case, have up to the present been working to a certain extent in the dark. We knew that the transformers would have less efficiency at light loads, but we had no reliable information. It was generally assumed that transformers at 1-20th of their output have a very poor efficiency—40 or 50 per cent.—and this assumption made, of course, against the system of distribution of which lightly loaded transformers are a necessary part. Now we find that our assumption was less favourable than the truth; we see by these tables that it is quite possible to have transformers with 75 per cent. efficiency at 1-20th of their full load, and as much as 85 per cent. at 1-10th of their full load. Now a transformer need never be worked below 1-20th of its load, and if we employ sub-stations 1-10th will more likely be the minimum load, and this minimum will be raised as time goes on. More houses and different classes of house are put on the mains, with the result that, as time goes on, the different requirements overlap, so that the tendency is to raise the lowest part of the load curve, and therefore increase the efficiency of distribution. But even without waiting for the future we see from Dr. Fleming's paper that the present requirements can be met with something like 80 per cent. efficiency in the hours of lightest load, and a much higher efficiency at fair loads. In other words, when we are earning least we are wasting 20 per cent.,

Mr. Kapp.

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Mr. Kapp

and when we are earning most (that is, in the hours of heavy load, between six and half-past eight) we are getting an efficiency of 96 per cent., wasting only 4 per cent.; so that, taking the whole efficiency of the system for over 24 hours, we get with transformers actually a higher distributing efficiency than with a continuous-current system, if the feeders consume 20 per cent. at the heavy load. Dr. Fleming's figures will help us to determine in a scientific and common-sense manner which particular system shall be used in any particular case, and the whole of the profession will feel that they have to be very grateful to Dr. Fleming for his painstaking research.

The PRESIDENT: May I ask Mr. Kapp, before calling upon anybody else to speak, whether the transformer he referred to as being one with thick copper and thin copper was an open-circuit transformer or a closed magnetic circuit transformer? secondly, was the thick copper next the iron, or the fine wire next the iron? next, which had he magnetised—the iron with the inner coil, or the outer coil? because that would make a total difference in the answer.

Mr. KAPP: A closed-circuit transformer. The thick copper was next the iron; the thin copper was outside this, and was not used in the test at all. The power was supplied at 100 volts to the thick copper in one case, and then a precisely similar carcase was taken and wound with very thin copper exactly the same number of turns, and exactly the same relative position as the thick copper in the real transformer. The copper is not wound quite close to the iron. It is a transformer very much of the same kind as those made by Brown & Boveri, to which Mr. Swinburne has referred. The copper is wound on a separate sleeve and slipped over the core.

The PRESIDENT: Then I understand you to say that you did not transform down, but were trying, so to say, to transform up?

Mr. KAPP: I did not use the fine-wire winding at all; I simply supplied the low-pressure coil with current at 100 volts.

The PRESIDENT: That is not the case quite; we are transforming down. If you had supplied the outer coil, and transformed down, very likely there would have been a different result. But I will go into that later on, in my remarks.

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Mr. SYDNEY EVERSHED: Perhaps I may remind you of an old controversy, which took place in the early days of the Royal Society, as to what happened when you put a fish into a pail brimful of water. Most of the Professors of that day said none of the water would be spilled; others said it would. For the last three years a controversy of a similar type has been raging round the transformer, and at last Dr. Fleming has put the fish in the water, and the result is very much as it was in the old experiment—a good deal of splashing and floundering. I think we ought not to allow this discussion to close without making some comment on the history of the controversy, and particularly as it refers to the constancy of the losses in the iron core of transformers. In the very early days of the Gaulard & Gibbs transformer Professor Ferraris carried out a number of experiments showing that the losses in the core actually fell off as the load was increased. They even became zero, and I am not quite sure that they did not become negative. Those experiments received some support from Dr. Fleming himself; he gave an abstract—an appreciative abstract—of them in his book on alternate-current transformers. The next feature of the controversy was Mr. Mordey's experiment with the "shilling thermometer" and a Mordey transformer. That was a serious mistake, as many people told him at the time—certainly I did; and most of us felt that the experiments of Ferraris and Mordey were not credible. But shortly after that Professor Ayrton made experiments of a similar kind, and confirmed those results. Well, I must confess that when Professor Ayrton lent the authority of his name to the experiments I felt some doubts in my mind as to whether there was not some flaw in the continuity of Nature. However, those of us who were in the habit of dealing with induction in iron, and particularly iron under alternate magnetisation, felt that it was far more probable that there was some mistake in Professor Ayrton's method than that these three experimenters could have hit upon a new law of Nature. In the meantime, in America, Professor Ryan had practically solved the whole question. He experimented in a totally new way on a live transformer, and those of us who read

Mr. Evershed.

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Mr. Evershed.

his results—and I am sorry to say not very many English engineers take much notice of what is done in a scientific way in America—felt that Professor Ryan had practically settled the matters in dispute. He showed that the magnetising current was *not* a sine function; that its form was exactly what had been predicted from the magnetic cycle performed by the iron core; that the core loss was *constant* at all loads; and that there was *no* evidence of viscous hysteresis. A little while after that Mr. Swinburne published his paper at the British Association, in which he described all the essential points in the design of transformers. American engineers took up that paper and Professor Ryan's work, and I must confess I rejoice to see that the transformer which comes out at the top of the tree is American, and not English. We deserve to be left behind in the race, and the Americans deserve to succeed. I may perhaps just refer to my own work in this direction. About three years ago I calculated the losses in various transformers from my knowledge of average commercial iron, and those transformers come out almost exactly identical with those now tested by Dr. Fleming. In other words, Dr. Fleming's experiments are precisely those I should have wished to carry out myself in support of the theory I have always held. I found a good closed transformer might have an efficiency of about 94·5 per cent.; that is to say, a particular transformer discussed in those articles had an efficiency of 94·5 at full load. If you opened the magnetic circuit of that transformer you raised its efficiency to 96 per cent.—a very small rise indeed. But at 1-10th of full load as a closed-circuit transformer it had an efficiency of 85 per cent. (that is to say, about as good as the best of those tested by Dr. Fleming); and when that transformer had its magnetic circuit opened, I clearly showed that its efficiency at 1-10th of full load would be 89 per cent. I cannot help thinking that that estimate of difference between open and closed circuit transformers (only 4 or 5 per cent.) is more likely to be near the truth than the figures so frequently published by Mr. Swinburne; and at the close of those articles I drew attention to the fact that the difference between open and closed circuit transformers, when both are designed on good lines, is small; that,

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unless condensers were very cheap and entirely trustworthy, there was practically nothing in favour of open circuit; and, finally, I showed that if condensers were cheap and trustworthy you might just as well throw away the iron core altogether. Some time ago Professor Ewing made a crucial experiment to find out whether the loss in the core was or was not constant at all loads: he found that it was absolutely constant. At the same time, in conjunction with Mr. Vignoles, I carried out, and published the result of, a small experiment proving quite as conclusively that there was no falling off in the loss as you loaded up a transformer. It is rather curious that nobody took any notice of either Professor Ewing's results or our own, and the tide only turned when Dr. Hopkinson published some experiments carried out on a similar plan to Professor Ryan's, and showed that, first of all, there is no viscous hysteresis, and, secondly, you could calculate the losses in transformers if you knew the cycle the iron went through. And shortly after that Professor Ayrton was converted—saw the errors of his ways, I suppose—and I have no doubt at the present moment Mr. Mordey has grave doubts about the accuracy of his thermometer. There is something very wonderful in the power of conversion of a live transformer, and I cannot help wishing that some of our Professors had come into contact with one some years ago. I can only conclude with congratulating Dr. Fleming very heartily upon his practical work, and can assure him there is more joy in this Institution over one Professor who repents than over the ninety and nine just persons. Mr. Eversheed.

Mr. MORDEY then read the following communication from Mr. W. B. Sayers:—

Mr. W. B. SAYERS [*communicated*]: At the time of writing I have seen only so much of Dr. Fleming's paper as is printed in the technical papers for week ending November 25th, and I cannot therefore offer any remarks—strictly speaking—upon the paper itself. At a time, however, when questions relating to transformers are before the members of the Institution, it is probably not inopportune to call attention to an undoubted fact, which, so far as I am aware, has not up to the present been definitely recognised and appreciated at full import. Mr. Sayers.

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Mr. Sayers. Just as a rapidly alternating current tends to flow more through the outer portion of the metal of the conductor, so, conversely, does a rapidly alternating magnetic flux tend to limit itself to the skin of a mass of iron through which it is passing.

If the annexed figure represents a piece of iron in section having a varying magnetic flux passing through it at right angles to the plane of the paper, eddy-currents (alternating, of course) will be set up in it as indicated by the line drawn inside the figure. The current will of course be spread through the whole region from the centre of the metal to its edge, though not by any means evenly, and quite obviously the central portion of the metal will be subject to the demagnetising effect of the whole of the current, whereas the metal near the surface will only be subjected to the demagnetising effect of the current which passes outside of it—*i.e.*, between it and the skin. It further appears that the very outer skin is actually magnetised in the right direction by the eddy-currents. From these considerations it appears that *calculations* of the losses in transformer cores which do not take into consideration the increased density of the magnetic flux due to the crowding towards the surface of the plates may be more or less erroneous, according to whether the plates are thicker or thinner, or the frequency higher or lower. May not the action referred to be largely the cause of the divergence between actual observed losses due to magnetic hysteresis and the values calculated from Professor Ewing's results on the supposition that the magnetic flux is uniformly distributed in the core plates of transformers?

Mr. Eversheb. Mr. EVERSHED: May I just add a remark, which follows on what Mr. Mordey has just read? The whole of that question of eddy-currents in laminæ was gone into by Professor J. J. Thomson, and also by myself in the papers I have referred to, and the effect of the eddy-current in crowding the induction to the outside is very small. In an extreme case the induction density at the centre of a lamina might be 2 per cent. less than at the outside.

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The PRESIDENT: Sir David Salomons has brought his rapid alternator, and would like to explain it to-night, and I therefore think we should be doing right in adjourning the discussion till December 8th, when some other speakers would like to take part, and Dr. Fleming will reply. I now, therefore, call on Sir David Salomons to explain his alternator.

Sir DAVID SALOMONS: From my long connection with this Institution, and from the great interest I have always taken in its welfare and proceedings, it appeared to me to be my duty to show this little apparatus to the members of the Institution of Electrical Engineers before it was seen by anyone else. This machine is intended to transform a direct current at 100 volts into an alternating current at 200 volts, having a frequency of 1,000,000 per minute. This transformed current is then raised to a pressure of 100,000 volts in the little oil transformer which stands by the side of the machine. You will observe that the apparatus has the appearance of a miniature dynamo of such a weight that two men can easily carry it. The arrangement of its parts is as follows:—

There are two discs, each revolving independently in its bearing. The discs are overhung so as to be capable of being brought to almost touch one another by shifting the bearings upon the bed plate, and grooves are made in this plate for the purpose of being able to approach and retire the bearing standards easily; and shafts are made in such a way that, when everything is properly adjusted, there will be no danger of the faces of the two discs coming in contact.

There are four terminals—two for attaching the wires leading to the continuous-current mains, and two for taking off the high-frequency current. When this apparatus is in action the discs revolve in opposite directions, each having a speed of 1,500 revolutions per minute, so that any point on one disc passes any point on the other disc at a speed equivalent to a point on one disc travelling at 3,000 revolutions per minute past a fixed point. The object of this arrangement is to obtain the equivalent of high speed without running the risks attendant on such a

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high rate of motion. The discs are 1 foot in diameter, and may be regarded as a combination of a continuous-current motor, an exciter, and an alternator.

In one disc the periphery consists of a great number of very small poles, which are wound somewhat like a Ferranti armature. The centre point of this disc has a thick bar placed as a diameter, around which wire is coiled. When the current is flowing through the wire on this disc, the polar ring acts as inductor to the alternating-current armature upon the other disc; and the iron bar acts as the field magnet to the motor. The other disc has exactly similar polar pieces around the periphery, and wound in a similar way, the centre portion being filled in with a flat Gramme ring; this latter being the armature of the motor, and the periphery the armature for the alternating current. In the centre of one disc is the commutator of the motor, and in the centre of the other disc are the brush-holders which carry the brushes to press on the commutator.

When the continuous current is turned on, the motor is set in action; and since the field magnet, as well as the armature of the motor, are free to move, they will divide their speed by revolving in opposite directions. A portion of the current travels around the inductor ring, producing an alternating current in the armature which faces the inductors. Hence an alternating current is produced, the frequency of which may be ascertained in the following manner:—

There are 10 poles to the inch in the peripheries of the discs, and consequently the number of the poles exceeds 360 on the circumference of each disc, since the diameter is 1 foot. The equivalent speed per minute being 3,000, the frequency will exceed 3,000 multiplied by 360—*i.e.*, a number in excess of 1,000,000 per minute. This current is then transformed from its pressure of 200 volts to 100,000 in the transformer, as before mentioned. The quantity of current at 200 volts is about 1 ampere. The small poles on the discs have the appearance of teeth somewhat similar to those of a cog-wheel.

The proposed name of this machine is "direct-to-alternating-current-transformer."

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As you are well aware, a first apparatus is always capable of improvement, and on looking at this transformer many improvements are obvious, chiefly in the engineering details. At the same time, as it now stands, it works very well; but when certain suggested alterations have been made, the output of current and the electro-motive force will be increased. I greatly regret that no current is available to-night; in consequence, I am unable to show you the machine in action, and therefore you must take my word as regards its performance.

This transformer is only just completed. No opportunity, therefore, has been offered for carrying out a series of experiments with it. But I trust that in course of another month or so I may re-introduce to you this machine in its improved form and show it in action, with a few experiments which may prove of interest. When I saw it last in action half an ampere was obtained, and no more, in consequence of a slight defect in a wire, which has since been remedied; and the voltage produced by the transformer did not not exceed 30,000 volts, by reason of one section in it having broken down. Under these conditions, a large vacuum tube was lighted, one terminal of the tube being connected with the transformer, the second terminal held in one hand, while the other hand rested on the other terminal of the transformer, so that the body was brought into the circuit. And it is a curious fact to note that no unpleasant physiological effects were produced; the tube was brighter when the body was so interposed than when the tube was connected with the transformer direct by means of wires. To all appearance the body acted as a condenser, so that the frequency was no doubt considerably raised. The finger passed along the tube caused the light in the tube to rise to it—an effect which is already known, but not always easily obtained.

This machine, therefore, is simple and effective; the object attained being to enable some 10 amperes of direct current to be transformed into a current of great pressure and high frequency, so that experiments which would under most circumstances be dangerous can be performed without fear. By the use of condensers the frequency can, of course, be enormously raised.

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I think I am right in saying that this is the simplest, the lightest, and the cheapest machine produced up to this time for the obtaining of high-frequency currents; and the principles involved are such that by making this transformer on a larger scale there is scarcely any limit to the frequency obtainable. All the present known laws which control electric currents appear no longer to hold good where they have very high frequency. In fact, very little is known of this part of the subject.

It was my desire to experiment with currents of high potential, without incurring the risk of anyone being killed, that led me to consult my friend Professor Fleming as to the best firm to employ to make me such a machine as I required, and he advised me to go to Messrs. Pyke & Harris, which I did. Of course I had followed Tesla's experiments with great interest, but found that he used very cumbrous machinery for obtaining a frequency far below that produced by the small apparatus I am showing to you to-night. The result of my interview with Mr. L. Pyke was that he suggested a machine for giving a high frequency, but I felt that its size and weight would be a bar to its use, to say nothing of the expense. I therefore thought the matter over, and suggested several possible ways of attaining the result by simpler means. Mr. L. Pyke accepted one of my suggestions and combined it with his own, so that you must not imagine that I claim the whole credit of this machine. So far, the credit should be divided between both of us; but there is an additional amount of credit due to Mr. Pyke, for his firm has constructed this machine entirely under his superintendence, and the actual details he has himself worked out.

Professor Ewing has just had a high-frequency machine constructed to his design, combined with a Parsons engine; but I believe the frequency is lower than this one, and its weight, of course, is vastly greater.

I have regarded that portability is an essential for a laboratory machine, since it is often required for a lecture at a distance. I would mention that the improvements which are contemplated, apart from improving the present machine in some of its details, are to add another pair of discs worked by the same motor and

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giving a lower frequency of, say, 100 per second. This will enable a lecturer to have at his command high or low frequency currents at will; and the additional weight will not be very great, nor will the expense of this addition be large, since the number of polar pieces will be far fewer, and the discs in consequence more easy to construct.

I must point out that this little direct-to-alternating-current-transformer has in it the possibility of uses which may already occur to your own minds before mentioning them. By slight modifications you have the means of converting an alternating current into a direct current for charging accumulators or for any other purpose. You have also the essence of an alternating-current motor. It might be questioned whether the machine has really 1,000,000 frequency. By the logic of its mechanism it certainly has. A revolving mirror could be used to demonstrate the fact experimentally. But one argument strongly in favour is this: Suppose that 1,000,000 frequency does not exist, then it would indicate that the current is not reversed each time that a north and south pole inductor is passed by an armature pole—*i.e.*, there would be set up a kind of lag. Under such conditions the probability is that, if the lag is at all considerable, instead of an alternating current being produced, a continuous one would result, no current at all, or at any rate a current of such a character that an ordinary form of transformer could not be used with it. But it has been shown that this is not the case. Therefore it is fair to assume that the theoretical results are attained.

The PRESIDENT: I have to announce that the following candidates have been elected:—

Members:

Hunter, William D. | Johnson, William.

Associates:

Chisholm, J. W. | Harrison, Captain Thomas, R.E., D.S.O.

The meeting then adjourned.

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The Twenty-first Annual General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 8th, 1892—Professor W. E. AYRTON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on Thursday, December 1st, were read and approved.

The names of new candidates for election into the Institution were read and ordered to be suspended.

The following transfer was announced as having been approved by the Council :—

From the class of Associates to that of Members—

A. J. Arnot.

Mr. F. V. Andersen, Mr. C. Bright, Mr. H. C. Donovan, and Mr. J. L. Ovens were appointed scrutineers of the ballot for President, Council, and Officers for the year 1893, and for the election of new members.

The PRESIDENT: During the past year death has, I am sorry to say, been busy in our ranks, and I have to-night to record the death of one of our most distinguished Foreign Members, Dr. Werner von Siemens, one of a family of inventors, of whom he was perhaps the most famous. The early struggles of the brothers Werner and William, our twice President, are quite pathetic, and are becoming historic. Those who desire to see the wide ground covered by the researches of Werner Siemens, and the wonderful results that he was able to attain, cannot do better than study the memoirs which he published last year, and the first volume of which has recently been translated into English. From them something else may be also learnt, which I believe it was the object of Dr. Werner von Siemens to teach in writing these memoirs, and that is, how a lad without money, without position, and without any of those accidents which give aid to a man's career, was able, as he called it, to do good work

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or, as we should call it, to achieve such wonderful success in electrical development. I therefore beg to move the following resolution :—

"That the President, Council, and Members of this Institution "desire to record hereby their profound regret at the decease of "their distinguished Foreign Member and Local Honorary Secre- "tary and Treasurer in Germany, Dr. Werner von Siemens, and "their deep sense of the loss which has by this event been sus- "tained by the whole scientific world; that they further desire "to express their sincere sympathy with his widow and the other "members of his family in their great bereavement."

Professor GEORGE FORBES: I beg to be allowed to second this motion. I am sure that every member of this Institution, as well as every member of the world of physical science, must feel that we have suffered a very severe loss by the death of Dr. Werner von Siemens. It would be superfluous for anyone here to speak of the merits of that great man, and I can only say that everyone who ever came into contact with him—even those who differed from him—felt the power of his presence and the ability of the man; I think everyone must feel, as I most certainly do, that no words one could utter are necessary to enable us to realise more than we do at present what a loss the scientific world has sustained. I beg to second the motion.

The resolution was put to the meeting and carried unanimously.

The SECRETARY then read the Annual Report of the Council, as follows:—

REPORT OF THE COUNCIL TO THE ANNUAL GENERAL MEETING, 8TH DECEMBER, 1892.

ELECTIONS AND TRANSFERS.

The number of members elected into the Institution during the current year has been, as might naturally be expected, considerably fewer than those elected last year, as, in order to join under the old rates of entrance fee and annual subscription, a very large number of candidates presented themselves for election

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in November, and even in December, of that year, who would otherwise certainly have desired to have their election deferred until January of this year.

Making allowance for this circumstance, the accessions to the register have been considerably more than the average, and consist of 9 Foreign Members, 12 Members, 101 Associates, and 98 Students—or a total of 220—and 37 candidates have been approved for ballot at the first meeting in January next.

The circumstances above alluded to affected with equal force the number of transfers during the year; only 15 Associates having been transferred to the class of Members, and 14 Students to the class of Associates.

Deaths and Resignations.

The losses by death during the year, although not above the average, after allowing for the increased register, number 21, and unhappily include many important members: Sir George Biddel Airy, formerly Astronomer Royal, *Honorary Member*;—Don Alonso Prados; Professor Shida, our Local Honorary Secretary in Japan; Mr. C. Nielsen, Chief of the Telegraph Department in Norway, who represented his country at all the Telegraphic Conferences since 1865, and who also represented the Institution as Local Honorary Secretary; Monsieur M. Simon; and, only two days ago, Dr. Werner von Siemens, one of the most distinguished electricians of the day, and whose loss will be severely felt in the scientific world,—*Foreign Members.* The 12 *Members* lost to us comprise Mr. Edward Graves, Engineer-in-Chief of the Postal Telegraphs, a Past-President, and for many years the Honorary Treasurer of the Institution, to whose long and invaluable services reference has quite recently been made; Sir John Coode, a Past-President of the Institution of Civil Engineers; Messrs. J. A. Betts, G. G. Newman, Jas. Radcliffe (three of our oldest members), G. B. Almond, J. A. Chambers, F. N. Gisborne, C. Hortsek, the Duke of Marlborough, Mr. H. Starke, and Mr. P. W. Willans. 3 *Associates*—viz., Messrs. H. Corderoy, J. Healey, and E. A. Horton—and 1 *Student*—Mr. Norman F. Wicks—are also among the deceased.

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4 Foreign Members, 2 Members, 17 Associates, and 1 Student have resigned during the year.

Papers.

In addition to the admirable and instructive Address of the President, the following papers dealing with the experimental as well as with the practical applications of electrical science have been read during the year, and have led to important and interesting discussions, viz.:—

LIST OF PAPERS READ BEFORE THE INSTITUTION DURING THE YEAR 1892.

Date.	Title.	Author.
Feb. 3.	Experiments with Alternate Currents of High Potential and High Frequency ...	Nikola Tesla, Foreign Member.
,, 11.	Some Experimental Investigations of Alternate Currents	A. Siemens, Vice-President.
Mar. 10.	Oil as an Insulator	Professor D. E. Hughes, Past-President.
,, 24.	Load Diagrams of Electric Tramways, and the Cost of Electric Traction	A. Reckenzaun, Member.
April 28.	Notes on the Light of the Electric Arc ...	A. P. Trotter, Member.
May 12.	On the Cause of the Changes of Electromotive Force in Secondary Batteries ...	Dr. J. H. Gladstone, Member, and W Hibbert, Associate.
May 26.	Electrical Distribution by the Newcastle-on-Tyne Electric Supply Co., Limited...	A. W. Heaviside and R. C. Jackson, Members.
Not read, but simply published in the *Journal*.	The Problems of Commercial Electrolysis ...	J. Swinburne, Member.
Nov. 24.	Experimental Researches on Alternate-Current Transformers	Dr. J. A. Fleming, Member

Annual Premiums.

In respect of papers contributed during the twelve months ending 30th June last, other than those by Members of the

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Council, the following annual premiums have been awarded by the Council :—

The Institution Premium, value £10, to Mr. Nikola Tesla, Foreign Member, for his paper entitled, "Experiments with "Alternate Currents of High Potential and High Frequency."

The Paris Electrical Exhibition Premium, value £5, to Mr. Anthony Reckenzaun, Member, for his paper on "Load Diagrams "of Electric Tramways, and the Cost of Electric Traction."

The Fahie Premium, value £5, to Messrs. A. W. Heaviside and R. C. Jackson, Members, for their paper on "Electrical "Distribution by the Newcastle-on-Tyne Electric Supply Com- "pany, Limited."

The Salomons Scholarship.

As announced at the meeting of May 26th, the Council awarded this scholarship to Mr. C. H. C. Woodhouse, a matriculated second year's student of the department of electrical engineering at the Central Institution.

Annual Conversazione.

The President's Conversazione, on the 1st of July, was again held in the Galleries of the Royal Institute of Painters in Water Colours, the attendance being very large.

Annual Dinner.

The fourth Annual Dinner of the Institution took place on the 18th of November, and was also largely attended.

Chicago Exhibition, 1893.

It having been brought to the notice of your Council that great reluctance existed on the part of electrical manufacturers in the United Kingdom to take part in the forthcoming Exhibition at Chicago, and that the probable result would be an almost complete absence of representation of that branch of British industry, creating an erroneous and unfavourable impression of its resources, it occurred to them that it might be possible, with the aid of such of our members who are manufacturers or closely

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allied to manufacturing firms, to form under the auspices of the Institution a collective representative exhibit. It was intended to raise a guarantee fund to meet such necessary expenses over and above the cost of forwarding the exhibits (which it was proposed should be defrayed by the manufacturers) as H.M. Commissioners would not undertake in respect of such exhibits.

They accordingly appointed a committee to consider this matter, and a circular was issued inviting exhibits; but they regret to say that the response has been too unsatisfactory to justify their moving further in the matter, the high duties imposed by the United States upon British manufactures being in nearly all cases assigned as the reason for not exhibiting.

Financial Position.

The financial position of the Institution is still satisfactory. The increased expenditure alluded to in the last Annual Report has been found to be much more than covered by the increased revenue. The amount of £327 10s. has been invested during the year on account of life compositions, and a considerable further amount will shortly be added to the General Investment Account, which practically represents the savings of the Institution.

THE LIBRARY.

REPORT OF THE SECRETARY.

I beg to report that the accessions to the Library during the year number 62; 10 of these were purchased, and the remainder are presentations from authors or publishers.

The specifications of all electrical patents continue to be supplied to the Institution, by the kindness of H.M. Commissioners of Patents.

The number of patents applied for this year, up to the 30th of November, was 21,656, of which 1,284, or 5·92 per cent., were electrical.

The corresponding numbers last years were 20,833 and 1,337, or 6·4 per cent.

The number of periodicals and printed proceedings of other

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Societies received regularly is somewhat larger than last year, as may be seen by the list appended hereto.

The number of visitors to the Library during the year has been 609, of whom 95 were non-members. The corresponding numbers last year were 347 and 25 respectively.

In accordance with the instructions of the Council, I have had compiled an index to the 10 volumes Nos. 11 to 20 of the *Journal of Proceedings*. This, I am happy to say, will be ready for issue before the end of the year.

F. H. WEBB,
Secretary.

APPENDIX TO SECRETARY'S REPORT.

TRANSACTIONS, PROCEEDINGS, &c., RECEIVED BY THE INSTITUTION.

ENGLISH.

Asiatic Society of Bengal, Journal and Proceedings.
Greenwich Magnetical and Meteorological Observations.
Institute of Patent Agents, Transactions.
Institution of Civil Engineers, Proceedings.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
Liverpool Engineering Society, Proceedings.
Physical Society, Proceedings.
Royal Dublin Society, Transactions and Proceedings.
Royal Engineers' Institute, Proceedings.
Royal Institution, Proceedings.
Royal Meteorological Society, Proceedings.
Royal Society, Proceedings of.
*Royal Society, Philosophical Transactions of.
Royal United Service Institution, Proceedings.
Society of Arts, Journal.
Society of Chemical Industry, Journal.
Society of Engineers, Proceedings.
University College Calendar.

AMERICAN.

American Academy of Science and Arts, Proceedings.
American Institute of Electrical Engineers, Transactions.
Canadian Society of Civil Engineers, Transactions.
Franklin Institute, Journal of.

* Presented by Professor D. E. Hughes, F R.S. (Past-President).

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John Hopkins University Circulars.
Library Bulletin of Cornell University.
Ordnance Department of the United States, Notes.

FRENCH.

Bulletin de l'Association des Ingénieurs Électriciens sortis de l'Institut Électro-Technique Montefiore.
L'Académie des Sciences, Comptes Rendus Hebdomadaires des Séances de.
Société Belge d'Électriciens, Bulletin de la.
Société Française de Physique, Séances de la.
Société des Ingénieurs Civils. Mémoires.
Société Internationale des Électriciens, Bulletin de la.
Société Scientifique Industrielle de Marseille, Bulletin de la.

LIST OF PERIODICALS RECEIVED BY THE INSTITUTION.

ENGLISH.

Electrical Engineer.
Electrical Plant.
Electrician.
Electricity.
Engineer.
Engineering.
English Mechanic and World of Science.
Illustrated Official Journal, Patents.
Industries.
Lightning.
Nature.
Philosophical Magazine.
Telegraphic Journal and Electrical Review

AMERICAN.

Electrical Engineer.
Electrical Review.
Electrical World.
Electricity.
Journal of the Telegraph.
Science.
Scientific American.

FRENCH

Annales Télégraphiques.
L'Électricité.
L'Électricien.
L'Industrie Électrique.
Journal de Physique.
Journal Télégraphique.
La Lumière Électrique.

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GERMAN.

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Electrotechnischer Anzeiger.
Electrotechnische Zeitschrift.
Verhandlungen des Vereins zur Beförderung des Gewerbfleisses.
Zeitschrift für Elektrotechnik.
Zeitschrift für Instrumentkunde.

THE PRESIDENT: I beg to move—"That the Report of the "Council as just now read be received and adopted, and that it "be printed in the Journal of the Institution."

Mr. AUGUSTUS STROH seconded the motion.

The PRESIDENT: Before putting that motion, I must ask if anybody wishes to ask any questions.

Mr. A. P. TROTTER: Sir, I have given notice of a question to the Secretary. The present process of balloting for Associates has been in vogue for some time. I wish to say nothing about the election of Students—that should be encouraged; nor of full Members—that is chiefly in the hands of the Council. [Sir David Salomons: "No, no"] Possibly not; but I understand that the Council pays special selection and investigation to the qualification of Members. But I refer only to the election of Associates. As we come into this room we receive balloting papers, and it appears to many members—it may be quite wrong; I merely ask the question—that it is almost impossible for anyone to effectually oppose the election of an Associate. Take, for example, this case: Supposing six names are down on each balloting paper; a large number of members, I believe, without any personal knowledge of those Associates, put their papers into the box. Members desiring to blackball any person are practically outvoted by five to one. I therefore wish to ask, just to test whether this is the case, or whether I and others are in error, how many candidates have presented themselves for election as Associates since the present mode of balloting was adopted, and how many of those have been actually elected.

The SECRETARY: Since the present method has been in vogue, 163 candidates have been nominated for election as Associates, and 163 have been elected.

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Mr. TROTTER: I am much obliged for the information. I have no comments to make, and no suggestions to offer.

Major-General WEBBER, in a few remarks, concurred generally in what Mr. Trotter had said, and thought it was desirable that the Council should reconsider the arrangements for the election of Associates.

Sir DAVID SALOMONS: I think I can answer the question in a moment. Every nomination of an Associate requires the names of a proposer and seconder; and if these are those of thoroughly reliable persons, we accept their word as to the qualification of the candidate, unless any special question arises. I think this is satisfactory. As regards the use of ballot boxes, I had better explain why they were discontinued, as my friend General Webber is in favour of their retention. It is a matter of congratulation to us that those boxes have disappeared, the simple reason being because there are so many joining our ranks it is found impossible to use the old system.

The SECRETARY: The President allows me to say a word in connection with this matter. I do not know whether it has escaped General Webber's memory, or whether he was away at the time the change was made, but I think it right to remind him and the meeting that the change in the mode of balloting for new members was made because the old plan was found impracticable without using the same ballot boxes two or three times over on the same evening. For the same reason the Institution of Civil Engineers abolished the ballot boxes and adopted the system of balloting papers. We have followed their footsteps in this, as in other matters, simply because we found it to be practically impossible to go on in the old way. Sometimes we have had as many as 50 or 60 candidates up for election on one evening, and it was very difficult to get members to remain while the change of ballot boxes was being made. It was on that account that the system was altered, and an explanation to that effect was given by the then President at the Ordinary General Meeting held on the 12th of November of last year.

Mr. G. L. ADDENBROOKE: I believe the names of two supporters are required for the election of Associates. I simply beg to move that a third name be required.

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The SECRETARY: The adoption of any such change as that suggested by Mr. Addenbrooke would necessitate a Special General Meeting and another confirmatory meeting. Our rules, which are really Articles of Association, are at present fully complied with. They require that a candidate shall be proposed by one member and seconded by another. As a general rule, we have several additional names.

A MEMBER: I should like to ask how many black balls at present exclude?

The SECRETARY: At least two-thirds of the total number of votes given must be in favour of the candidate, to ensure his election. I may mention, with reference to Mr. Trotter's question, that I think perhaps a wrong inference was drawn from my reply, judging from the cheers with which it was received, namely, that all Associates proposed since the new system was adopted had been elected. I should like to add that during the long period—nearly 15 years—that I have had the honour of being your Secretary, there has been, I believe, only one case—certainly there have not been more than two—where a candidate has been blackballed; and in such a case, on the written requisition of three members, a second ballot can be granted. In the case or cases to which I have alluded that was done, and the candidate or candidates were re-elected without any further difficulty. With that exception, or those two exceptions, during my time, even under the old system there was no blackballing. I do not think, therefore, that the new system can be considered to have affected that aspect of the matter.

Mr. KAPP: I quite appreciate Mr. Trotter's intention of having a more rigorous ballot. But, from a practical point of view, I do not believe that ballot boxes are any better than the papers, and I speak from my own experience. I am one of those individuals who have often been pressed by the Secretary to do certain work. I remember, at the end of certain meetings, when I wanted to get away to catch my train, the Secretary has come to me and said, "Now vote: here are the balls." And I would throw them in, and sometimes they went into the wrong compartment. I maintain that is a far worse system than when the

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Secretary gives me a paper and I can strike a name out if I want to blackball. The hurried voting with the ballot box was certainly worse than the present system.

Mr. SWINBURNE: Does striking out one name blackball that candidate, or does it merely mean that he is not voted for? You cannot, as I understand it, blackball one man without blackballing the whole list.

Mr. VIGNOLES: May I make a small suggestion which may get over the difficulty? It appears to be agreed that the difficulty as regards the use of ballot boxes is the number of boxes that would be necessary, and it is a very serious one. But you could combine the advantages of both systems by simply voting for or against a person on the present balloting papers. That is to say, you should make a certain mark if you wish to vote for, and strike out the name if you wish to vote against a man. That seems to me to combine the advantages of both systems.

Sir DAVID SALOMONS: Perhaps you would accept a proposition on the part of the Council at the next meeting. There appears to be a difficulty, and perhaps if we had something printed at the bottom of the ballot paper it would get over it.

A MEMBER: Would you have the proposer and seconder printed as well? That, perhaps, would satisfy some members.

The PRESIDENT: Are these definite propositions, or merely suggestions? Of course there is no doubt that this Institution labours under one very great disadvantage: it is extremely popular, and is becoming more and more popular, and the number of candidates coming in is so very large that it is extremely difficult to do anything but what we do; but, at the same time, if any definite proposition is put before the meeting, the Council of course would give it full consideration.

Mr. BIGGS: What on earth are the Council for if they are not the proper persons to decide upon the qualifications of candidates? It is not a matter which should concern the general body of members.

Mr. TROTTER: In asking the question which I put, I distinctly declined to offer any suggestions, because I think it is one which

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the Council are fully able to deal with, and a matter which should be left to the Council, in whom we ought to have confidence.

The PRESIDENT: It has been moved and seconded that the Report of the Council be received and adopted, and printed in the Journal of the Institution. I now put the motion.

The motion was carried unanimously.

Major-General WEBBER: I am called upon to move—"That "the members of this Institution desire to offer to the President, "Council, and Members of the Institution of Civil Engineers their "cordial thanks for their continued kindness and liberality in "allowing the use of their hall for the meetings of this Institution." None of us who have used this beautiful hall, as we have done for the last 21 years, can but feel grateful for the accommodation which we receive, and the kindness with which our request has been met, not only for its fortnightly use, but also for its increased use when the needs of discussion of papers, and so forth, have required us to ask for weekly meetings. I think this expression of the thanks of the Institution on this occasion is especially appropriate, for during the last year we have taken every advantage of this kindness by holding more frequent meetings than were put down on our sessional cards.

Mr. MORDEY having seconded the motion, it was carried unanimously.

Sir DAVID SALOMONS: I have to move—"That the thanks of "this Institution are due to the Local Honorary Secretaries and "Treasurers for their kind services during the past year." I will not devote any time to making a speech, as there is a great deal before us; but I would call attention to the fact, which we must all deeply regret, that we have lost no less than three foreign Secretaries by death during this year—viz., the Secretaries for Japan, Professor Shida; for Norway, Mr. Nielsen; and now, for Germany, Dr. Werner von Siemens, to whose family we have just passed a vote of condolence. I hope next year, when a similar resolution is passed, we shall have a more pleasant task before us in feeling that it is to those who are living that we are returning our thanks.

Professor HUGHES seconded the motion, which was unanimously carried.

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Mr. SWINBURNE: I have to move—"That the thanks of this "Institution are due to Mr. F. C. Danvers and Mr. Augustus "Stroh for their kind services as Honorary Auditors." I am sure you will all agree with me that the duties of Auditors are arduous and very responsible, but certainly not interesting from a scientific point of view. Anyone who has had anything to do with book-keeping will agree that our cordial thanks are due to the Honorary Auditors.

The motion, being seconded, was unanimously carried.

Mr. STROH: In the absence of Mr. Danvers, I rise to express my grateful acknowledgment of the vote which you have so kindly passed. The duties of the Honorary Auditors are not very heavy, I am happy to say. We have always performed them with very great pleasure, and to the best of our ability. I thank you very much.

Mr. CROMPTON: I move—"That the thanks of this Institution "are due to Messrs. Wilson, Bristows, & Carpmael for the kind "and valuable services rendered by them as Honorary Solicitors."

Dr. FLEMING seconded the motion, which was unanimously carried.

The PRESIDENT: We can now resume the discussion on Dr. Fleming's paper, "Experimental Researches on Alternate-"Current Transformers." I call on Mr. Mordey to re-open the discussion.

Mr. W. M. MORDEY: I wish to confine what I have to say on Dr. Fleming's paper, as far as time allows, to one point—the very interesting question of the sudden rush of magnetising current into transformers, noticed by him, and very fully described in the latter part of his paper. But before proceeding to that I would like to refer to the question of the loss in the iron of transformers. My name has been a good deal connected with this question, because I drew attention to it here some time ago. Some experiments which we made in the works of the Brush Company tended to show that the loss in the iron decreased as the load increased in the transformer. Some who have recently experimented on the question have got results that do not confirm mine. A great many experiments have Mr. Mordey.

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Mr. Mordey. been made. Professor Ayrton has made two sets of experiments, in which he discovered the effect. He has now, with Dr. Sumpner, made another set of experiments, in which the effect is less. In their paper read before the British Association the effect is, however, clearly shown in the curve given, but it is not an important one; there is, however, a decided drop in the iron loss. I think we may say that Professor Ayrton "cancels out." He has made three sets of experiments, and two are decidedly in favour of the view I took up, and the other is not so decided. Several other experimenters have made investigations, chief among them being Professor Ewing, whose arrangement was, however, so unlike a commercial transformer that I was not surprised at his result; and now Dr. Fleming in this admirable paper has given some results from transformers of my own design, and of others', and he fails to discover this effect. It will be interesting to find an explanation of the fact that Dr. Fleming's values at no load almost exactly confirm mine, while we differ at full load. The transformers used in his tests differ in some details of arrangement from those used in my experiments, and it may be that we are both right. But possibly I was wrong. At any rate, I feel that, when time allows, I should make some more experiments. I have not made any hitherto, partly for want of time, and because it has only recently been borne in upon me that I am possibly wrong. I am not at all sure. However, if I am wrong, I will come and make a clean breast of it. But I would like to say a word as to the way in which the question has been treated. When I first brought the matter up at the Institution here (I think on the discussion of a paper by Mr. Swinburne), the thing was spoken of by the *Electrician*, and elsewhere, as absurd; it was pointed out in many columns that it was against the laws of Nature. Even last week one of the speakers referred to it as being now quite settled, and was very comforted to find that the "laws of Nature" were still observed by transformers. Well, I always envy those people who have had a special revelation on the subject of the "laws "of Nature," especially as applied to iron under the influence of alternating currents. As a rule, they hide behind the columns

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of the technical papers and throw leaderettes at the heads of their friends. These utterances have always amused me, partly because, when I first noticed that effect and described it here, I got a letter from Professor Ewing, in which he said he had been specially interested in this question as to the iron loss as affected by the load, "because," he says, "it seems to me your "result is in agreement with my molecular theory." Now here are these sapient people whose knowledge of the laws of Nature is so prodigious that they were perfectly shocked by my results; there was Professor Ewing, who has perhaps made a closer study of the losses in iron and of the magnetic behaviour of iron than anybody else, or all the others put together, who quite thought the thing was in accord with his most carefully formed views. I will not occupy any further time on that, except to say that really the question, however interesting in its scientific aspects, is not so important as it appears to be thought, for this reason: if I am right—if the losses in iron do decrease with the load—it only affects the efficiency at full loads or at considerable loads; whereas the great and important question on which this battle of transformer design, this question of transformer distribution generally, has turned, has been the question of losses at no load. Mr. Mordey.

There is a very proper feeling in this Institution against the advertising of the wares of any particular firm or manufacturer, but we must remember that this is a paper intended for the assistance of practical manufacturers, as Professor Fleming expressly states in his concluding paragraph, and practical manufacturers are not in business for their health. They are in business for commercial purposes; they want to make things that they can sell, and that they can make and sell at a profit, and with which buyers can work at a profit. We can only properly and usefully discuss such matters by giving careful attention to questions of price and profit. I think that perhaps we are apt to be a little too squeamish on this subject. We may freely use a certain letter L to represent self-induction, but if we put two horizontal crosses on the L, and make it £, to represent an even more important engineering quantity than self-induction, it is "bad form."

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Mr. Mordey. While on this subject, if time permitted, I would have wished to make some remarks on the commercial aspects of the subject; but I will only now say that it is merely a question of cost to get transformers so good that the losses at no load are negligible, and that it is not a difficult matter to calculate to what extent it is desirable, commercially, to invest money in efficient transformers rather than to spend it in power.

I said I wished to speak on the question of the sudden rush of magnetising current into transformers. When I heard Dr. Fleming's paper read I was astonished at that result, because I had never observed it myself, nor am I aware that any of our people had ever observed it. It struck me as an important thing, because I thought it might throw light on certain effects and breakdowns of transformers that I had not been able to understand. In the early part of our experience transformers, that normally ran quite well, would sometimes break down, apparently without any adequate cause. So, after the paper was read, I spent a couple of afternoons in making experiments which, with your permission, I will describe, as perhaps they throw a little additional light on this curious effect. By the kindness of Major Waller, Mr. Dalton, and Mr. Partridge, of the London Electric Supply Corporation, I was able to make some experiments at the testing room in the Adelphi with current from the London Electric mains. At first I was inclined to think the effect was a capacity effect. But Dr. Fleming's explanation is the true one. He suggested the explanation, and I am very glad to have been able to make a definite experiment to show that his explanation is the correct one.

(*a.*) I first used a 5-H.P. Ferranti transformer on the 2,400-volt mains from Trafalgar Square. On the same mains there were many transformers in use in customers' houses, and in parallel with this transformer was another, only a few feet away. I mention this because it indicates that the effect could scarcely be due to capacity, as a number of transformers connected in parallel form a connection across the mains close at hand, and seem to preclude such an explanation. Then a

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100-volt lamp was put in series with the primary of this transformer, the secondary being open, and the primary circuit was closed rapidly. Out of 47 contacts there were seven bad "rushes" of current. These rushes were so brilliant that I was afraid the lamp would break—exactly as Dr. Fleming described. Besides the seven bad rushes of magnetising current when the transformer was switched on, out of the 47 times there were a number in which there was a minor rush—a considerable increase over the normal magnetising current. Mr. Mordey

(*b.*) A secondary circuit of non-inductional resistance was put on, taking 11 amperes at 50 volts. The rushes on making the primary were just as noticeable as before. Out of 46 contacts there were five bad rushes, and six or eight minor rushes. The secondary was closed all the time.

(*c.*) With the same arrangement, but with the primary closed all the time, the secondary was made and broken many times. There was no abnormal increase of current in the primary.

(*d.*) A 20-H.P. Ferranti transformer was put on, and tried as in (*a*). Sixty contacts were made; there were five or six small rushes of current, and then the last contact broke the lamp by an extremely violent rush. I think there must have been fully 20 amperes through that lamp instead of less than half an ampere. That entirely confirms, so far, what Dr. Fleming observed.

(*e.*) In place of the Ferranti, I put one of my 20-H.P. transformers, made by the Brush Company for the London Electric Company, with a 50-volt lamp on the primary. I tried both the "live" side and the earthed side of the primary, always with the secondary circuit open. Out of 56 contacts I only noticed one little flash—one very small increase.

For more sensitiveness, a 100-volt lamp was put on, and in 100 contacts there were only two or three trifling rushes of current, that were scarcely perceptible, the effect was so slight. Several hundred contacts were then made with this transformer, and always with the same result.

This finished the experiments at the Adelphi, which were all made at 83 $\sim$. These experiments so far show that the effect

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Mr. Mordey. may range from very slight to very objectionable, according to the kind of transformer used. Different qualities of iron used may explain this. The presence of a load on a secondary does not prevent it; the presence of other transformers in parallel disposes of the supposition that it is a capacity effect on the mains.

(*f.*) Our 24-H.P. transformers have been supplied to the City of London Company, and as some were being put on I took the occasion of breaking and making circuit there, but could get no rush. This was at 100 ∿.

(*g.*) One of these 24-H.P. transformers was then tried in the shops in the same way, and out of 485 contacts I could get no effect. The contacts were made by a switch.

(*h.*) Then I made a number of experiments with a large amount of twin rubber-covered cable in the circuit to give a certain amount of capacity, but I got no rush effect at all.

(*i.*) Then I took a 5-H.P. Ferranti transformer—the one I had used at the London Electric—and made 400 contacts with that, and got no effect at all at 100 ∿, whereas at the London Electric station at 83 ∿ the effect had been very marked and frequent.

(*j.*) Then I made 100 contacts at 1,800 volts with this Ferranti transformer, and at 100 ∿ I could get nothing at all, but got about a dozen flashes at 83 ∿.

(*k.*) Then I put my 24-H.P. transformer on at 83 ∿ and 1,800 volts, and out of 126 contacts got four minor rushes.

(*l.*) At 100 ∿ and 2,000 volts a large number of contacts were made, with no effect.

I thought possibly the rapidity of closing the switch might have something to do with it—that at a low frequency one would be more likely to get on to a definite part of the curve instead of coming down on two or three waves.

So (*m.*) I reduced the frequency, and instead of 100 ∿ I tried 50 ∿, but with slow contacts I got no effect.

(*n.*) Then I tried by making very quick contacts by tapping the wire, and got a very violent effect indeed at 50 periods—showing, in fact, that the contact made slowly and deliberately was not an instantaneous contact at all, but that with a very

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quick contact you get this effect. It must be remembered that a small spark always strikes across between the contact pieces, before the actual contact is made, so that, even with a rapidly acting switch, a quite instantaneous closing of the circuit does not take place. (In order to ascertain if different degrees of magnetisation had any effect, the same transformer was tried at voltages varying from 1,000 to 2,400, always with about the same results.) Mr. Mordey.

(*o.*) At 100 ∿ and slow contacts there was no effect; but with very rapid contacts there were occasional minor rushes, the lamp varying from black to fair incandescence, showing that the magnetising current ran up to perhaps half an ampere.

These experiments show that there is a relation between frequency and time occupied in switching: the lower the frequency the worse the result. Of course when one gets on the right track it is pretty easy to go on.

Then it occurred to me to try the effect of rapid making following slow breaking. I thought that a slow break might leave the transformer unmagnetised. I thought if I could draw out the current so that the final waves gradually decreased until they were very small, that I should leave the residual magnetism in the transformer very slight indeed. It appeared probable that rapid making would be harmless if it followed slow breaking, because by slow breaking the current would be tailed off in gradually decreasing waves, as in Fig. 1.

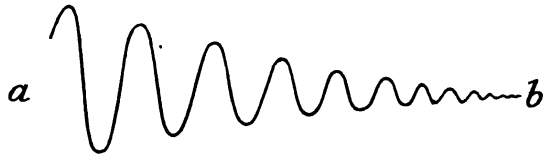

FIG. 1.

(*p.*) I made a number of trials at 83 periods at the works with the small Ferranti transformer, and they fully confirmed this supposition. It was not possible to get the rush effect if you slowly broke the current by merely using a slow-acting switch. Slow breaking followed by rapid making never showed more than a very slight "rush." Slow breaking followed by slow making

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Mr. Mordey never showed any effect at all, nor did slow making. That is to say, if the current is made to decrease slowly at breaking (say from *a* to *b* of Fig. 1), or to increase slowly at making (say from *b* to *a* of Fig. 1), the effect is avoided.

I do not find, with my transformers, that any precautions are necessary. The good quality of the iron used, combined with the higher frequency, appear sufficient to prevent this effect, and switches of ordinary form may be used. But when lower frequency is employed, it appears desirable to use a switch, preferably with a carbon sparking piece, that breaks and makes rather slowly. It does not appear that any further precaution is needed.

Then there was a question mentioned by Dr. Fleming in connection with this subject that I did not understand. He said if the transformer were put on backwards--that is to say, if it were magnetised by the secondary instead of by the primary—this effect was slight or did not occur. I did not understand that. I did not see, if residual magnetism was the cause, why the effect should not occur when the magnetising current was applied to the secondary. Well, I made some experiments, and found it did occur, if the current were made through the secondary rapidly. But the real reason why it does not occur under ordinary circumstances if the transformer be magnetised through the secondary, appears to be that the resistance of the switch contacts bears a much greater proportion to the resistance of the whole in the case of the secondary switch with a large current than in the case of the primary switch with a very small current.

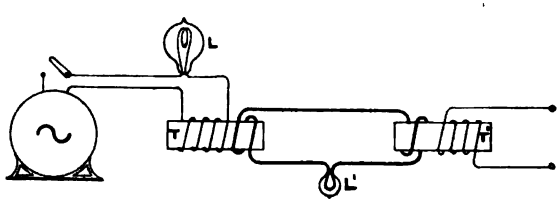

FIG. 2.

(*q*.) To see whether the rush occurred when the transformer was indirectly magnetised on the secondary side—the low-tension side—I made an experiment as shown in Fig 2.

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On the left is the alternator. The secondaries of the transformers, T and T′, are connected, and in their circuit is a little 1-C.P. "fairy" lamp, L′. The thin wire of T′ is open. It will be seen, therefore, that a rush on magnetising the primary of T will be seen on lamp L, and that a rush on magnetising the secondary of T′ will be shown on the lamp L′. The two lamps were specially chosen for their respective positions, L being only slightly incandesced by the normal magnetising current of the primary of T, while the normal magnetising current of the secondary of T′ only slightly incandesced the lamp L′. An excessive incandescence of either lamp, therefore, would show a "rush" on its respective circuit. I found, on making and breaking the circuit quickly at the switch, I got the rush in lamp L occasionally. I found also that I occasionally got the rush in lamp L′, but not usually at the same time as in L. T was magnetised, and had its own rush quite independently of T′. One lamp would glow at one time and the other at another. Sometimes I got a simultaneous glow. But the rush of the current in one was not usually simultaneous with the rush of current in the other. That is rather curious, but is probably a result of lag, and may perhaps be explained by supposing that the currents in T′ are slightly later in phase than in T. A sudden contact may therefore find the iron of T in a state favourable to a rush, while that of T′ is unfavourable. When a simultaneous rush occurs it indicates that both have been left by the last receding current in a magnetised state sufficiently favourable to allow a rush. Mr. Mordey.

(*r*.) I wanted to see whether, when the rush occurred on the primary, it penetrated to the secondary—that is to say, whether a large wave of current set up a large wave of magnetism. I put a 100-volt 16-C.P. lamp on the secondary 100-volt circuit, and made and broke the primary rapidly, but found no such effect. Whether a rush occurred in making the primary circuit or not, the secondary lamp always glowed with the same incandescence, showing that, although we got a rush of current many times greater than the normal magnetising current going into the transformer, it did not penetrate through

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Mr. Mordey. to the secondary. Therefore there does not appear to be a large wave of magnetism set up by the enormous rush of current.

I am not able to explain this, and wish to direct attention to it. The point is one of great interest. From the intense incandescence of the lamp in the primary (and in view of the great suddenness of the effect), there is no doubt that the momentary current is several amperes, instead of a fraction of an ampere. For instance, the ordinary magnetising current in some even of the large transformers used is not more than from 0·1 to 0·2 ampere —insufficient to visibly heat the filament of the 100-volt lamp generally used, the normal current of which is 0·6 ampere. But a sudden rush would often violently incandesce, and sometimes even break, the filament in a manner that could scarcely be exceeded in brilliance if it had been switched direct on the 2,000-volt mains. This current would be sufficient to magnetise the iron of the transformer far above any ordinary working limits, and indeed to complete its saturation, and under ordinary circumstances would of course set up an opposing E.M.F. in the primary of several times 2,000 volts. But apparently the current enters without even setting up a momentary wave of magnetism in excess of the normal, for otherwise the secondary circuit would be affected, and show the result on a lamp placed as described.

What I have just said, applies whether the rush be regarded as a succession of alternate currents (as Dr. Fleming concludes from Exp. 8, p. 682), or a single rush of current in one direction.

Dr. Fleming's explanation is that the sudden rush is due to residual magnetism,—that if the receding wave left the iron with a certain magnetic set, and the succeeding wave came in the same direction, it would not meet with the same opposition as if the following wave were in the opposite direction.

It appeared that was the correct explanation. Using a lamp that was not incandesced by the normal magnetising current, you would get all degrees of incandescence by making rapid contacts; the lamp would be generally black, but you would get all degrees of incandescence up to brilliant light, showing that the current that entered depended upon the part of the wave at which the contact was made. I thought that I

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might make an experiment as a way of settling or illustrating that point. Mr Mordey.

(*s.*) I took a small 1-ampere 1-C.P. lamp (Fig. 3) and put it

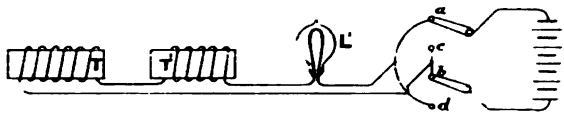

FIG. 3.

in a series with several transformers, and a secondary battery to supply the current, so as to get the waves in either direction at will, and I found that the result quite confirmed the explanation which Dr. Fleming had given. When I made the contact so that the current flowed in the same direction as the previous current, the lamp took about a second or a second and a half to incandesce; when in the opposite direction, it took four or five seconds to incandesce—showing that the current had to wipe out the previous magnetism in the iron of the transformer in the latter case, and set up its own magnetic field before it could reach the steady stage. [Mr. Kapp: "Continuous current?"] Direct current; no alternator at all.

My experiments have been made somewhat hastily, in order that I might be able to discuss this section of the author's paper this evening, but the results have been carefully noted. It will, however, be seen that investigation is needed to clear up one or two curious points. So far as the results go, however, they appear to point to the following conclusions:—

1. The "sudden rush" is due to residual magnetism.
2. It appears on the magnetising circuit, whether primary or secondary.
3. In some transformers it is more frequent and violent than in others, probably because of the quality of iron used.
4. It consists of large waves of current, which do not appear to set up unusually high degrees of magnetisation.
5. It is prevented by slow breaking or slow making, under ordinary circumstances.
6. It is less noticeable at high than at low periodicities.

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Mr. Mordey.

7. At 100 ∩∪ with good iron it is almost inappreciable, and no precautions are needed to prevent it.
8. Even at low periodicities, if good iron is used, it is avoided by simply using a rather slow-acting switch.
9. It is not an effect accompanying increase of load on a working transformer.
10. A closed secondary circuit does not prevent it.

When working at this, I remembered that I had read a long while ago a paper by Lord Elphinstone and Mr. Vincent that bore on this point. I find it in the *Telegraphic Journal*, volume viii., April the 15th, 1878. A number of experiments were described before the Royal Society showing the great retentiveness of electro-magnets. When the current was taken off, the authors were able to load up electro-magnets in a very curious way. In one case an electro-magnet-weighing only 5 oz., when magnetised by a single bichromate cell, would hold up 1 lb. They took the current off, and went on loading it up for 24 hours, when it carried 50 oz.

Mr. Crompton.

Mr. R. E. CROMPTON: I believe that most of those who are in the room will admit that I have for some years past held strongly to the opinion that the distribution of energy by alternating currents has yet to be improved to a very considerable extent before it can seriously compete with the older and simpler methods of distribution by direct currents. In the year 1888 I read a paper on this subject, and I think it would astonish some of the gentlemen who discussed that paper if they were now to turn up and read that discussion. I then gave a figure for the consumption of fuel which at that time was laughed at by the supporters of alternating-current distribution, but which has not even yet been reached by the most improved and economical alternating supply stations now in existence. On the other hand, I then gave a figure for direct distribution which has already been surpassed in several of the London stations, so that I wish to make it clear that real progress in electrical distribution during the last five or six years has been in direct-current distribution, and that up to the date of the present paper alternating distribution has almost stood still—that is to say, when regarded from

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the point of view of economy. The causes of the difference in the rate of progress between the two systems no doubt is chiefly due to the great difficulty found in ascertaining where the losses in alternating distribution really lie. Those working direct systems have been able from the very first to so locate and measure their losses that they have been able to introduce corrections into their designs which have resulted in the present increased economies; but with the alternating system the engineers in charge of the stations are still so completely in the dark that the only guide they have as to the real output of their station is that taken from the readings of the consumers' meters, and as these are only taken at long intervals of time, it increases the difficulty. Looking at the matter from this point of view, the author's paper is an exceedingly valuable addition to the literature of alternating supply, as it does give us some hope that in the future these losses can be better located, and that one cause of loss—viz., that in the transformers—will be greatly diminished, especially at the times of light loads; but it would be a great mistake if the high efficiencies of certain transformers given in this paper were to lead the world to suppose that these improvements in transformer design will make any great reduction in the heavy losses which now exist. The best way of bringing the extent of these losses before you is to take a figure representing what would be the true economic efficiency of the best existing generating and distributing plants when worked under the best conditions, and independently of load-factor—that is to say, when worked continuously at full load. We may suppose that under these conditions both classes of plant may attain the same efficiency. Now we know that with the best existing direct-current plant it is quite possible to generate and distribute to the users at a consumption of 3·7 lbs., say 37-10ths of a lb., of coal per Board of Trade unit delivered at the lamps; and it is quite possible that the best alternating plant could do as well under the same conditions. Let us call this figure 37. Now we find that whereas the best figure obtained in practice up to the present with direct-current plant is 64, leaving only a difference between 37 and 64 as the loss due to bad load-factor,

Mr. Crompton

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Crompton. and which loss will probably still be considerably diminished by improved design, methods of working, and better organisation; in the case of alternating machinery the difference is as between 37 and 160, leaving 123-10ths, or 12⅓ lbs., of coal per unit as the amount wasted, and which has yet to be dealt with. We know that a portion of this loss lies still in the transformers, and is probably due to the low load-factor conditions under which transformers are necessarily worked. Now, although Dr. Fleming shows in his paper that some modern transformers worked at one-tenth of their load show fair efficiencies, yet even under the improved conditions of placing them in transformer sub-stations it is doubtful whether the load-factor will often be as high as one-tenth, so that these high efficiencies will not be reached in practice. As a matter of fact, the load-factor at a transformer sub-station will occupy a position lying between that of the load-factor at the central station itself, and that of a private house—that is to say, that it would lie between 14 and 5. The efficiency of the best transformer described in the paper and calculated from the figures furnished in the paper would not exceed 54 per cent. if worked with the average load-factor of a London private house, but its efficiency may be raised to 80 per cent. if worked on a load-factor of 7 in a transformer sub-station.

The other point to which the author has called attention—namely, the great rush of current which he has noticed when transformers are first switched on—may have a very serious effect, as tending to break down the insulation of these transformers, and I shall be glad to hear what the author proposes to do to prevent this.

Mr. Mordey. Mr. MORDEY: If I am in order, I wish to point out that the object of my remarks was to show that with proper precaution this effect is not dangerous, nor the cause of danger, in any case. Mr. Crompton may be quite comfortable in his mind as to the breakdown of transformers from that cause.

Dr. Sumpner. Dr. SUMPNER: In the first place, I wish to allude to two methods of testing transformers which Dr. Fleming mentions in the first page of his paper. These methods are due to Dr. Hopkinson and myself, and were described in the same number of the *Electrician* some time since. They are classed together here

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as if they were much alike and were subject to the same disadvantages. But although alike in principle they are very different in practice. There is all the difference between them that exists between a method which can only be used in a laboratory, and another suitable for use in works. Dr. Hopkinson's method requires very special apparatus only to be found in a first-rate laboratory. The method I adopted merely needs an ordinary wattmeter, and measures directly the quantity which is most wanted, namely, the losses occurring in the transformer. It is an economical method, because no more power need be used than that which is actually wasted in the transformers themselves; and it is not only economical in power, but also in plant. Mr. Mordey, who has used the method in testing transformers at the Brush Works, has found it possible to test 50-H.P. transformers with a dynamo giving out not more than 5 H.P. No doubt, as Dr. Fleming mentions, it can only be applied when there are two transformers available. But that is no disadvantage in works. The accurate laboratory testing of transformers is not a matter which will always need attention. What need exists has, in fact, been to a great extent removed by this exhaustive paper of Dr. Fleming's. But it will always be desirable in works to subject transformers which have been just made to some simple thorough test to ascertain anything which may be faulty about their construction. The method I have mentioned is specially suitable for this purpose. By its means two transformers may be run at full load for twelve hours without wasting more energy than would otherwise be needed to test one of those transformers at full load for a single hour. When it is necessary to test a single transformer, a simpler and more accurate method than any of those mentioned by Dr. Fleming is one in which the iron and copper losses occurring in the transformer are separately determined by means of a single wattmeter. The wattmeter should first of all be used to test the iron losses in the transformer—that is to say, the losses on open circuit—when energy is supplied to the *thick*-wire coil at the proper voltage. Next, the thick-wire coils of the transformer should be short-circuited, and the same wattmeter used to test the copper losses, by passing the current

Dr. Sumpner.

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Dr. Sumpner.

into the *fine*-wire coil and measuring the power read by the wattmeter when the full-load currents are passing through the transformer. The reading of the wattmeter in the second case measures the copper losses in the transformer. With all due deference to Mr. Mordey, the iron losses may, I think, be taken as constant at all loads, and the total losses occurring in the transformer are simply the sum of the two wattmeter readings, and from that sum the efficiency of the transformer can of course be easily found.

The next point I wish to refer to is connected with wattmeters. I have always held the opinion that a good wattmeter is not only the simplest, but also the most accurate, means of measuring power supplied by alternating currents. A wattmeter made with proper care will, I have always believed, measure power quite sufficiently accurately for all practical purposes. Of course this has always been Mr. Swinburne's view, and we were all much surprised, no doubt, to learn that one particular wattmeter made by him read so wrongly. If the explanation is that which Dr. Fleming gives in his paper, the result is of great importance, as it forms a great argument against the use of wattmeters. If a maker, who believes that the inductance in the wattmeter can be reduced till it is negligible, only succeeds in making an instrument which has a considerable inductance, it means that wattmeters are unreliable instruments. If, however, the explanation of the errors in this particular wattmeter is that which Mr. Swinburne has suggested—namely, that it is due to eddy-currents in pieces of metal near the coils—the matter is of very little importance, since nothing is easier than to remove the pieces of metal and thereby render the instrument exact. I have accordingly thought it worth while to examine this question carefully. I have come to the conclusion that it is impossible to account for the errors in the instrument in the way suggested by Dr. Fleming without assuming a far larger capacity in the coils than they can possibly possess. On the other hand, the hypothesis of eddy-currents in pieces of metal near the coils renders all the observations consistent with each other. I have shown, in fact—I do not wish to go into it now, as it is late, and it would take

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too long—that, taking two results which Dr. Fleming has given, it is possible to predict the other seven or eight readings of the wattmeter; and I suggest that this explanation may be incorporated with the printed version of my remarks. I will only add now that Dr. Fleming himself was the first to point out, about six years ago, the principle on which the explanation rests. At that time Dr. Fleming showed that when a piece of metal was suspended near a coil through which an alternating current of electricity was passing, the eddy-currents induced in the metal plate were such as to cause a force between the coil and the suspended metal plate. The explanation given by Dr. Fleming was that the eddy-currents lagged behind the electro-motive force induced in the plate by the alternating currents in the coil near it. If Dr. Fleming had not apparently forgotten all about this lag, he would perhaps have been able to see the reason why the Swinburne wattmeter reads too high when it is measuring power supplied to condensers, and too low when measuring power supplied to choking coils. However, I have gone fully into the matter, and my theory of it may be printed afterwards. A more convincing proof that my view is correct is the fact that I have put the matter to an experimental test. I have taken an ordinary Ganz wattmeter, made like a Siemens dynamometer, and used it to test the power given to both condensers and choking coils, and tested whether bringing up a plate of metal near the coil altered the deflection of the wattmeter. I found that the deflection of the wattmeter was increased when it was used to measure power supplied to condensers, and was diminished when used to measure power supplied to choking coils. For instance, in one case of testing condensers the true power was 35 watts; when I brought two plates of metal, one on each side of the coil, up to the instrument, the reading, instead of being 35, was 146 watts—that is to say, four times the proper amount of power. In this experiment the voltage was 102, the current 13 amperes, and the frequency 200. When I was testing a choking coil, the primary coil of a "hedgehog" transformer, the reading was altered from 105 to 73—that is to say, it was diminished to 70 per cent. of its proper value. These tests, I think, prove the matter conclusively.

Dr. Sumpner.

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Dr. Sumpner.

Dr. Fleming has, I am glad to see, laid stress on the importance of the quantity which he calls the "power-factor." I have felt for a long time that this factor is a much more important thing than the inductance of the circuit. About 18 months ago, when Dr. Fleming read his paper on the Ferranti mains before this Institution, I wrote an article for the *Electrician* the chief object of which was to point out the significance of this quantity, and I gave its value in several important cases.

The following communication contains my theory of the errors in the wattmeter alluded to by Dr. Fleming:—

Suppose the current in the volt coil of the wattmeter at any moment, t, is $v \sin p\,t$, and that in the current coil $A \sin (p\,t - \phi)$, where ϕ is the lag due to the inductance of the circuit, being positive for choking coils and negative for condensers: the electro-motive force induced in a plate of metal near the current coil will be proportional to the rate of *decrease* of the current—

$$A \sin (p\,t - \phi),$$

or to

$$-\cos (p\,t - \phi);$$

and the currents induced in the metal will be proportional to

$$-\cos (p\,t - \phi - \phi'),$$

where ϕ' is an angle of lag owing to the time constant of the currents in the metal. The action on the pressure coil of the wattmeter will be that due to a current—

$$A \sin (p\,t - \phi) - A' \cos (p\,t - \phi - \phi'),$$

where A' is some constant depending on the position of the metal plate.

The deflection of the wattmeter will be determined by the mean product of this current and the current in the pressure coil; or

$$A\,v \cos \phi - A'\,v \sin (\phi' + \phi).$$

If there were no metal plate, the deflection would be proportional to

$$A\,v \cos \phi.$$

So that the watts as read by the wattmeter will bear to the true watts the ratio,

$$1 - \frac{A'}{A} \frac{\sin (\phi + \phi')}{\cos \phi}.$$

Since ϕ' and the ratio of A' to A are constant, this factor takes

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the form,

$$1 - a - \beta \tan \phi,$$

Dr. Sumpner.

where a and β are constants, and $\cos \phi$ is equal to the "power-"factor" of the circuit with which the wattmeter is connected.

Dr. Fleming has given sufficient data in his paper for testing the accuracy of this formula, and I find that by taking $a = 0{\cdot}2$ and $\beta = 0{\cdot}0267$ all the cases mentioned in which the Swinburne wattmeter gave erroneous readings can be satisfactorily accounted for. The following table gives the results:—

Circuit Tested.	Power-Factor, Cos ϕ.	Tan ϕ.	Ratio of Watts read by Swinburne Wattmeter to True Watts.	
			Experimental.	Calculated.
"Hedgehog" transformer	0·063	15·61	0·34	0·39
Condensers	0·022	−45·1	1·93	2·00
Condensers & "hedgehog" transformer in parallel ...	0·17	5·77	0·67	0·65
3 small "hedgehog" transformers ...	0·096	10·38	0·65	0·53
Mordey transformer ..	0·77	0·83	0·76	0·78
Th.-Houston „ ...	0·54	1·56	0·77	0·76
Kapp „ ..	0·61	1·29	0·74	0·77
Ferranti „ ...	0·81	0·72	0·78	0·78
„ „ ..	0·85	0·62	0·89	0·79

The power-factors given in the second column are taken from Dr. Fleming's paper, or calculated from the numbers there given. The experimental ratios given in the fourth column are obtained by dividing the readings of the Swinburne wattmeter by the corresponding readings of the dynamometer wattmeter, the numbers in every case being taken from Dr. Fleming's paper.

In connection with Dr. Fleming's interesting observations about the sudden rush of current which sometimes occurs at the moment of switching on the primary circuit of transformers, I should

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Dr. Crompton. like to refer to a paper of mine printed in the *Philosophical Magazine* for June, 1888, and partially reproduced in Dr. Fleming's book "The Alternate-Current Transformer," vol. i., pp. 263-268. I there show that when an alternating electro-motive force is suddenly applied to a circuit possessing self-induction, the current which passes may, for the first few alternations, far exceed the final value to which the current settles down; and actual curves are drawn for a Kapp transformer under special circumstances. When the data representing ordinary working conditions of transformers are taken into account it will be found that this sudden rush of current may be of enormous magnitude if the impressed electro-motive force of the mains happens to be *small* compared with its maximum value at the moment the primary circuit is switched on. There is no need to assume, as Dr. Fleming does, that the effect is due to the iron of the transformer being left magnetised in a manner depending on the conditions under which the transformer was previously disconnected from the circuit. It must be remembered that the iron of transformers is never worked at a high induction density, and that, even if the core were left magnetised as much as possible when the previous current was switched off, it would still be capable of receiving a large increase of magnetism before reaching saturation.

Mr. Raworth. Mr. RAWORTH: May I say one word? I should be very sorry on this occasion if the discussion should be turned away from the consideration of Dr. Fleming's paper, which I understand was on transformers and methods of testing them. Mr. Crompton has been enjoying himself to-night. We know he is never quite happy unless he is telling us he is perfectly sound in his views on low tension, and that those who advocate high tension are totally and completely wrong. Now, Sir, if it were not that Mr. Crompton takes so much trouble to smash up the enemy, we might get the notion that that enemy was of no importance; but seeing that he never misses a chance of having a shot at the opposition, we must come to the conclusion that it is not so despicable as he tries to make out. I hope the discussion will not be thrown off the direct issue by Mr. Crompton throwing down

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the red rag. We shall be happy at a suitable time to pick it up, and, by the aid of the scientific gentlemen who have come forward to help us to elucidate this question, to give a good account of the alternate-current transformer system. He says we do not know our primary losses nor our secondary losses; but I can assure him that, although we may not always know them exactly, we do know the limits of them. In the case of Newcastle-on-Tyne, where they have to supply a very large district with alternating currents, they get paid for a certain number of units which are passed through the meters; and the coal, or a certain black substance resembling coal, which is used to produce the result amounts to 20 lbs. per unit. At Liverpool, where a very large district has the advantage of the low-tension system, if it is an advantage,—also the advantage of accumulators, if they are an advantage,—they also use 20 lbs. of this black stuff which is presumably coal, to produce one unit. There is no difficulty in comparing these results. It is a very happy circumstance that in both places they are using this North Country slack worth 6s. or 7s. a ton, and that they also both use Lancashire boilers with automatic stokers, so that we have "low tension" fairly pitted against "high tension," with absolutely equal results. Mr. Raworth.

Mr. ALBRIGHT: I have only one word to say with regard to what Mr. Raworth has just said. It must be a matter of common knowledge to anybody who knows what they do at Liverpool that they practically do not use batteries. Mr. Albright.

The PRESIDENT: It is evident that the remarks on the paper before us are not nearly exhausted, and I therefore adjourn the discussion until January next.

I have to announce the result of the ballot for Council and Officers as follows:—

President:

W. H. PREECE, F.R.S., M. Inst. C.E.

Vice-Presidents:

ALEXANDER SIEMENS, M. Inst. C.E.	Sir DAVID SALOMONS, Bart., M.A.
R. E. CROMPTON, M. Inst. C.E.	Sir HENRY MANCE, C.I.E., M. Inst. C.E.

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Ordinary Members of Council:

Major G. W. Addison, R.E.
Frank Bailey.
Professor J. A. Fleming, M.A., D.Sc., F.R.S.
Professor George Forbes, M.A., F.R.S., F.R.S.E.
W. T. Goolden, M.A.
Dr. Edward Hopkinson, M.A.
Colonel R. Raynsford Jackson.
Gisbert Kapp, M. Inst. C.E.
Professor A. B. W. Kennedy, F.R.S., M. Inst. C.E.
Professor J. Perry, D.Sc., F.R.S.
W. M. Mordey.
James Swinburne.

Associate Members of Council:

W. A. Chamen.
C. P. Sparks.
Arthur Wright.

Honorary Auditors:

Frederick C. Danvers.
Augustus Stroh.

Honorary Treasurer:

Sir David Salomons, Bart., M.A., Vice-President.

Honorary Solicitors:

Messrs. Wilson, Bristows, & Carpmael, 1, Copthall Buildings, E.C.

The President: The scrutineers also report the following candidate as duly elected:—

Associate:

Thomas Joseph Gough.

The meeting then adjourned.

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ABSTRACTS.

J. VIOLLE—THE RADIATION OF INCANDESCENT BODIES.

(*Journal de Physique*, *July*, 1892, *p.* 299.)

As the temperature of a body rises the total radiation increases, and at the same time the nature of the radiation changes; radiation of shorter and shorter wave-length coming into action. The lowest temperature at which radiation begins to which the human eye is sensitive is about 350°; this point varies a little with different observers, and varies also with different bodies. The author has been trying for some time to find the law of increase of radiation with temperature for rays of particular wave-lengths, and gives the following results. The unit taken is the radiation at 1,000°. The last column are the results obtained by Le Chatelier, working with red glass instead of true monochromatic light.

Temperature.	$\lambda = 656$.	$\lambda = 589{\cdot}2$.	Red Glass.
775°	0·045	0·027	0·04
954°	0·55	0·53	0·55
1,045°	1·80	1·93	1·68
1,500°	85	116	98
1,775°	280	430	450

The discrepancy M. Violle attributes to the light which the red glass allows to pass becoming less and less monochromatic as the radiating body becomes hotter, and believes that his own results with true monochromatic light obtained with the spectro-photometer are the more accurate. He deduces from them the fact that radiation of any particular wave-length does not keep on increasing indefinitely as the temperature rises. Confirming this, he quotes some previous experiments with the thermopile on the ultra-red spectrum at a distance as far below visible red as blue is above it; the radiation obtained there from a body at 10, 12, 14, and 16 hundred degrees being 1, 1·85, 2·47, and 2·91 respectively, showing that the intensity of radiation of a given wave-length does not increase very rapidly with temperature after a certain limit.

Captain DE PLACE—THE SCHISÉOPHONE.

(*Comptes Rendus*, *Vol.* 115, *No.* 17, *p.* 582.)

This is a microphonic apparatus for testing metal for flaws. A sort of Crossley microphone consisting of rods of carbon is arranged on a stand. The rods form a triangle or square, and surround, but apparently do not touch, a steel rod. This rod is caused by any suitable means to keep constantly tapping on the piece of metal to

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be tested. The microphone is joined up in circuit with a battery, and a coil in an adjacent room. The coil is fixed at one end of a long graduated rod, and another coil is connected to a telephone and moved away along the rod till the sound in the telephone due to the induction of the fixed coil on the movable one is almost *nil*. Keeping the two coils in this position, the piece of metal (rail, armour plate, &c.) is then slowly moved past the instrument by an assistant, the rod still tapping on it. Any flaw in the metal is at once detected by the sound in the telephone increasing. In testing pieces of metal of varying shape and thickness a table is drawn up, from tests on a sample known to be good, of the proper distance between the coils at which the sound should nearly vanish when the tapper is on different parts of the test piece, and deviations from these distances when testing a similar piece denote the presence of flaws. The taps should not be more rapid than two in three seconds, and the tapper is best worked pneumatically by a ball and tube, like an instantaneous shutter.

H. ABRAHAM—NEW DETERMINATION OF "v."

(*Journal de Physique, September*, 1892, *p.* 361.)

The capacity in electrostatic units of a certain air condenser was calculated from its linear dimensions, which were measured with great accuracy. Its capacity in electro-magnetic measure was determined experimentally; the ratio between the two values for the capacity being "v." The second value was obtained as follows:—

By means of a rapidly revolving commutator, driven at a known and perfectly constant speed, the condenser was alternately joined to a charging battery and discharged through two coils of a Thomson galvanometer. The effect of the interrupted current thus obtained was exactly counteracted by passing through the other two coils of the same galvanometer a small known and adjustable current obtained (potentiometer-wise) from the charging battery itself. The constants of the two coils were accurately known, and thus sufficient data obtained to arrive at the capacity. The commutator was driven by a 1-H.P. Gramme, apparently at about 40 per second. The speed was known to 1 in 2,000, and all other values to well within this limit. The details of the optical measurement, both of speed and also of the distance between the plates of the air condenser, are illustrated. The measurements were repeated on five different days, three times each day; the apparatus being more or less taken to pieces between each set so as to avoid constant sources of error. The fifth day another condenser was used, and the results were still concordant. Before each test the commutator was run with the condenser disconnected and its own capacity and that of the wires determined. The results give—

$$v = 2{\cdot}992 \times 10^{10} \pm 0{\cdot}003.$$

H. ABRAHAM—THE OUTPUT OF AN INFLUENCE MACHINE.

(*Journal de Physique, October*, 1892, *p.* 409.)

This is carried out much on the same lines as the previous investigation. An influence machine was made as closely as possible the theoretical machine. The

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inducing plate was charged by a battery, and the machine discharged through a differential galvanometer as above, the effect being neutralised in the same way. The plate was rotated at constant speed by a Gramme machine. The results show (1st) that with speed constant the output is directly proportional to excitation—the author believes, to 1 in 10,000; (2nd) with excitation constant the output is directly proportional to speed (to 1 in 1,000).

By way of a check the theoretical output was determined from the dimensions of the machine, and thence the value of "v" calculated. It came out 2·98 to 3·01 $\times 10^{10}$, which shows that the results are not very far wrong. The difference is due to uncertainty as to which of two corrections to allow for the inside edges of the plates.

F. OSMOND—THE RESISTANCE OF IRONS AND STEELS.

(*Lumière Electrique*, *Vol.* 46, *No.* 41, *p.* 93.)

The specific resistance varies very much with composition, and also with hardness. Pure electrolytic iron has a specific resistance r_0 (resistance between the faces of a 1-cm. cube at zero C., in microhms) of 9·4; iron wires with various percentages of carbon, from 10 to 16·7; piano wire, 11·8; steel with 3 per cent. silicon, 38; steel with 25 per cent. nickel (magnetic), 45; steel with 25 per cent nickel (non-magnetic), 76; Hadfield manganese steel (13 per cent. Mn), 81. The temperature coefficient (between 10° to 35°) falls in about a parabolic curve with increase of specific resistance; soft iron having r = 9·4 and T.C. = 0·54 per cent.; hard cast steel, r_0 = 80, T.C. = 0·13 per cent. (cf. Barus and Strouhat, U.S. Survey, 1885). Le Chatelier has also given curves of resistance with temperature, showing that the T.C. of iron alloys is a very variable quantity at high temperatures. Two samples of steel rose steadily about 0·6 per cent. per degree to 710°, then the curve of resistance and temperature suddenly changed, and the resistance rose 1·2 per cent. per degree till 800°, after which it rose only 0·2 per cent. per degree. If suddenly cooled, it retains in part the high resistance it had reached at the high temperature. The curves of several other irons and steels are given, which show similar sudden changes of T.C. Tempering, even when very slight, lowers the resistance. On one sample heating to 60° for three hours (the effect is gradual) made a difference of over 1 per cent. in r_0, while tempering in melted lead for half an hour reduced r_0 by 50 per cent. M. Osmond has found no effect on r_0 of very soft steel by drawing (though Cowleson has shown that when a hard-drawn iron wire is annealed at a dull red, r_0 diminishes 4 per cent.). If this is true, it is of practical value, since we may draw down wire, and thereby increase its strength, without increasing its r_0; and of theoretical interest, in that its electrical resistance will be its only property that is not influenced in the same direction by drawing and by annealing.

E. VAN AUBEL—THE INFLUENCE OF MAGNETISM ON THE LENGTH OF A BAR OF BISMUTH.

(*Journal de Physique*, *Oct.*, 1892, *p.* 424.)

The author took special care to have his apparatus as rigid as possible. The bar of bismuth was about 30 cm. long, and was supported on a massive copper

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casting built into a cellar wall. The bismuth was surrounded by a magnetising coil, and its upper end acted on a long horizontal lever, the motions of the bar being multiplied 18 times at the end of the lever, where a small plate of plane glass was attached horizontally. A total reflection prism was placed close under the glass, one side being horizontal and the other vertical. Monochromatic sodium light was then thrown on the hypotenuse of the prism, whence it was reflected on to the glass plate, thence back to the hypotenuse and into a microscope, through which the interference fringes in the air space between plate and prism were observed. By this means a variation of ·000 000 008 in the total length could have been determined. No movement whatever could, however, be detected when the magnetising current was switched on; consequently, if there is any effect, it must be taken as less than this amount. The magnetising force (mean intensity of the field) of the coil was measured by the rotation of the plane of polarisation of a ray of polarised light, and found to be 1,039 C.G.S. units.

C. FELDMANN—LIGHT AND POWER IN GLOW LAMPS.

(*Elektrotechnische Zeitschrift*, *Vol.* 13, *No.* 50, *p.* 667.)

The results in tables and curves are given of a set of experiments carried out at the Municipal Lighting Station at Cologne with alternating current. Three groups of six 16-C.P. lamps were run at voltages such that the efficiency at starting was 1·78, 2·74, and 3·32 watts per C.P. for groups A, B, and C respectively; and curves are given of both watts per C.P. and percentage of original C.P. during about 1,000 hours. The latter curve drops very quickly at first and slower afterwards for A and B, and at 1,000 hours reached 18 and 24 per cent. The curve of C fell in a straight line to 52 per cent. The "Watts per C.P." curve of A and B rose much more rapidly than that of C, and cut the latter close together at about 150 hours. At 1,000 hours A was 7, B 6·8, and C 5·5 watts per C.P. With these lamps (Kohtinski), at any rate, it is very bad economy to over-run them. The following (condensed) table gives the averaged results obtained from more than 500 lamps of 28 makers and 49 different types. The first column gives the watts per candle at starting, the voltage being kept constant throughout:—

0 Hours.	100 Hours.	200 Hours.	400 Hours.	700 Hours.	1,000 Hours.	1,200 Hours.
2·4	2·8	3·3	4·2	5·2	5·7	5·8
2·9	3·0	3·3	3·8	4·4	5·3	6·3
3·3	3·4	3·5	3·7	4·2	5·0	5·6
3·8	4·1	4·3	4·7	5·6	6·3	6·7
4·5	4·7	4·9	5·4	6·4	7·0	7·1

The 4·5-watt lamps were a bad make. The best lamps, both for constancy of C.P. and efficiency, were those designed for about 3·4 watts. The experiments incidentally showed that the efficiency was the same for alternating and direct current.

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A. ABT—PERMANENT MAGNETISM OF NICKEL AND STEEL.

(*Centralzeitung für Opt. und Mech.*, 11, *No.* 20, *p.* 229.)

Bars of nickel, glass-hard steel, and straw-colour tempered steel, all of roughly the same dimensions, were magnetised in a spiral, and their permanent magnetism measured by magnetometer. For low magnetising power the P.M. of the nickel was 4·8 and 4·3 times greater than that of the hard and tempered steel respectively. For high powers the steels were three and four times stronger than the nickel. Nickel showed signs of saturation.

R. LOHNSTEIN—THE PASSAGE OF WEAK CURRENTS THROUGH ELECTROLYTES.

(*Wiedemann's Annalen, Vol.* 47, *No.* 10, *p.* 299; *Lumière Electrique, Vol.* 46, *Nos.* 45, 46, *pp.* 288, 338.)

The measurements were principally made with zinc plates in a zinc-sulphate solution. A ballistic galvanometer of about 1·4 ω resistance was connected to the two zinc plates; the needle was then deflected by a magnet, allowed to oscillate freely, and the log. dec. noted. In swinging, it set up a slow, small alternating current through itself and the electrolyte, thus increasing the damping of the needle. Having taken the log. dec. on open circuit, the total resistance in circuit can be calculated (the theory is given at full length) from the fact that log. dec. $= a + b/R$, a being a constant depending on the air damping, and b on current damping. R is the total resistance in circuit. The log. dec. on open circuit, R being $= \infty$, gives the a constant: this varies a little with temperature, &c.; in the case mentioned $a =$ 0·011 to 0·015, and b about 0·368. The (maximum) current was of the order of 2 microamperes. The log. dec., closed through the liquid, was generally of the order of 0·06. Measurements by bridge and telephone generally confirmed the results. With polished zinc there was great surface resistance, and silence could not be got in the telephone-bridge method. By depositing pure zinc on the electrodes more reasonable results were got; with a very rough deposit, measured immediately, a very low and apparently true ohmic resistance was measured which agreed with the bridge method, almost perfect silence being now obtainable; after standing some time, the resistance became much larger. A fine-grained close deposit gave much higher values. Freshly amalgamated zincs also gave true resistance; but, after standing, the spurious, or surface, resistance appeared. Experiments with copper, and also silver, gave similar results; but the true resistance could only be got by amalgamation, not by deposition. The author thinks it probable that the spurious, or surface, resistance may be due to a condensation of the liquid at the surface of the electrode.

A. OBERBECK—THE RESISTANCE OF ALLOTROPIC SILVER.

(*Wied. Ann., Vol.* 47, *No.* 11, *p.* 353; *Lum. El., Vol.* 45, *No.* 38, *p.* 343.)

The author was trying to make high resistances by depositing thin sheets of silver on glass, but found them unreliable, the original resistance being far higher

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than that calculated from the thickness. A film the first day measured 683 ω; on the 4th, 203rd, and 641st days, 395 ω, 60·6 ω, and 58·5 ω respectively. The films remained perfectly unchanged in reflecting power and colour by transmitted light. The author's silver was therefore very much nearer the normal state than that mentioned in Mr. Carey Lea's researches on allotropic silver (*Phil. Mag.*, 1890–91). Working with the various coloured forms of Mr. Lea, the conductivity was found to increase after warming (110° and 160°), generally some thousand-fold; and in some cases the resistance fell from infinity to a few hundred ohms or less. Two varieties, however, were always infinity. Exposure to sunlight decreased the resistance continuously till, in two hours, it reached about one-tenth its former value. Pieces were cut off from time to time, half of each being in the shade. The effect was cumulative and permanent, the decrease of the sunned piece over the other increasing with the time of exposure, and the ratio between each pair remaining about the same after three months. The effects of various acids, and also of exposure to currents of damp and dry air, on the resistance are also given at length. The author concludes that these resistance effects strengthen the belief in a true allotropic form of silver as held by Mr. Lea.

ANON.—JOINING METAL TO GLASS.

(*Beiblätter, Vol.* 16, *No.* 7, *p.* 458; *Centralzeitung für Opt. und Mech.*, 13, *p.* 77, 1892.)

Five per cent. of melted copper is added to 95 per cent. melted tin, and the alloy granulated and re-melted. It holds excellently to clean glass, and has nearly the same coefficient of expansion. Melting point, 360° C. A small addition of lead makes it softer and more fusible; zinc has the opposite effect.

ANON.—DESCARTES ON THE MAGNETIC CIRCUIT.

(*Lumière Electrique, Vol.* 45, *No.* 27, *p.* 45.)

Descartes in 1656 gives a theory of magnetism which is curiously suggestive of the modern theory of lines of force and the magnetic circuit. He considers that all magnets are due to loadstones, &c., having been traversed by a "subtle fluid" which is constantly flowing out at the north pole of the earth, curving round in the air, and re-entering at the south pole, whence it returns to the north pole again by "pores." These "pores," or tubes, which appear to exist in all substances, are lined with what may be called bristles, which stick out in all directions, but may, by the act of magnetisation, be all set one way and allow the magnetic fluid to pass freely. The set is permanent in steel, and temporary in soft iron. The flow of current is much more rapid and easier in iron than in air, and a magnet does not lose strength by magnetising a piece of iron in its neighbourhood, but the contrary, as the iron offers an easier path than the air and more fluid flows.

— CARDANI - ON THE HEATING OF BARE WIRES.

(*Lumière Electrique, Vol.* 46, *No.* 45, *p.* 283.)

The wires experimented on were fixed between two supports and their rate of

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vibration determined; they were then traversed by a current and the rate of vibration again determined: from this their temperature was deduced. The latter was found to follow fairly regular laws, which are discussed, and the following results are given. The current, C, required to raise a wire of d millimetres diameter is $C = a\, d^{\frac{3}{2}}$, where

$$a = \pi \sqrt{\frac{21}{5} \times \frac{10^3}{4} \times \frac{K\,\theta}{\rho_0 (1 + \beta\,\theta)}}.$$

K = coefficient of radiation + convection, θ = rise of temperature above air, β = temperature coefficient of the wire, ρ_0 = specific resistance at the temperature of the air, and 21/5 joules = 1 calorie.

Values of a for copper:—

Diameter.	5°.	10°.	20°.	30°.	40°.	50°.
0·1	9·79	13·83	19·52	23·86	27·51	30·68
0·3	6·93	9·77	13·78	16·70	19·17	21·24
0·5	5·82	8·19	11·49	13·96	16·00	17·76
0.8	4·79	6·74	9·45	11·48	13·14	14·58
1·0	4·47	6·23	8·73	10·60	12·14	13·46
2·0	3·47	4·88	6·83	8·28	9·47	10·49
3·0	3·05	4·29	6·00	7·27	8·31	9·20

E. BUELS—BRONZE WIRES FOR TELEGRAPH PURPOSES.

(*Journal Telegraphique*, *Vol.* 16, *No.* 9, *p.* 229.)

Bronze wires were first brought in for telephone work, and were found a great improvement. When the author first tried them for telegraph work on a long Belgian line, he found that they were subject to a peculiar fault. He had two parallel lines across Belgium in the main Paris-Berlin route. These both usually worked Hughes satisfactorily. At times, however, he found the two lines affect each other very badly, but then one could be worked Morse and the other Hughes. The interference occurred only in dry weather, and towards the middle of the day—not at all on damp days. After much trouble, the author found that the fault was due to static induction, which was counteracted in iron wires by inductance, but the inductance in the bronze wires being so much less allowed it to show itself. The fault was much decreased by using Van Rysselberghe apparatus, which partially restored to the line the inductance that it lacked.

J. KRÄMER—CALCULATIONS ON THE MAXIMUM SPEED FOR AN ELECTRIC RAILWAY.

(*Elektrotechnische Zeitschrift*, *Vol.* 18, *No.* 43, *p.* 581.)

The theoretical maximum speed of a perfect motor on rails is taken at 500 kilometres (310 miles) per hour. This is on the level, with curves of not less than

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20 kilometres (12 miles) radius, and 12-foot driving wheels. The problem of an electric locomotive and two cars is then investigated at considerable length, and the author appears to have considerable doubts whether the proposed 250 kilometres per hour on the projected Vienna and Buda-Pesth line of Messrs. Ganz is feasible. The author would put the maximum at not more than 200 kilometres (120 miles), if as high.

SIEMENS BROS.—APPARATUS FOR MEASURING PERMEABILITY.

(*Lumière Electrique, Vol.* 40, *No.* 45, *p.* 278.)

The instrument resembles the ordinary Siemens dynamometer. The moving coil is traversed by a constant and known current—say 2 A. The fixed coils are solenoids, traversed by another known current, and into them pieces of the iron to be tested are inserted. The moving coil is brought back to its zero position by a spring on a torsion head, as in the dynamometer, readings proportional to B and H being obtained with and without iron in the solenoid respectively. Absolute measurements may be made by employing a bifilar suspension for the coil instead of a spring, and using the earth's control.

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